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POPPER'S METHODOLOGY AND  
ECONOMIC THEORY

by

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## POPPER'S METHODOLOGY AND ECONOMIC THEORY

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Sir Karl Popper, according to Blaug, has had, 'more than any other philosopher of science, an enormous influence on modern economics' (1978, p. 714). Yet economics, Sir John Hicks has concluded, is 'a discipline, not a science' (1983, p. 365), and many would agree with Alfred Eichner that economics 'as a discipline is in a crisis' (1983, p.3). It is nevertheless argued here that Popper's methodological advice, having been largely ignored, has not contributed to scientific weaknesses of economic theory but, if properly understood, could help to remedy them.

### I: PROBLEMS AND THEORIES

For Popper, the starting point of any science is the attempt to solve an interesting empirical problem. Such a problem arises as we try to understand the world better by finding a satisfactory explanation for some group of observable phenomena. An explanation is a set of statements which describe the phenomena and another set, comprising conjectured universal laws and initial conditions, from which the descriptive statements can be deduced. It is satisfactory only if the conjectured laws have not been falsified by rigorous and independent testing, and this requires them to have a variety of testable consequences. Scientific progress means finding explanations

of well-tested laws themselves in terms of more general laws from which they, in turn, can be deduced as, most likely, can be other laws of lower generality conjectured to solve other problems of explanation. It follows that more general laws have a richer content of testable consequences and so can be better tested. But Popper does not believe that science could ever reach ultimate explanations which would halt its further progress (1963, p. 155; 1972, Ch. 5).

Astronomers had long had a major problem of explaining the movements of the planets and, after a century and a half of work by Copernicus, Brahe, Kepler and Galileo, there came the great solution of Newtonian mechanics, rich in terrestrial as well as celestial content. It had a quick and strong influence on European thought. Adam Smith wrote about it, and seems to have used its hypothetico-deductive method in his own work on both economics and ethics (Blaug, 1980, p. 57). James Mill, Ricardo's mentor, believed that political economy had a similar aim of explaining 'the whole of the subject to which it relates' (Hutchison 1978, p. 52). J.B. Say also took Newtonian mechanics as the appropriate model for economics (Heimann, p.111), and Walras praised Gossen as combining 'the glory of Copernicus with that of Newton because of his solution of the social question' (Lekachman, p. 235).

Neither, of course, had much influence on 19th century England, but there, too, the marginal utilitarians' counter-revolution against Ricardian economics did not disturb Smith's archetypal model of 'the simple system of natural liberty'. Nor did Marx, in rejecting that system, have any narrower vision of the problem of economic

explanation. In our century, although there have been many new insights and changes of emphasis, 'mainstream' economics has had the Smithian concern of analysing how, in Hahn's blunt words, 'a society of greedy and self-seeking people, constrained only by the criminal law and the law of property and contract, should be capable of an orderly and coherent disposition of its economic resources' - one, moreover, 'with certain desirable consequences' (1984, p. 111).

Unlike natural sciences, therefore, economics did not begin with piecemeal investigations of limited empirical problems. Ignoring the peculiar isolation and relative stability of the solar system which made its behaviour explicable by rather simple laws, and ignoring, too, the need for controlled experiment in terrestrial uses of Newtonian mechanics, economists leapt to a general theory of competitive markets within whose general framework they tried to explain all economic phenomena of consumption, exchange, production and distribution.

From the beginning, this general theory has had strong normative aspects. Smith used it to discredit the 'commercial system', and Mises to attack socialism. On its basis Pigou erected an 'economics of welfare', and later much use has been made of 'Pareto optimality'. Recently there has been high-level work on 'optimum control' theory.

Normative problems have never been regarded as unimportant by Popper who long ago refuted the view of logical positivists that metaphysical statements are 'meaningless' (1959, s.4). He does, however, exclude them from science by a demarcation criterion of falsifiability,

a logical relation between the universal statement of a theory and the singular statements which describe empirical tests on consequences deduced from it with the help of initial conditions, and against a 'background' of other relevant theories which are not questioned. The logical relation is that the truth of such singular test statements does not transmit to a universal law, but their falsity would be transmitted to it. We can, that is, never prove a scientific theory by empirical tests, but we may be able to disprove it by them.

Metaphysical theories are not empirically falsifiable and so not scientific; and normative statements are, of course, metaphysical. Again Popper recognizes that metaphysical ideas have sometimes provided a scientific research programme by indicating 'the direction of our search, and the kind of explanation that might satisfy us; and it made possible something like an appraisal of the depth of a theory' (1983, pp. 192-93). Examples are the influence of astrology on Kepler, and of Greek atomism on 19th century science. Nor does he think that all metaphysical elements can be eliminated from science; 'they are too closely interwoven with the rest'. Nevertheless he holds that whenever they can be detected in a theory they should be eliminated in order to improve the theory's testability. (1983, pp. 179-80).

