THE POVERTY OF HISTORICISM

By

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trend, and conditions under which it would disappear are to him unthinkable. The poverty of historicism, we might say, is a poverty of imagination. The historicist continuously upbraids those who cannot imagine a change in their little worlds; yet it seems that the historicist is himself deficient in imagination, for he cannot imagine a change in the conditions of change.

29 THE UNITY OF METHOD

I suggested in the foregoing section that the deductive methods there analyzed are widely used and important —more so than Mill, for example, ever thought. This suggestion will now be further elaborated, in order to throw some light on the dispute between naturalism and anti-naturalism. In this section I am going to propose a doctrine of the unity of method; that is to say, the view that all theoretical or generalizing sciences make use of the same method, whether they are natural sciences or social sciences. (I postpone the discussion of the historical sciences until section 31.) At the same time, some of these doctrines of historicism which I have not yet sufficiently examined will be touched upon, such as the problems of Generalization; of Essentialism; of the role played by Intuitive Understanding; of the Inexactitude of Prediction; of Complexity; and of the application of Quantitative Methods.

I do not intend to assert that there are no differences whatever between the methods of the theoretical sciences of nature and of society; such differences clearly exist, even between the various natural sciences themselves, as well as between the various social sciences. (Compare, for example, the analysis of competitive markets and of Romance languages.) But I agree with Comte and Mill—and with many others, such as

C. Menger—that the methods in the two fields are fundamentally the same (though the methods I have in mind may differ from those they had in mind). The methods always consist in offering deductive causal explanations, and in testing them (by way of predictions). This has sometimes been called the hypothetical-deductive method,[1] or more often the method of hypothesis, for it does not achieve absolute certainty for any of the scientific statements which it tests; rather, these statements always retain the character of tentative hypotheses, even though their character of tentativeness may cease to be obvious after they have passed a great number of severe tests.

Because of their tentative or provisional character, hypotheses were considered, by most students of method, as *provisional in the sense that they have ultimately to be replaced by proved theories* (or at least by theories which can be proved to be 'highly probable', in the sense of some calculus of probabilities). I believe that this view is mistaken and that it leads to a host of entirely unnecessary difficulties. But this problem[2] is

[1] See V. Kraft, *Die Grundformen der wissenschaftlichen Methoden* (1925).

[2] See my *Logic of Scientific Discovery*, on which the present section is based, especially the doctrine of tests by way of deduction ('deductivism') and of the redundancy of any further 'induction', since theories always retain their hypothetical character ('hypotheticism'), and the doctrine that scientific tests are genuine attempts to falsify theories ('eliminationism'); see also the discussion of testability and falsifiability.

The opposition here pointed out, between *deductivism* and *inductivism*, corresponds in some respects to the classical distinction between *rationalism* and *empiricism*: Descartes was a deductivist, since he conceived all sciences as deductive systems, while the English empiricists, from Bacon on, all conceived the sciences as collecting observations from which generalizations are obtained by induction.

But Descartes believed that the principles, the premises of the deductive systems, must be secure and self-evident—'clear and distinct'. They are based upon the insight of reason. (They are synthetic and *a priori* valid, in Kantian language.) As opposed to this, I conceive them as tentative conjectures, or hypotheses.

These hypotheses, I contend, must be refutable in principle: it is of comparatively little moment here. What is important is to realize that in science we are always concerned with explanations, predictions, and tests, and that the method of testing hypotheses is always the same (see the foregoing section). From the hypothesis to be tested—for example, a universal law—together with some other statements which for this purpose are not considered as problematic—for example, some initial conditions—we deduce some prognosis. We then confront this prognosis, whenever possible, with the

here that I deviate from the two greatest modern deductivists, Henri Poincaré and Pierre Duhem.

