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METHODOLOGICAL SCEPTICISM

by

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I

In 1914 Henri Poncaré wrote that sociology, in which he would undoubtedly have included economics, 'is the science with the greatest number of methods and the least results'.[1] Nearly eighty years later, John Hicks, pre-eminent among economic theorists, had become sufficiently sceptical of his subject as to claim that it is 'a discipline not a science'.[2] He is, of course, by no means alone in holding this kind of view. Presidential addresses to economic societies seem to show a marked trend towards such scepticism as respected economists near the close of professional life. Nor is it confined to old people; there is, today, widespread dissatisfaction with the world's economic problems and the inability of conflicting schools of thought to explain them adequately.

Hicks doubts go deeper than that as they focus on a basic question of the applicability of scientific methods to economic data. This question was discussed years ago by an equally distinguished thinker, the philosopher of scientific discovery, Karl Popper, who concluded that 'all theoretical or generalizing sciences make use of the same method, whether they are natural or social sciences', - a method which always amounts to 'deductive causal explanation, prediction and testing'.[3] It may, then, be instructive to compare these different views on the nature and significance of economic science or inquiry.

II

Hicks makes a distinction between predictions that are strong or almost certain and those that are weak or merely probable. This is related, surely, to an earlier acceptance of the logico-subjective interpretation of probability, developed by Keynes and Jeffreys, as the most appropriate one for economics.[4] The distinction is not absolute, but the predictions of natural science are much stronger than those of economics, which are always weak and require judgement about their reliability. 'The issue', he writes, 'is whether that reliability is suitably assessed by the method that is appropriate in experimental sciences, by a statistical standard error ... The applicability of the calculus (of probability) to economic forecasting cannot be taken for granted; it always requires defence.'[5]

So far, the critique is not a strong one. Popper rejects the logico-subjective interpretation of probability as hopeless: the subjective interpretation of probability as degree of rational belief is bound to take the logical theory as its basis, and there is a basic difference between logical and objective probabilities in that the former are untestable tautologies and the latter testable hypotheses.[6] That, however, is a side issue here. The main one is the applicability of the probability calculus to testing economic theories because forecasts or predictions are a type of test.[7]

Hicks gives only a brief explanation of reasons for caution in applying the probability calculus; 'the great historical, or structural, changes which have occurred with such dismaying frequency in the present century cannot possibly be treated as random disturbances'.[8] As Feller remarks, 'the word "random" is not well-defined',[9] but Hicks, in spite of favouring a subjective interpretation of probability, relies upon an objective definition from the relative frequency theory of von Mises, whose axiom of randomness excluded sequences of observations from which gamblers could profit.[10] Tests of statistical significance also require independence of observations, i.e. that later observations are not influenced by earlier ones, or that the joint probability of two or more occurrences of the same event is the product of their separate probabilities.[11] For such independence is required for the derivation of the laws of large numbers, the binomial and normal distributions, the central limit theorem, and their derivatives for small-scale sampling theory such as the Student t-distribution or the ψ^2 and F distributions. None of these, however, can be deduced from a logico-subjective interpretation because that excludes independence.[12] Keynes himself had noted this[13] yet used the law of large numbers as a supposed bridge between subjective and objective probabilities.

If, following Popper, we use

$$p(a, b) = f$$

to denote the probability of a, given b, is objectively f (a relative frequency), then b, on a subjectivist interpretation, denotes subjective knowledge about an event which must change with every occurrence of it or of other events. Probability of successive occurrences cannot, then, be constant but would be affected by earlier occurrences which increase b, or subjective knowledge. On an objectivist interpretation, however, independence is guaranteed for observations because b means constancy of experimental conditions, or constancy of relevant objective conditions.

It would appear, then, that Hicks could be questioning the constancy of relevant objective conditions in regard to econometric explanations of economic events, together with the assumption that 'errors' in these explanations are random. Such explanations are, of course, quantitative and, on that account, meet Popper's approval because quantification makes theories more testable.[14] They would also be inherently probabilistic, not only because of the well-known phenomenon of errors in measuring variables, but also, if he is right, in asserting a basic indeterminacy in even natural phenomena.[15]

the kind of absolute chance asserted by quantum mechanics, and well brought out in another way by A. Landé.[16] We might thus express an economic theory as

$$x - \phi(y, \theta) = P(\varepsilon, \tau)$$

where x and ε are scalars; y, θ and τ are vectors; ϕ is a functional relation between x and y with parameters, θ; and P is a probability distribution of ε with parameters, τ. Here ε is to be interpreted as a compound of errors of measurement in x and y, and of the absolute error from the basic indeterminancy of x. Here ϕ is a conjectural hypothesis, and P is also one, applying to conjectured relative frequencies of errors in virtual (finite) sequences of well defined observations, as distinct from their actual relative frequencies against which P is to be tested.[17]