There is no doubt that classical economists were influenced by metaphysical ideas - Smith by Hutcheson's views on natural order, Ricardo and Mill by utilitarianism, and Marx by Hegelianism. Utilitarianism, however, did not reach its widest influence on economic theory until the marginal utility school substituted subjective

valuations for objective labour cost as the basic determinant of market phenomena. Later some modifications were made by restricting utility to ordinal as distinct from cardinal aspects, thereby eliminating attempts to aggregate utilities, and then dispensing even with ordinal utility by appeal to 'revealed preferences' for different bundles of goods. These changes have weakened, but not killed, welfare economics as conceived by Pigou.

They may be held to have reduced metaphysical elements from theories, as Popper recommends, but have not done much to make them more testable. To understand this properly, attention must be paid to Popper's important distinction between the falsifiability of a theory and its falsification. (1983, pp. xxii-xxiii). A falsification is a test statement which contradicts a deduction from a theory, together with initial conditions for the deduction and relevant background theories which have been provisionally accepted as true.

But falsification is never unproblematical. One difficulty is that it could relate, not only to the theory which we seek to test, but to the initial conditions and associated background theories, including those which determine measurement of test variables; for all measurement, or indeed observation, is 'theory-impregnated' (1963, p. 62). These would have to be critically checked or scrutinized before deciding that the theory had been falsified. As Popper says: 'we can indeed falsify only systems of theories and .... any attribution of falsity to any particular statement within such a system is highly uncertain' (1983, p. 187). Another difficulty relates to the falsification of deductions from a theory because no conclusive proof can be given for any empirical statement (idem, p. xxii). Test

statements have thus to be themselves tested by reproducible and intersubjective experiments or observations, although no number of these can lead to a conclusive proof.

To meet this difficulty Popper proposed a number of methodological rules for reaching decisions about falsification.<sup>1</sup> Bartley objects that they are not needed and, indeed conflict with Popper's own dismissal of attempts to justify theories or beliefs (1982, p.167). It is enough, Bartley thinks, to regard both tests and what they test as criticizable, and to 'stop criticizing - temporarily - ... when we reach positions against which we can find no criticism' (idem, p.159).<sup>2</sup> They agree that nothing should be done to protect a theory against falsification by introducing ad hoc hypotheses, or to reduce its testability by restricting its logical content of consequences.

These points are worth making if only to correct the mistaken but common idea that Popper's idea of falsification means that 'a well-established theory, based on many observations' can be simply disproved 'by a single experiment' (Hicks, 1984, p.216).

The immediate concern, however, is to relate falsification to mainstream theory. Although Smith has been admired for judicious use of a wide knowledge about historical and contemporary conditions, he used them to illustrate, or qualify, his theory rather than to

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1. For a convenient list of such rules see Blaug (1980,p.19).
  2. This comment is in line with Bartley's new theory of 'pancritical rationalism', explained in the article quoted.

test it. A similar comment applies to Marx. Ricardo, from whom Marx derived some of his theory, has long been criticized for ignoring such material, and his successors were not much better in this respect. Blaug has commented on the empirical emptiness of the Hicks-Allen revision of consumer demand theory (1980, p.164), and Caldwell on the difficulties of attempted tests of Samuelson's revealed preference theory (1984, pp. 150-56). The main difficulty is that initial conditions for explaining consumer behaviour by such theories include constancy in tastes or preferences, and such constancy is impossible to check directly and independently.

In any case, economic theorists, as Blaug complains (1980, p.259), have seldom bothered about empirical testing of their explanations. Many have been primarily interested in logical structure and elaboration with little more than casual appeal to commonsense impressions of market behaviour. Some have even held that empirical testing of economic theory is needless or futile, and there is a more widespread view that its assumptions are in no need of direct testing. These matters have to be looked at before considering further the question of falsification.

## II: BASIC ASSUMPTIONS

The most rigorous theoretical system is an axiomatic one, such as Euclid's geometry, which Ricardo admired as a model. It has the merit, for Popper, of facilitating rigorous tests because it reveals any new assumption as a revision of the system (1959, s.16). Nevertheless only a few branches of science have been axiomatized,

and then only temporarily because scientific theories keep changing with the growth of knowledge. Newton's mechanics came close to being an axiomatic system but, although Smith and his followers were influenced by it, no real attempt was made to give their archetypal model an axiomatic formulation until Debreu's famous monograph (1959). Hahn shares Popper's view about an axiomatic system making for rigorous testing and, in a review of Kornai's Anti-Equilibrium, remarked 'before Arrow and Debreu there would have been nothing sufficiently precisely claimed to be the case to enable one to falsify the claim' (1984, p. 137).