Poincaré and Duhem both recognized the impossibility of conceiving the theories of physics as inductive generalizations. They realized that the observational measurements which form the alleged starting point for the generalizations are, on the contrary, *interpretations in the light of theories*. And they rejected not only inductivism, but also the rationalistic belief in synthetic *a priori* valid principles or axioms. Poincaré interpreted them as analytically true, as definitions; Duhem interpreted them as instruments (as did Cardinal Bellarmino and Bishop Berkeley), as means for the ordering of the experimental laws—the experimental laws which, he thought, were obtained by induction. Theories thus cannot contain either true or false information: they are nothing but instruments, since they can only be convenient or inconvenient, economical or uneconomical; supple and subtle, or else creaking and crude. (Thus, Duhem says, following Berkeley, there cannot be logical reasons why two or more theories which contradict one another should not all be accepted.) I fully agree with both these great authors in rejecting inductivism as well as the belief in the synthetic *a priori* validity of physical theories. But I cannot accept their view that it is impossible to submit theoretical systems to empirical tests. Some of them are testable, I think; that is, refutable in principle; and they are therefore synthetic (rather than analytic); *empirical* (rather than *a priori*); and *informative* (rather than purely instrumental). As to Duhem's famous criticism of crucial experiments, he only shows that crucial experiments can never *prove* or establish a theory; but he nowhere shows that crucial experiments cannot *refute* a theory. Admittedly, Duhem is right when he says that we can test only huge and complex theoretical systems rather than isolated hypotheses; but if we test two such systems which differ in one hypothesis only, and if we can design experiments which refute the first system while leaving the second very well corroborated, then we may be on reasonably safe ground if we attribute the failure of the first system to that hypothesis in which it differs from the other.

results of experimental or other observations. Agreement with them is taken as corroboration of the hypothesis, though not as final proof; clear disagreement is considered as refutation or falsification.

According to this analysis, there is no great difference between explanation, prediction and testing. The difference is not one of logical structure, but rather one of emphasis; it depends on *what we consider to be our problem* and what we do not so consider. If it is not our problem to find a prognosis, while we take it to be our problem to find the initial conditions or some of the universal laws (or both) from which we may deduce a *given* 'prognosis', then we are looking for an *explanation* (and the given 'prognosis' becomes our 'explicandum'). If we consider the laws and initial conditions as given (rather than as to be found) and use them merely for deducing the prognosis, in order to get thereby some new information, then we are trying to make a *prediction*. (This is a case in which we *apply* our scientific results.) And if we consider one of the premises, i.e. either a universal law or an initial condition, as problematic, and the prognosis as something to be compared with the results of experience, then we speak of a *test* of the problematic premise.

The result of tests is the *selection* of hypotheses which have stood up to tests, or the *elimination* of those hypotheses which have not stood up to them, and which are therefore rejected. It is important to realize the consequences of this view. They are these: all tests can be interpreted as attempts to weed out false theories—to find the weak points of a theory in order to reject it if it is falsified by the test. This view is sometimes considered paradoxical; our aim, it is said, is to establish theories, not to eliminate false ones. But just because it is our aim to establish theories as well as we can, we must test them as severely as we can; that is, we must try to find fault with them, we must try to falsify them. Only if we cannot falsify them in spite of our best efforts can we say that they have stood up to severe tests. This is the reason why the discovery of instances which confirm a theory means very little if we have not tried, and failed, to discover refutations. For if we are uncritical we shall always find what we want: we shall look for, and find, confirmations, and we shall look away from, and not see, whatever might be dangerous to our pet theories. In this way it is only too easy to obtain what appears to be overwhelming evidence in favour of a theory which, if approached critically, would have been refuted. In order to make the method of selection by elimination work, and to ensure that only the fittest theories survive, their struggle for life must be made severe for them.