Application of the probability calculus requires that measurement of variables is sufficiently precise for its errors to be independent and random. That may be difficult to achieve in economic data because of incomplete coverage or the imperfections of aggregation. Hicks emphasizes here, too, discrepancies between theoretical concepts and their practical counterparts, mentioning as obvious examples cost, profit or quantity of money.[18] Trial and error, on the sides of both theory and practice, can lead to better choices and so measurements, but range of choice may be severely limited by lack of quantitative data which is often difficult and expensive to collect, requiring authority and resources available only to governments.

Econometricians are well aware of other sources of error. Functional relations may be inaccurately specified because of faulty choice of variables or omissions. Trial and error may again improve the situation but there is a computational limitation of having to work with functions which have a linear form, so that these may be only approximations to theoretical functions, although often quite good ones. More important, estimation of the parameters in a function is affected by interdependence between explanatory variables, one that is to be expected because of general interactions between economic agents or sectors. The econometric solution of that problem is to work with more or less complete systems of equations that distinguish between endogenous variables, which interact, and exogenous variables which can be taken to be independent of one another. That, of course, makes heavy demands on data, and leads to problems of identifying parameters for the endogenous variables. If, moreover, lagged exogenous variables have to be treated as predetermined, it cannot be assumed that their successive values are independent, so that new difficulties arise in applying the probability calculus. There is, too, a further difficulty in regard to degrees of freedom; the significance of estimates of parameter values depends on a sufficient excess of joint observations of variables over the number of parameters to be estimated. The number of explanatory variables to be included in a function may thus have to be severely restricted below theoretical requirements, and this can lead to misspecification due to omitted variables.

The foregoing reminder of difficulties in econometric estimation might be extended, but it is sufficient to illustrate the seriousness of Hicks' doubts regarding tests of significance for empirical formulation of economic theory, tests which must rely upon the probability calculus. General doubts, however, fall short of disproving particular cases. There are tests for independence and randomness, and some estimates of dependent variables and parameters survive them, especially if we can apply a method of estimation such as generalized least squares (notably Durbin's version). E. Malinvaud, for example, has concluded that 'once he has chosen his model, the econometrician will generally be in a position to judge which test or which estimation method is appropriate to the particular case in which he is interested',[19] although stressing the need for good theoretical models and good statistical data. Popper admits that there are cases where all attempts to find a law have failed and all predictions have been falsified; but 'in no case can we say with finality that there are no laws in a particular field. (This is a consequence of the impossibility of verification.)'[20]

III

This leads to the second leg of Hicks' argument: 'our theories are not that sort of theory',[21] i.e. the sort which can be falsified by confrontation with fact. The reason given is that economic laws 'are always subject to a ceteris paribus clause'.[22] He surely cannot mean that scientific laws are unconditional; for every law, empirical or logical, relates to a universe of discourse or class of things which are considered as coming within the framework of the law.[23] As, moreover, a law is a universal statement the framework must not be so narrow as to specify only a unique occurrence; on the contrary, a scientific law is more valuable the greater is its level of universality because that increases its empirical content and testability.[24] Even Newton's very general laws of motion are now recognized as requiring velocities to be small relatively to the speed of light, and gravitational fields to be less than enormous. Similarly, the general gas law holds exactly only for an 'ideal gas'; i.e. one with negligible intermolecular attractions and molecules of negligible size.

There are, of course, other senses of invoking ceteris paribus, and their explanation is helped, at the same time as Popper's idea of 'confronting theory with fact' is explained, by means of the following symbolism.

Let

U = a system of universal statements that include deductive laws of logic and mathematics[25] but more particularly empirical laws.

u = another empirical law which is conjoined to U and which is to be tested.

c = a class of singular existential statements which describe specific events, C, (an event being a class of occurrences that differ only in regard to their individual positions in space and time).

e = a singular existential statement which both describes another event, E, and follows logically from U, u and c.

