Positivism

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POSITIVISM AND THE PHILOSOPHY OF SCIENCE.

The term *positivism* was coined in the second quarter of the 19th century by one of the founders of sociology, Auguste Comte. Comte believed that human reasoning passes through three distinct historical stages: the theological, the metaphysical, and the scientific. In the theological stage, natural and social phenomena are explained by reference to spiritual forces. In the metaphysical stage, `ultimate causes' are sought to explain such phenomena. In the scientific stage, attempts to explain phenomena are abandoned, and scientists seek instead to discover correlations among phenomena. Another important figure in the development *of classical positivism* was the physicist Ernst Mach, who propounded a `fictionalist' view of theories. Scientific theories are useful mnemonic devices, but progress in science only occurs when such useful fictions are replaced by statements which contain only observation terms. Though both Comte and Mach had some influence on the writings of economists (Comte influenced J.S. Mill and Pareto, Mach was mentioned in passing by Samuelson and Machlup), their primary influence was on the ideas of certain 20th-century philosophers of science, the logical positivists.

The major tenets of *logical positivism* were developed in the 1920s by Moritz Schlick, Herbert Feigl, Kurt Gödel, Hans Hahn, Otto Neurath, Friedrich Waismann, Rudolf Carnap and other members of the famous Vienna Circle. Logical positivism

was a radically empiricist philosophical position, and its founders believed it marked a new beginning for philosophical inquiry. The goal of all philosophical analysis was henceforth to be the logical analysis of the knowledge claims of the positive, or empirical, sciences: hence the label, logical positivism.

The first task facing the logical positivists was to define what constitutes a knowledge claim. Their solution was to analyse the logical form of statements. Only statements that are either analytic (e.g. definitions) or synthetic (testable statements of fact) qualify as cognitively significant, or meaningful. All other statements lack cognitive significance: they are meaningless, metaphysical, non-scientific. Analyses that make use of such statements may express emotional stances, or 'general attitudes towards life', or moral valuations, but they do *not* express knowledge claims.

To put their programme into operation, the logical positivists needed an objective criterion of cognitive significance which could be used to distinguish synthetic statements from meaningless ones. One early solution was the principle of verifiability: a synthetic statement has meaning only if it is verifiable. Unfortunately, statements of universal form (e.g. all ravens are black), which are frequently encountered in science, are unverifiable. Other criteria included falsifiability, Ayer's weak verifiability, Carnap's translatability into an empiricist language, and confirmability. None of these was able to resolve the problem conclusively, however. Another dilemma was posed by the presence of theoretical terms in statements made by scientists. Some positivists followed Mach in insisting that they should be eliminated from science, while others argued that such statements should be retained. A final element of the logical positivist programme was an emphasis on the unity of

science, variously defined as meaning that all true sciences share a common method, that the results of all sciences should ultimately be expressible in a common physicalist language, or that the results of the various sciences should be integrated, better to assist the scientific planning of society.

Hahn died in 1934, and Schlick was murdered in 1936 by an insane student. But it was Hitler's rise, to power, and the subsequent flight of intellectuals, that primarily caused the disintegration of the Vienna Circle in the 1930s. Logical positivism was modified and ultimately replaced over the next two decades by a more analytically austere form of positivist thought, *logical empiricism.* Though differences exist in their analyses, philosophers who have contributed to this later tradition include Carnap, Ernest Nagel, Carl Hempel, and Richard Braithwaite.