Before their work the basic (but incomplete) assumptions of the archetypal model were seen to be institutional, behavioural and technical (Hahn 1984, pp. 72-75; Hutchison 1981, pp. 235-39):

- (a) Economic transactions are supposed to take place within a system of markets subject to a legal framework to protect property and contracts, and facilitated by some kind of monetary arrangements.
- (b) Economic transactors are individuals who pursue their own self-interest in a rational way and on the basis of such complete knowledge that they do not fail to realize planned transactions.
- (c) Production conditions are such that output grows no more rapidly than required inputs - unless there is (unexplained) technical progress.

These assumptions may be usefully classified in accordance with logical distinctions made by Musgrave and approved by Hutchison (1981, pp. 289-90). Negligibility assumptions assert that no other influences than those recognized by the theory have any appreciable influence on the phenomena which it tries to explain. Domain assumptions restrict a theoretical system to specified conditions. Heuristic assumptions simplify analysis of a complex process by taking, as a first approximation, one factor in the process at a time.

Negligibility assumptions could be approved as ruling out use of a ceteris paribus clause which, Hicks claims, is an inevitable feature of economic theories and makes them difficult to test (1983, pp. 371-72). That is precisely the reason for Popper forbidding the use of ceteris paribus; everything which could influence phenomena should be stated in the theory which explains them if its explanation is to be satisfactory (1957, p. 125; 1983, p.288). The institutional assumptions (a) are clearly domain assumptions and both Robbins and Machlup would regard them as important; for, in their view, the main empirical task is to check the applicability of economic theory to particular situations (Caldwell, pp. 102 and 106). The behavioural and technical assumptions (b) and (c), could both be regarded as heuristic - as first approximations to an adequate explanation. But, as Hutchison remarks, even if attempts were made to find higher order explanations these are likely to be multiple and difficult to fit together (1981, p. 290).

Austrian economists took a peculiar epistemological view of the behavioural assumptions. Wieser, Mises and, following them, Robbins held a belief that social scientists differed from natural

scientists in being able to discover scientific truth directly by intuition and empathy. The behavioural assumptions are thus both true a priori and empirically meaningful. It follows that there is no need to test them (Blaug, 1980, p.47; Caldwell, p.121; Hutchison 1981, p.206). As Mises put it: 'they are so broadly based in common human experience that once enunciated they become self-evident and hence do not have to meet the fashionable criterion of falsifiability'. If that could be accepted, any correct deduction from these true assumptions, and initial conditions within the domain of the theory, would be true empirical statements. Popper, of course, emphatically rejects such a view; intuition may be a valuable stimulus to conjectures but it is by no means a reliable source of truth (1963, p. 28).

Blaug's statement about Popper's influence on modern economists is made subject to the important qualification that few of them have ever read him, but accepted the 'twist' which Friedman gave to the Popperian methodology (1978, p.714). By this Blaug means Friedman's influential assertion that the truth or falsity of a theory's assumptions are irrelevant to its scientific worth, which depends only on its ability to yield non-falsified but falsifiable predictions (Friedman, pp. 8-9). The Austrians' Verstehen belief is thus superfluous. Friedman also differs from them in accepting the need for economic theory to be quantitatively tested.

He also differs from Popper in holding the instrumentalist view (deriving from Berkeley and Mach) that scientific theories are no more than instruments for enabling us to derive predictions from

initial conditions. As instruments cannot be true or false, theories have to be judged only by their suitability for predicting. This stance has also been taken by a new breed of 'rational expectations' theorists, and Hahn has tartly observed that 'just so might an ancient Roman have spoken about the oracles .... I choose to be an economist - not a witchdoctor!' (1984, p.312).

Nor does Popper hold with it. For him the basic aim of science is explanation, not prediction, and a wrong explanation may yield good predictions. More important, the scientific value of predictions is that they are tests of a theory - tests of its explanatory power, not of its predictive power. (1983, s.12). There are, moreover, two kinds of prediction, those of a known kind of event and those of a new kind of event or discoveries. Instrumentalism can account only for the first kind of prediction because, if theories are regarded as instruments for prediction, their purpose must, like that of other instruments, be determined in advance, i.e. by the known kind of predictions. From the standpoint of scientific progress, therefore, instrumentalism is a retarding position (1963, pp. 117-18).

There is, too, a problem of deciding between rival theories which happen to yield equally good predictions. Friedman's choice is on grounds of simplicity. That idea was also held by conventionalists like Poincaré and Duheim who had argued that theories are conventions about the simplest and most convenient ways of describing or interpreting nature.<sup>3</sup> They, accordingly, also held that there could be no question

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3. Blaug finds evidence of conventionalism, too, in Smith's work (1980, p. 57).

of falsifying , or 'verifying', scientific laws because these are needed to determine what an observation is, more particularly a measurement. Simplicity, not truth, had then to be the criterion of choice between theories.