If then, U, u and c are held to be true, e is also held to be true, and C is said to cause E or E to be the effect of C.[26] Testing the theory u means trying to falsify it[27] because although

$$[(U \wedge u \wedge c) \rightarrow e] \wedge [e \wedge U \wedge c] \nrightarrow u$$

it must be the case that

$$[(U \wedge u \wedge c) \rightarrow e] \wedge [\sim e \wedge U \wedge c] \rightarrow \sim u.$$

But acceptance of the truth of the negation of e is a decision based on reasoned convention which is always open to critical scrutiny, and so revision.[28] For such acceptance is not a matter of logical compulsion but one of reasoned judgement, possibly made in the light of other considerations, including theoretical considerations. In the case of quantitative theory, acceptance depends on the accuracy of measurements, which are themselves 'theory-impregnated'[29] and not so fully objective. Popper remarks that 'the empirical basis of objective science thus has nothing "absolute" about it. Science does not rest on rock-bottom'.[30] It is an interactive process of conjecturing tentative hypothesis and testing them inter-subjectively by interpretations of relevant and reproducible observations or measurements.

This account can accommodate Hicks' classification of causal types.[31] He distinguishes between those which are:

(i) additive, i.e. operate both separately and jointly;

(ii) sole, i.e. operate in isolation;

(iii) overlapping, i.e. operate only jointly and simultaneously;

(iv) negative, i.e. do not operate directly but offset other causes which would inhibit still other causes.

Popper's 'initial conditions', C, would include (i) and (ii), and also (iii), although the joint causes here could be treated as a single (conjunctive) cause. We would have,

(i) $(c_1 \vee c_2) \vee (c_1 \wedge c_2) \rightarrow e$

(ii) $c_1 \rightarrow e$

(iii) $c_1 \wedge c_2 \equiv c_3 \rightarrow e$

and could represent (iv) by a causal chain of conjunctions

(iv) $[(c_1 \rightarrow {\sim}c_2) \wedge (c_2 \rightarrow {\sim}c_3) \wedge c_3 \rightarrow e] \rightarrow c_3 \rightarrow e$

as the negative causes c_1 and c_2 cancel out to leave c_3 as the cause of e. But it could also be argued that the above chain yields

$$c_1 \rightarrow c_3 \rightarrow e$$

so that c_1 could be regarded as the cause of e, given the presence of c_2.

It is perhaps clearer to think here of a system of theories, the "background" theories, U, and specific hypotheses, u_1, u_2 and u_3, each relating to one of the causal conjunctions. Hicks does not explicitly consider theories, but focuses attention on separable and non-separable causes, a distinction which, for quantitative theories, seems to correspond to that between exogenous and endogenous variables. The distinction may be formulated as requiring

$$c_1 \rightarrow c_2 \ , \ c_2 \nrightarrow c_1$$

for c and c_2 to be separable causes, and

$$c_1 \leftrightarrow c_2$$

for them to be non-separable. (The distinction also corresponds to that between a sufficient condition and a necessary and sufficient one.)

In the case of quantitative theories, taking background theories, U, for granted, we would have a system, or model, of "structural" equations, u_s, containing both exogenous variables, c^*, and endogenous variables, c. If the system of equations could be solved we would, of course, be able to bring it into the form of reduced equations, u_r, explaining the c in terms of the c^*. There would then be two causal explanations for each c, a structural explanation in terms of c^* and other c, and another in terms only of c^*, but they need not be inconsistent. Hicks rightly says that 'the abundance of exogenous elements in economics is ... an indication of the modesty of the scientific status ... which is all that economics can hope to achieve',[32] an over-dogmatic statement in its a priorism but correctly indicating what Popper would call a 'low degree of universality'.[33] Scientific progress means reaching towards theories of greater universality which would here involve conversion of exogenous into endogenous variables, or else dispensing with the need for some exogenous variables in causal explanations.

To revert to ceteris paribus. The scope of this restriction is obviously less in the case of a system or model of structural equations than in that of a single structural equation, being confined to the model's exogenous variables. And the greater the level of the model's generality, the smaller will be the number of such variables, and so the scope of ceteris paribus. But, as Popper points out,[34] ceteris paribus must not apply in another sense - in the sense of holding constant any of the conditions or variables which are not included in his initial conditions because they are held to be irrelevant. In this sense 'a ceteris paribus clause ... must not be added to a theory since it would destroy its testability'. Testing involves trying out a theory under wide variation of irrelevant conditions.

Hicks, however, in questioning the applicability of the frequency interpretation of probability to economics, seems to question the extent to which this sort of testing is possible. The econometrician, he says, must be treating observations known to him (extending, say, from 1960 to 1977) as a sample of a "larger" population; but what population?[35] The criticism, however, fails to see that the modern, axiomatic treatment of probability[36] makes it a theoretical concept relating to a theoretical population or, better, a sample space of logically possible events resulting from a given ideal experiment (or objective situation described by a causal theory). Probabilities thus conjectured have themselves to be tested by comparison with objective relative frequencies for such time periods as are available. These need not be anything like infinite, or even large if the conditions for small sampling theory are met.