There were six major tenets of the logical empiricist programme. First, the *unity of science thesis* was narrowed to mean only a unity of scientific methods. The next three had to do with the structure and appraisal of theories. The *hypothetico-deductive model of theory structure* states that all sciences employ theories, which may be represented formally as axiomatic, hypothetico-deductive structures. Such structures have no empirical import until some of their elements (usually the deduced theorems, or predictions of the theories) are given an empirical interpretation via the use of correspondence rules. Not every statement will have an empirical interpretation. Those containing theoretical terms, in particular, will not be interpretable. Are such sentences then meaningless? Not at all; according to the *indirect testability thesis* such sentences gain cognitive significance indirectly when the theories in which they are embedded are confirmed. Finally, concerning the

questions of demarcation and theory assessment, logical empiricists settled on *confirmationism* as their primary criterion of theory appraisal. A theory is scientific if it is testable; test instances confirm or disconfirm the theory; the acceptability of the theory depends on its degree of confirmation. Degree of confirmation is measured by such things as the quantity and precision of favourable test outcomes, the precision of procedures of observation and measurement, the variety of supporting evidence, and whether new test situations support the hypothesis. Additional non-empirical criteria of appraisal (e.g. simplicity, elegance, fruitfulness, generality, extensibility) may also be invoked if theory choice on empirical grounds yields no preferred theory. The last two tenets of logical empiricism concerned the logic of scientific explanation. All explanations in science must be expressible in the form of a deductive argument in which an explanandum, a sentence describing the event to be explained, is logically deduced from an explanans. The explanans contains a group of sentences, some of which express initial conditions, and at least one of which states either a general or a statistical law. The *deductive-nomological* and *inductive-probabilistic covering law models of scientific explanation* take their names, then, from the types of laws (general or statistical) used in the explanations. Additionally, logical empiricists believed in the *symmetry thesis:* explanation and prediction are structurally symmetrical, the only difference between them being one of temporality. In the case of an explanation the phenomenon described in the explanandum has already taken place, whereas in the case of a prediction it has not yet occurred.

As documented in Suppe 1977, logical empiricist ideas (sometimes dubbed "the received view") came under heavy attack in the mid-20th century. The viability of both

the hypothetico-deductive model of theory structure and the indirect testability thesis depended on one's ability to draw a clear distinction between observational terms (terms that refer to observables, to 'brute, atomic facts') and non-observational, theoretical terms. Unfortunately, in many sciences there are degrees of observability, and no hard division can be drawn between theoretical terms that refer to non-observables, and non-theoretical terms that refer to observables. Furthermore, because observation itself is not a neutral activity but requires both data selection and interpretation, it was argued (by critics like Karl Popper and Norwood Hanson) that all observation is theory-dependent. Regarding confirmationism, the failure to solve Hume's problem of induction and a number of paradoxes of confirmation undercut attempts to construct an inductive logic of confirmation. In addition, Popper challenged the desirability of making statements that have a high inductive probability. Finally, many explanations in a variety of sciences could not be reconciled with the two covering law models of scientific explanation.

The influence of positivism within the philosophy of science declined considerably through the 1960s and 1970s. As noted by Hands 2001, its apparent successor has been dubbed the *naturalistic turn,* an approach that, rather than laying out *a priori* criteria for identifying appropriate scientific practice, instead employs the tools of the sciences themselves to investigate scientific practice. There are, of course, many different scientific disciplines from which to draw such tools; some that have been used are cognitive psychology, evolutionary biology, sociology, and economics. Depending on which scientific practice is analyzed, reflexivity issues may appear (e.g., in using economic analysis to explain the development of economics and the behavior of

economists). Other important issues facing the naturalistic turn are choosing among the various tools on offer, and deciding whether the ensuing analysis has prescriptive implications in addition to descriptive merits. Another movement that has had less impact in philosophy of science proper, but great influence in a number of sciences including economics, derives from the work of Karl Popper. A critic of inductivism and confirmationism, the father of falsifiability and of critical rationalism, Popper had sufficient insight, foresight, and longevity to influence a number of generations of philosophers of science, among them J. Agassi, W.W. Bartley III, P.K. Feyerabend and Imre Lakatos. Within economics, the work of T.W. Hutchison, Mark Blaug and Lawrence Boland most directly reflect Popper's influence, while that of Wade Hands and Bruce Caldwell reflect a critical reappraisal.