Popper has more respect for conventionalism than for instrumentalism, regarding it as a 'system which is self-contained and defensible', but argues against accepting it. (1959, s.19). The reason is that, if truth is regarded as irrelevant, we can always evade falsification by modifying definitions or introducing auxiliary hypotheses. This was seen as long ago as 1803 by the great chemist Black who wrote: 'a nice adaptation of conditions will make almost any hypothesis agree with the phenomena. That will please the imagination but does not advance our knowledge'.

To avoid this danger of scientific stagnation Popper advocates the acceptance of a methodological rule: accept only those auxiliary hypotheses whose introduction does not diminish the degree of falsifiability or testability of the system in question (1959, p. 83). In regard to the suggested criterion of simplicity, he says that aesthetic or pragmatic preferences are of little significance for the theory of knowledge, but simple statements are more valuable than a complex alternative 'because their empirical content is greater and because they are better testable' (1959, p. 142).

III: THE RATIONALITY PRINCIPLE

Although rejecting intuitionism, instrumentalism or conventionalism, and insisting upon the need for empirical testing, Popper comes fairly close to some views of the Austrian school. He believes, for example, that 'the main task of the theoretical social sciences ... is to trace the unwanted repercussions of intentional human actions' (1963, p. 342), a view which was stressed by Manger (Hutchison, 1981, p. 183-87) and, of course, reflects Smith's Invisible Hand.

He holds, too, their principle of methodological individualism: 'social phenomena, including collectives, should be analysed in terms of individuals and their actions and relations' (1963, p.341). This viewpoint has been adopted by all theorists of general equilibrium, and quite explicitly by Hahn (1984, p.1), although Blaug has reservations about it as an exclusive principle (1980, p.51) as do some macro-theorists.

In a qualified way he also comes to a position which is close to their view of the behavioural assumption. For, contrary to the opinion expressed by Planck to Keynes (Economic Journal 1924, p. 206) Popper thinks that economics can be simpler than physics because, in most situations, people act more or less rationally. 'This makes it possible to construct comparatively simple models of their actions and interactions, and to use these models as approximations'. It is, he holds, this advantage which constitutes the main difference between economics and the natural sciences

because other differences, such as specific problems of conducting experiments or applying quantitative methods, are differences only of degree (1957, pp. 140-141).

There is, accordingly, for the social sciences, a distinctive method which he calls 'situational logic', the 'zero method' or the 'method of logical reconstruction'. This involves constructing a model of a problem situation, containing all relevant aims and all available relevant information, 'especially that of possible means for realizing these aims' (1967, p. 359). To 'animate' the model we use a rationality principle - 'the principle of acting appropriately to the situation; clearly an almost empty principle'. He gives as an example 'the pure logic of choice as described by the equations of economics' (1957, p. 141), and elsewhere tells us that he was attempting 'to generalize the method of economic theory (marginal utility theory) so as to become applicable to the other theoretical social sciences' (1974, p. 93).

The 'almost emptiness' of the rationality principle is due to having 'little or nothing to do with the empirical psychological assertion' that people act, in most cases, rationally. (1967, p. 359).

Rather it turns out to be an aspect of, of a consequence of, the methodological postulate that we should pack or cram out whole theoretical effort ... into an analysis of the situation; into the model. If we adopt this methodological postulate, then, as a consequence, the animating law will become a kind of zero principle. For the principle may be stated in this way: we assume no more than that the actors act within terms of the model, or they 'work out' what was implicit in the situation. This, incidentally, is what the term situational logic refers to.

This interpretation, unlike the Austrian one, is not held to be a priori valid. It cannot be because, as an oversimplification, it is not universally true. But neither, as empiricists would require, has it to be directly tested, because it has the status of a methodological principle. What has to be tested is the whole situational model, which includes the principle. If such tests are unfavourable, then 'it is a sound methodological policy to decide not to make the rationality principle accountable but the rest of the theory; that is, the model' (idem, p. 362). The main reasons for adopting this policy are that it leads to more informative testing of a social theory, and the rationality principle, although false, is 'a good approximation to the truth'. An attempt to replace it, moreover, 'seems to lead to complete arbitrariness in our model building' (idem, p. 362).

Simon, has the same idea of defining what he calls 'substantial rationality' in relation to 'the achievement of given goals within the limits imposed by given conditions and constraints'. That describes the behavioural assumption of mainstream economics when goals are specified as maximizing utility and profits. It 'freed economics from any dependence on psychology' (1979, pp. 67-68).