There is, however, the associated question of the permanence or durability of causal economic theories, one which Popper has acknowledged:[37]

> '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 natural constants - 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.'

Compare, for example, a physical and an economic equation which have a similar form. Van der Waal's equation for gases may be written as

$$\bar{P}\ \bar{V} = 8.314\ T; \quad \bar{P} = P + \frac{a}{V^2}\ , \ \bar{V} = V - b \qquad (1)$$

where P, V and T denote pressure, volume and temperature, a denotes attractive force between molecules, and b the minimum volume to which a mole of gas can be compressed. One of Friedman's versions of the quantity theory of money is

$$P\ \bar{Y} = kM, \qquad \bar{Y} = [\frac{\lambda}{D+\lambda}\ Y]^{\beta} \qquad (2)$$

where P denotes the general price level, Y national income and M the quantity of money. In (1) 8.314 is the universal gas constant, and a and b are specific constants for a particular gases. But in (2), neither k, the velocity of money, nor β, the apparent income elasticity of demand for real money balances, nor λ, the "time constant" for the exponential lag of measured income, is constant over space or time.

These matters are of more concern to theorists, whose primary interest is in explanation, than to applied economists, who are interested in short-term forecasting and control of an economy, or some segment of it.

Economic models, although still quite imperfect, have sometimes proved useful for policy purposes. And, if the passage of time meant only that the parameters of a more general model underwent change, that would not deprive it of value as a qualitative explanation. The log-normal distribution has had such value as an explanation of income inequalities, and so, despite theoretical weaknesses, has had the Cobb-Douglas function in regard to factor productivities. Friedman, of course, makes a similar claim (not generally accepted) for the quantity theory of money.

Such examples show that, if we are still groping, we are not groping in the dark; and, if we have the heart to go on groping, it seems best to grope in the way which Popper holds is the method of science. Physics, itself, is said to have a crisis in regard to quantum mechanics, and perhaps also in regard to cosmology. Even in this great science final truth has not been reached nor, according to Popper, could it ever be reached.

Specific differences between natural sciences and economics over conduct of experiments and use of quantitative methods are, he holds, differences of degree rather than kind. In one respect, moreover, he finds economics to be less complicated than physics (contrary to the opinion expressed by Planck to Keynes).

> 'For in most, if not all, social situations there is an element of rationality, which makes possible the construction of a model on the assumption of complete rationality (and perhaps also on the assumption of the possession of complete information) on the part of all individuals concerned, and of estimating the deviation of the actual behaviour of people from the model behaviour, using the latter as a kind of zero coordinate.'[38]

He also speaks of a 'situational logic' or 'situational analysis' to deduce limitations put on choice by objective problem situations.[39]

His prime example of the 'zero method' is the 'logic of choice' as exemplified in economic theories of consumption and production. Hicks, who has done much to advance this logic of choice, emphasizes its dependence on 'the maximization postulate', and now defends this in a way which is akin to Popper's ideas of a zero method and situational analysis. In regard to consumption, for example, he thinks that the postulate helps 'in separating out the current-price explanations' from non-economic causes. He also points out that some apparent qualifications to the postulate can be brought within its scope, for example, in production theory profit could be defined to include degree of freedom from managerial strain. 'The constraints may be internal to the chooser, as well as external to him.'[40]

It is of some interest to note that although, as Popper says, there is no logic of choice in natural science, it did have a role in the pre-scientific era of what Hicks calls the 'Old Causality'.[41] This attributed causes of events to actions of superhuman or even human agents. Nor does modern science, - the 'New Causality' - dispense with an optimization postulate.[42] Voltaire's enemy, Maupertuis, formulated a 'principle of least action' which stated that nature always minimized a product of mass, velocity and distance travelled. It enabled him to unify laws of mechanics and light, and won support from Euler. Last century, Hamilton unified most laws relating to gravitation, optics, dynamics and electricity by maximizing or minimizing a 'time integral of kinetic potential'; and this century Einstein used a similar principle for relativity theory such that bodies in space-time move so as to maximize a function called 'the interval'. This suggests that optimization postulates can have power to raise the level of universality of economic theories, even if heroic work on general competitive equilibrium has so far had much less than complete success.

It is of more interest to compare the interpretations of Hicks and Popper in regard to Newton's laws of motion because Hicks has attempted to use them for a defence of inductivism - which is anathema to Popper. Can, Hicks[43] asks, empirical association be brought together with logical implication? There is, he answers, plenty of evidence in economics and science that it can. Did not Newton himself declare, in relation to his inverse square law of gravitational attraction, 'hypotheses non fingo'? Hicks asserts that Newton reached this law from acquaintance with Galileo's inductive work on projectiles which had shown they had a vertical component of downward acceleration. To this finding Newton applied mathematics so as to deduce the laws of planetary motion which Kepler had also found inductively. Newtonian mechanics thus, started from 'a relation that had emerged from the facts', and provided a 'logical bridge' to other inductions. And it gained such 'inductive strength' from later applications and observations as to reach 'the nearest thing to "proof" which any theory ... can ever have'.