In the 1990s an historical dehomogenization of the writings of the logical positivists of the Vienna Circle began. A rehabilitation of Otto Neurath, whose anti-foundationalism, advocacy of pluralism, and emphasis on scientific practice led many to see him as a precursor of the naturalistic turn, was the most notable result. Some historians and philosophers also praised his willingness to advocate the scientific planning of society and of science, to employ the philosophy of science as a tool in the restructuring of society. For these interpreters, the emergence of a more austere logical empiricism in the 1950s represented not a scientific advance but a retreat to more neutral formalism in response to the ideological pressures of McCarthyism and the Cold War (e.g., Reisch 2005). This interpretation parallels Philip Mirowski's 2002 historical account of the development of formalism in economics during the same period.

POSITIVISM AND ECONOMICS.

There are various ways to describe the influence of positivist thought in economics. If one focuses on the period in which positivist philosophy of science was invoked by economists, the positivist epoch spanned roughly forty years, from the late 1930s to the late 1970s. This is not to say that during this period economists self-consciously adopted the philosophical positions outlined above. As shown in Caldwell 1994, what in fact occurred was that certain economists writing about methodology borrowed, usually somewhat haphazardly, from the language of positivism, while others invoked various positivist positions to defend or to criticize theories and practices in economics.

Four economists from this period whose writings most reflect the influence of positivism are T.W. Hutchison, Fritz Machlup, Paul Samuelson, and Milton Friedman. In the 1938 book, *The Significance and Basic Postulates of Economic Theory,* Hutchison launched an empiricist attack on the pure logic of choice, a doctrine that had been espoused and defended by Lionel Robbins six years earlier in his *The Nature and Significance of Economic Science.* For more than fifty years, Hutchison was to continue to criticize all forms of economics that were based on untestable foundations, his targets ranging from the apriorism of Ludwig von Mises to the elaborate mathematical models of general equilibrium theory. Fritz Machlup offered one response to Hutchison with his 1955 paper, "On the Problem of Verification in Economics," where he invoked the indirect testability thesis to defend the use of theoretical constructs in economics against what he dubbed Hutchison's "ultra-empiricism." In the Introduction of his *Foundations of Economic Analysis,* Paul

Samuelson borrowed from the work of physicist Percy Bridgman when be insisted that economists search for operationally meaningful theorems. The intent of Samuelson's revealed preference approach to demand theory was to place consumer theory on an observational basis. Finally, Milton Friedman's influential 1953 piece, `The Methodology of Positive Economics', contained the famous argument that the realism of the assumptions of a theory is irrelevant; what counts in the assessment of a theory is its relative predictive adequacy and its simplicity. Though Friedman's unique brand of instrumentalist methodology owes more to the American pragmatists than to positivism, his approach came to be viewed as synonymous with positivism through the 1950s and 1960s.

Though economists today rarely invoke positivist philosophy of science in defending their preferred practices, there is plentiful evidence of its continued influence, mostly in terms of what is considered to be "appropriate" or "legitimate" practice, with "positivist" often being equated with "truly scientific." Thus, important areas like game theory and transactions cost analysis initially encountered substantial opposition from mainstream economists because such analyses, though rich in terms of explaining diverse economic phenomena, often did not produce the sort of testable hypotheses demanded by positivist doctrine. (Strangely, during its period of dominance, general equilibrium theory was much less affected by such critiques.) Similarly, the positivist belief in the cumulative development of science tends to render less important both heterodox approaches to the discipline and the study of doctrinal history. Finally, the insistence on defining progress in terms of "the discovery of law-like relationships" or "better predictive ability" has fueled a sustained growth in data collection and in computing

power, the development of new econometric techniques, and a staggering increase in empirical studies. That all this has resulted in at best meager progress (see Backhouse 1997) in establishing robust economic "laws" and in improving forecasting power has typically engendered not a reassessment of the goals, but a redoubling of resources committed to reaching them, with the attendant opportunity costs. It will be interesting to see what the entry on "positivism" in the next edition of the *Palgrave* reveals about its legacy in economics.

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