Obvious difficulties arise if account is taken of incomplete information and risk or uncertainty. It has long been recognized that problems of duopoly or oligopoly could have a determinate solution only by making unpalatable further assumptions. Imperfect competition presents a similar difficulty, and it is only in quite recent years that general equilibrium theorists have sought to relax the institutional

assumption of perfectly competitive markets for goods and labour.

Even with the assumption of perfect competition the theory has the serious weakness of failure to account for money's function as a 'store of value' - as a financial asset which confers liquidity. People and firms need liquidity because of uncertainty about future market conditions, even if they have none about present markets. Yet, as Hahn points out, 'the best developed model of the economy' - due to Arrow and Debreu - cannot find room for money (1982, p.1). Nor, he adds, do we have any 'theory of expectations firmly founded on elementary principles comparable, say, to the theory of consumer choice' (idem, p.3).

That might be challenged in view of the development of a new theory of 'rational expectations'. Hahn shows, however, that there is 'a logical canker at the heart' of this theory because 'there can be a whole manifold of rational expectation paths' (1984, p.322). It might also be challenged in view of claims like Malinvaud's that 'the logical extension of microeconomic theories to situations involving uncertainty has been well elucidated' (1972, p.273). But, as Radner says, this extension 'requires that the economic agents possess capabilities of imagination and calculation that exceed reality by many orders of magnitude ... (and) that the theory requires in principle a complete system of insurance and futures markets which appears to be too complex, detailed and refined to have practical significance' (1968, p.45). According to Hayek, moreover, we could never have anything like complete, relevant information about even present economic conditions because that has to include ways in which they are interconnected (p.1). Simon

argues that, even if we had it, and were helped by computers, we could not hope, as consumers or producers, to process it in order to arrive at the optimal solutions stressed in economic theorizing (1979, p. 72).

For him, the only hope of understanding imperfect competition and decisions made under conditions of uncertainty is through a shift from the substantial rationality of situational analysis to 'procedural rationality', defined as behaviour which is 'the outcome of appropriate deliberation'. This requires research into the ways consumers, producers and investors make their decisions. 'Choosing between alternative models of the situation', he says, 'calls for determining empirically the processes used by the person or organization making the decisions' (p. 84).

Popper has a different view. He has distinguished between predictions which relate to a specific event and those which relate to a kind of event. In physics both types of prediction can often be made but sometimes only the second; it can explain the ocean's waves but not the course of any particular wave. Economics, and other social sciences, are largely confined, he thinks, to the second type of prediction, called by Hayek 'explanation in principle'. This requires situational analysis designed to explain, in a rough and ready way, a kind of event, and in which initial conditions for predicting the event are replaced by a model which 'incorporates typical initial conditions' (1967, p. 358). And, in such a model, 'we replace concrete psychological experiences (or desires, hopes, tendencies) by abstract and situational elements, such as "aims" and "knowledge" (p. 359).

Aims, presumably, have to include reduction of risk, and the objective situation methods for reducing or avoiding risk; e.g., information services, inventory adjustment, liquidity holdings, insurance and futures markets. Knowledge, similarly, would have to be regarded as partly subjective and related to uncertainty via expectations. Popper has not given guidance on these important matters,<sup>4</sup> and we are still handicapped, as Hahn emphasizes, by lack of any satisfactory theory of expectations. It is not, of course, a difficulty confined to mainstream economics. 'Radical economics' may seek to avoid it by eliminating or severely restricting demand conditions from its own analyses, but that is even more unrealistic.

#### IV: FALSIFICATION AND HISTORY

How far have economists followed Popper's advocacy of subjecting scientific theories to rigorous tests? Very little, according to Blaug, who says they have preached it but rarely practised it (1980, p. 259). Some even doubt whether there is anything to test. Hayek, for example, has said: "I rather doubt whether we have any "laws" which social phenomena obey' (1967, p.42).

Popper has always acknowledged that such testing is far from easy for social sciences.

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4. Popper alluded to this problem very indirectly when he wrote that situational logic meant 'constructing a model on the assumption of complete rationality (and perhaps also on the assumption of complete information) on the part of all the individuals concerned'. (1957, p.141).

But it cannot be doubted that there are some fundamental difficulties here. In physics, for example, the parameters of our equations can, in principle, be reduced to a small number of parameters - a reduction which has been successfully carried out in many important cases. This is not so in economics; here our parameters are themselves in the most important cases quickly changing variables. This clearly reduces the significance, interpretability and testability of measurements. (1957, pp. 142-43).

Later he admitted that tests of a situational model 'are not easily obtainable and usually not very clearcut', because models are 'always and necessarily rough' and so have 'a comparatively low degree of testability'. Nevertheless, testing may help us to choose between rival models, and, in the social sciences, tests of a situational analysis can sometimes be provided by historical research' (1967, p. 360).