This emphasis on induction is quite at odds with Popper's views about scientific laws, summarized in III above. But it may be enough here to give Popper's view of Newton's discovery, and of his own assertion that he had reached it by induction;[44] - this assertion is historically and logically false. Popper's historical conjecture differs from that of Hicks by supposing Newton's starting point to have been Kepler's laws, not Galileo's law of projectiles. He argues that Kepler, after testing the Copernician idea of planets revolving in circles about the sun against Brahe's astronomical records, had to reject it as false, and then turned to eliptical motion as an explanation of these records, partly because of astrological ideas of influences coming from the stars.

More important is the demonstration of logical falsity, which owes much to Hume and Kant. Let K be a class of true statements about past observations, including those made by Galileo; and let B be a class of logically possible statements about future observations. Hume had shown, in effect, that no contradiction results from conjoining B with K. It then follows, as a matter of pure logic, that no statement in B can contradict K together with any statements that are derivable from K, including Newton's theory and all predictions made from it. That is an absurd conclusion; Newton's theory could predict a solar eclipse on a particular day, and it is conceivable that this eclipse would not occur. The absurdity is removed only if we give up the idea that Newton's theory can be derived from observations. As Kant saw, such a theory transcends all observations because it is universal in form, and an interpretation of observations, not their logical consequence.[45]

Footnotes:

1. Science and Method (Dover edition, 1952), pp. 18-19
2. Classics and Moderns (1983), ch. 32
3. The Poverty of Historicism (1947), pp. 130-31
4. Causality in Economics (1979), Ch. VIII
5. Classics and Moderns (1983), p. 371
6. Realism and the Aim of Science (1983), p. 296. See also The Logic of Scientific Discovery (1959), Ch. VIII
7. The Logic of Scientific Discovery (1959), p. 33
8. Causality in Economics, pp. 105-06
9. W. Feller, An Introduction to Probability Theory and its Applications (1950), p. 27
10. A. Church later improved this criterion; see Realism and the Aim of Science, Pt II, s.21. Popper had also improved the relative frequency interpretation, but later developed a preferred propensity interpretation; see op.cit., Pt II, Ch. III
11. Feller, op.cit., p. 118
12. Realism and the Aim of Science, p. 300
13. A Treatise on Probability (1921), pp. 342 f
14. Objective Knowledge (1972), p. 356
15. The Open Universe (1982), p. 125
16. idem, pp. 129-30
17. Quantum Theory and the Schism in Physics (1982), p. 70
18. Classics and Moderns, p. 372
19. Statistical Methods of Econometrics (1964), p. 614
20. The Logic of Scientific Discovery, pp. 205-06
21. Classics and Moderns, p. 372
22. idem, p. 371
23. A. Tarski, An Introduction to Logic (1941), p. 73

24. The Logic of Scientific Discovery, s.16

25. The Open Universe, p. 162

26. The Logic of Scientific Discovery, s.12; The Open Society (1945), p. 342

27. The Logic of Scientific Discovery, Ch. III, esp. p. 76

28. idem, Ch. IV

29. Objective Knowledge, p. 71; Realism and the Aim of Science, p. 108

30. The Logic of Scientific Discovery, p. 111

31. Causality in Economics, Ch. II

32. Causality in Economics, p. 22

33. The Logic of Scientific Discovery, s.18

34. Realism and the Aim of Science, p. 288

35. Causality in Economics, p. 120

36. See, for example, W. Feller, op.cit., Ch. I

37. The Poverty of Historicism, pp. 142-43

38. The Poverty of Historicism, p. 141

39. Objective Knowledge, p. 178

40. Classics and Moderns, p. 371

41. Causality in Economics, p. 6

42. see M. West, Mathematics in Western Culture (1953), pp. 261-64 of Penguin edition (1972)

43. Causality in Economics, pp. 28-34

44. Conjectures and Refutations (1963), Ch. 8

45. Yet the inductivist view is widespread and influential, usually based on some kind of subjectivist view of probability and the appeal of what Popper calls a 'simple inductive rule'. He explains and criticizes these ideas in Realism and the Aim of Science, Part II, Ch. II. See also his joint paper with David Miller, 'A proof of the impossibility of inductive probability', Nature, 21 April 1983, pp. 687-88

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