Hicks, indeed, argues that 'economics is on the edge of science and on the edge of history' (1979, p.4) - on the edge of science because of inherent difficulties in testing economic theories, and on the edge of history because 'economics is in time, and therefore in history in a way that science is not'. (1984, p. 218).

I find that all experimental sciences are, in the economic sense 'static' ... it does not matter when an experiment is performed ... The more characteristic economic problems are problems of change, of growth and retrogression, and of fluctuation. The extent to which these can be reduced into scientific terms is rather limited; for at every stage in an economic process new things are happening ... We need a theory that will help us with these problems; but it is impossible to believe that it can ever be a complete theory. (1979, pp. x-xi).

Hutchison had similar thoughts.

Because of the 'complexity' or 'openness' of the material, paucity of constraints, or because of what might be called the historical-institutional dimension, there is the need for repeated testing and retesting of relationships which cannot be taken as constant, regular or even 'normal' over long periods. Unlike laws, trends have to be constantly retested in case they are altering, and it is on trends that much or most economic predictions have to be based. (1981, p. 284).

It is not clear what Hutchison means by trends. If they are merely statistical descriptions, then they have no value for scientific explanation or prediction. J.S. Mill had seen the point in his Logic, and warned that trends should not be used unless they could be explained by reference to a set of genuine laws. Popper agrees, saying that 'the causal explanation of a regularity consists in the deduction of a law (containing the conditions under which the regularity asserted holds) from a set of more general laws which have been tested and confirmed independently' (1957, p.125). These initial conditions have to be stated fully and explicitly. The trend changes if they change, but so long as they persist it can be treated like a law and made to yield predictions. (These points could be easily demonstrated by mathematical theories of economic growth.)

Hicks does not refer to trends but to a 'counter-factual method' used by 'the more ambitious kind of historian who does attempt to explain facts', as distinct from narrating them.

It is becoming understood, by such historians, that causal explanation of general changes that have occurred in the past requires the use of a model. To say that A was the cause of B is to say that, if A had not happened B would not have occurred. But how can we say that? We do know that A did happen, we do not know, in the same ways, what would have happened if A had not happened. We can only guess it or, better, deduce it from general principles; but for that limited purpose quite weak principles may suffice. A model is a structure that is based on such principles. (1984, p. 218).

This counter-factual method has an obvious correspondence to Popper's situational logic applied to historical explanation (1957, s.31). That logic is a conjectural and idealized reconstruction of a situation, interpreting the way people tried to deal with it, and also how far they then had their perspectives and behaviour change.

In other words, our scheme of problem-solving by conjecture and refutation or a similar scheme may be used as an explanatory theory of human actions, since we can interpret actions as an attempt to solve a problem. Thus the explanatory theory of action will, in the main, consist of a conjectural reconstruction of the problem and its background. A theory of this kind may well be testable (1972, p.179).

Such theories or models can be used, together with other familiar universal laws (often trivial ones) for purposes of historical explanation (1957, p.149).

Most testing of economic theories has been done by econometric analysis of historical data - time series. Popper, unfortunately, has not considered this aspect of our problem, but Hicks has found that econometric testing has serious limitations.

How far can we be justified in claiming that a model has been successfully fitted, when the deviations of experience, from what has been predicted by the model, satisfy tests of randomness? That must presumably mean that in a sufficiently long series they would average out. But how can one apply that notion to a period of history? (1984, p. 218).

Elsewhere he has said that 'the great historical, or structural, changes which have occurred with such dismaying frequency in the present century cannot plausibly be treated as random disturbances' (1983, p. 369).

This criticism loses some of its force if we follow Popper's idea that verification of a theory is never possible, only its falsification. If a theory does not survive econometric testing, that is a reason for abandoning it and looking for a better one; if it does survive such testing we could accept it only provisionally. The 'historical, or structural, changes', moreover, to which Hicks refers, should be viewed as changes in the initial conditions of a theory, and not put anonymously into any 'pound of ceteris paribus'. The Austrians are surely correct in stressing the need for checking that the conditions of a theory hold for any situation before seeking to apply it to that situation. And, if they are not met, it may be possible to modify the theory so that it applies to both the old and the new situation; e.g. by introducing further explanatory variables or even dummy variables if changed conditions cannot be quantified.

Doubts, too, about the significance of statistical tests for data which cannot be part of a 'sufficiently long series' may be eased by noticing Malinvaud's distinction between data which can be considered as randomly determined by the process which generates them and those which can be considered as randomly selected from a deterministic universe. He and other econometricians adopt the first position (1968, p. 65). Popper's 'propensity' interpretation of probability also regards this as the real disposition of an objective situation to generate virtual relative frequencies of occurrence for a particular event (1983, s.20, 1984, pp. 68-74). Nevertheless there are well-known practical difficulties about randomness in deviations from model estimates; e.g. systematic errors of measurement,

mis-specified equations, omitted variables, lagged endogenous variables treated as predetermined, etc.

It has also to be recognized that there is considerable disillusionment with results of econometric work. Hahn has remarked that the methods of Roman augurs 'worked as well as does most econometrics' (1984, p.312), and Blaug has complained that often 'econometric studies reach conflicting conclusions and, given the available data, there are frequently no effective methods for deciding which conclusion is correct' (1980, p.261). Leontief goes further: 'econometricians fit algebraic functions of all possible shapes to essentially the same sets of data without being able to advance, in any perceptible way, a systematic understanding of the structure and operations of a real economic system' (Eichner, pp. x-xi).

One source of trouble has been identified as 'lack of correspondence between the terms in which the theory runs and the terms in which it is described' (Hicks, 1983, p. 372), due, Leontief thinks, to habits of relying upon official statistics or otherwise readily accessed data which was compiled for administrative, business or other purposes. He invites economists to accept, as natural scientists have done, 'the harsh discipline of fact finding'.

Kenen stresses insufficient care in distinguishing between 'the testing of hypotheses and the estimation of structural relationships', and calls for more effort in finding 'tests that will help us to discriminate between hypotheses having different economic implications'. (Quoted by Blaug (1980), p.257).<sup>5</sup> There

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5. One such attempt is that of Granger for testing bivariate causation (Econometrica, 1969). But Osborn (Supplement to the Economic Journal, 1984) has shown that, if this method is extended to a multivariate model, it applies to the reduced form, instead of to the structural equations.

is , indeed, a good deal of instrumentalism in econometric work whereby equation forms and variables are selected or adjusted so as to make theories give better predictions, without due consideration of the need for theoretical justification of such changes, damage to the generality of a theory, or the need for testing against rival hypotheses.

V: INDUCTIVE PROBABILITY

Economists need no absolution from the sin of induction because they have never practised it. J.S. Mill, it is true, wrote approvingly, and at length, about induction in his Logic, but held that it could not be used in social sciences because of the complexity of their data and the impossibility of experiment. J.N. Keynes took a similar view. Jevons also wrote much about induction in his Principles of Science yet interpreted it, not as a generalization from particular cases, but as conferring probability upon a hypothesis (which 'we must invent' ) if that hypothesis 'yields deductive results in accordance with experiment'. (p.228).

That viewpoint was clearly expressed by Bertrand Russell.

The principle of induction, as applied to causation, says that if A had been found very often accompanied or followed by B, then it is probable that on the next occasion on which A is observed it will be accompanied or followed by B. ... The principle itself cannot, of course, without circularity be inferred from observed uniformities, since it is required to justify any such inference. It must therefore be, or be deduced from, an independent principle not based on experience (1946, p.699).

Later he found 'induction is not quite the universal proposition we need to justify scientific inference', but thought one or more were needed for the logical justification of science (1948, p. 524).

Hicks has recently come to something like the same view. He accepts, with Jevons, Russell and Popper, that one cannot use induction to provide a scientific law, but gives a confused argument that there are 'two senses of implication, empirical and logical' and tries to bring the two together.

Can that be done? There is plenty of experience ... to show that it can. For the statement of association, though itself of a purely inductive character, is nevertheless a proposition; and in that capacity ... it can have implications, in the logical sense. Some of these implications may be testable, inductively. If the test is successful, a bridge, a logical bridge has been built between the two inductions; the coherence, the logical coherence, of the bridge strengthens our confidence in the induction which it supports. (1979, p. 29).

But neither of his 'two inductions' comes within the generally accepted meaning of induction as the inference of a universal empirical statement from particular empirical statements. No such inference is valid and, as Jevons saw, the first induction is an 'invented hypothesis'. The second induction is a finite number of particular test statements which agree with particular statements deduced from the hypothesis; they could not verify it but could falsify it.

Hicks then moved to a view of inductive probability.

No induction can provide a proof, a complete proof. Nevertheless, when there are several propositions that have been (provisionally) established by induction, they can have logical, and particularly mathematical, consequences; and the consequences may be such that

they can be tested empirically. The test, again, is an induction; it provides no proof. But when several (inductive) propositions have been provisionally established and it has also been shown that there is a logical connection between them, then each of them fortifies the others. (1984, p. 216).

I interpret this to mean that our confidence or 'degree of belief' in a theory increases as different tests of its consequences fail to falsify it. Earlier Hicks approved Jeffrey's definition of probability as 'a valid primitive idea expressing the degree of confidence that we may reasonably have in a proposition, even though we may not be able to give either a deductive proof or disproof of it'. He thus accepted the subjective view of probability as depending on knowledge, and so on successful testing of a theory. (1979, Ch. VIII).

Many scientists, of course, including nuclear physicists, take the same view of 'inductive probability' as Russell, Jeffrey, Hicks and, it may be added, Keynes. Popper has long argued against both a subjective interpretation of probability as degree of rational belief and the possibility of an inductive logic (1983, Part II). Quite recently he and Miller have given a short proof that, on any interpretation of probability which is consistent with the calculus of probability, evidence cannot increase the probability of any statement which goes beyond that evidence. (1983a and 1984).

Let  $h$  denote a hypothesis and  $e$  evidence which favours it in the sense that

$$h \rightarrow e, p(e, h) = 1, p(he) = p(h)$$

where  $\rightarrow$  signifies entailment,  $p(e, h)$  the conditional probability of  $e$  with respect to  $h$  and  $p(he)$  the joint probability of  $h$  and  $e$ . The

hypothesis can be expressed as a conjunction of an implication and a disjunction,

$$h = (h \leftarrow e) (h \vee e)$$

$\vee$  signifying the inclusive connective "or". In this expression  $(h \leftarrow e)$  represents all the logical content of  $h$  which goes beyond  $e$  because  $e \rightarrow (h \vee e)$ . We have, moreover, given  $e$ ,

$$p(h \leftarrow e, e) = p[(h \leftarrow e) \vee e, e] = p(h \vee e, e) = p(h, e) \quad (1)$$

The question of inductive probability can be put as follows.

Assuming that

$$p(h, e) > p(h) \quad (2)$$

does  $e$  make

$$p(h \leftarrow e, e) > p(h \leftarrow e)$$

i.e. does  $e$  support  $(h \leftarrow e)$ ? The answer is no because it can be shown that

$$p(h \leftarrow e) - p(h \leftarrow e, e) = p(\sim h, e) p(\sim e) > 0 \quad (3)$$

where  $\sim$  signifies negation.

Using the probability calculus we find

$$\begin{aligned} \text{RHS} &= [1 - p(h, e)] [1 - p(e)] \\ &= 1 - p(h, e) - p(e) + p(h, e) p(e) \\ &= 1 - p(h, e) - p(e) + p(h \vee e) \\ &= 1 - p(h, e) - p(e) + [p(e) - p(\sim h, e)] \\ &= 1 - p(h, e) - p(\sim h, e) \end{aligned}$$

Using now (1) and the propositional calculus we have

$$\begin{aligned} \text{RHS} &= p[\sim(\sim h, e)] - p(h, e) \\ &= p(h \leftarrow e) - p(h \leftarrow e, e) \end{aligned}$$

This result means that 'all probabilistic support that is not counter support is purely deductive' (1984, p.434) - a result which is 'completely devastating to the inductive interpretation of the calculus of probability' (1983, p.688).

#### V: CONCLUSION

It will be evident from this account that Popper has had no more than a peripheral influence on the practice of economics, and so has little, if any, responsibility for the regrettable scientific state of our discipline. Although economists have always paid some attention to past or present economic conditions, they have seldom attempted to derive their theories from facts and so have found little need of his critique of induction. They have paid some attention to his stress on falsification, but have seldom understood it properly. The consequence has been undue emphasis on prediction, to the neglect of explanation, and too little attempt at stringent testing of theories to help rational choice between them. His warnings against conventionalism and instrumentalism have hardly been noticed, and there has been some kind of wrong assumption that successful tests of a theory increase its probability, as distinct from its 'degree of corroboration'. (1983, s.28).

The strongest link between his views and those of economists seem to be in regard to the rationality principle, although it was used by Adam Smith, and invoked by Austrian economists, long before Popper's birth. It is possible to see a connection, however imperfect,

between Smith's invisible hand, Austrian praxeology, Popper's situational logic and Hicks' counterfactual method. What they have in common is an interpretation of rationality as 'substantial' in the sense of referring to the behaviour of agents that is 'appropriate' for achieving their aims, and so neglecting what Simons calls 'procedural' rationality. Emphasis, that is, is put on the objective features of a problem situation in finding solutions, to the neglect of psychological processes.

Its application has, no doubt, led to many valuable insights, but these must fall short of scientific explanation until the principle can be made to embrace satisfactorily those important choices or decisions that go beyond objectively measurable risks. Such embracement cannot be asserted as impossible, but it is, of course, still far from being realized. Until it is realized, the scientific value of economics may be limited, as Hahn thinks, to giving logical refutations of unsound theoretical or policy proposals; or as Hicks (agreeing with J.M. Keynes) thinks, to clarifying and sharpening thought on what have still to be more or less matters of imperfectly informed judgment. And, even if it were realized, we would have much rougher approximations to truth than experimental sciences reach because of the inherent limitations of the rationality principle, and the likely incoherence of other approaches to our problems.

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