This, in outline, is the method of all sciences which are backed by experience. But what about the method by which we *obtain* our theories or hypotheses? What about *inductive generalizations*, and the way in which we proceed from observation to theory? To this question (and to the doctrines discussed in section 1, so far as they have not been dealt with in section 26) I shall give two answers. (*a*) I do not believe that we ever make inductive generalizations in the sense that we start with observations and try to derive our theories from them. I believe that the prejudice that we proceed in this way is a kind of optical illusion, and that at no stage of scientific development do we begin without something in the nature of a theory, such as a hypothesis, or a prejudice, or a problem—often a technological one—which in some way *guides* our observations, and helps us to select from the innumerable objects of observation those which may be of interest.[1] But if

[1] For a surprising example of the way in which even botanical observations are guided by theory (and in which they may be even

this is so, then the method of elimination—which is nothing but that of trial and error discussed in section 24—can always be applied. However, I do not think that it is necessary for our present discussion to insist upon this point. For we can say (*b*) that it is irrelevant from the point of view of science whether we have obtained our theories by jumping to unwarranted conclusions or merely by stumbling over them (that is, by 'intuition'), or else by some inductive procedure. The question, 'How did you first *find* your theory?' relates, as it were, to an entirely private matter, as opposed to the question, 'How did you *test* your theory?' which alone is scientifically relevant. And the method of testing described here is fertile; it leads to new observations, and to a mutual give and take between theory and observation.

Now all this, I believe, is not only true for the natural but also for the social sciences. And in the social sciences it is even more obvious than in the natural sciences that we cannot see and observe our objects before we have thought about them. For most of the objects of social science, if not all of them, are abstract objects; they are *theoretical* constructions. (Even 'the war' or 'the army' are abstract concepts, strange as this may sound to some. What is concrete is the many who are killed; or the men and women in uniform, etc.) These objects, these theoretical constructions used to interpret our experience, are the result of constructing certain *models* (especially of institutions), in order to explain certain experiences—a familiar theoretical method in the natural sciences (where we construct our models of atoms, molecules, solids, liquids, etc.). It is part of the method of explanation by way of reduction, or deduction from

influenced by prejudice), see O. Frankel, 'Cytology and Taxonomy of Hebe, etc.', in *Nature*, vol. 147 (1941), p. 117.

hypotheses. Very often we are unaware of the fact that we are operating with hypotheses or theories, and we therefore mistake our theoretical models for concrete things. This is a kind of mistake which is only too common.[1] The fact that models are often used in this way explains—and by so doing destroys—the doctrines of methodological essentialism (cp. section 10). It explains them, for the model is abstract or theoretical in character, and so we are liable to feel that we see it, either within or behind the changing observable events, as a kind of permanent ghost or essence. And it destroys them because the task of social theory is to construct and to analyse our sociological models carefully in descriptive or nominalist terms, that is to say, *in terms of individuals*, of their attitudes, expectations, relations, etc.—a postulate which may be called 'methodological individualism'.

The unity of the methods of the natural and social sciences may be illustrated and defended by an analysis of two passages from Professor Hayek's *Scientism and the Study of Society*.[2]

In the first of these passages, Professor Hayek writes:

'The physicist who wishes to understand the problems of the social sciences with the help of an analogy from his own field would have to imagine a world in which he knew by direct observation the inside of the atoms and had neither the possibility of making experiments with lumps of matter nor the opportunity to observe more than the interactions of a comparatively few atoms during a limited period. From his knowledge of the different kinds of atoms he could

[1] With this and the following paragraph, cp. F. A. von Hayek, 'Scientism and the Study of Society', parts I and II, *Economica*, vols. ix and x, where methodological collectivism is criticized and where methodological individualism is discussed in detail.

[2] For the two passages see *Economica*, vol. ix, p. 289 f. (italics mine).

build up models of all the various ways in which they could combine into larger units and make these models more and more closely reproduce all the features of the few instances in which he was able to observe more complex phenomena. But the laws of the macrocosm which he could derive from his knowledge of the microcosm would always remain "*deductive*"; they would, because of his limited knowledge of the data of the complex situation, scarcely ever enable him to predict the precise outcome of a particular situation; and he could never verify them by controlled experiment—although they might be *disproved* by the observation of events which according to his theory are impossible.'

I admit that the first sentence of this passage points to certain differences between social and physical science. But the rest of the passage, I believe, speaks for a complete *unity of method*. For if, as I do not doubt, this is a correct description of the method of social science, then it shows that it differs only from such interpretations of the method of natural science as we have already rejected. I have in mind, more especially, the 'inductivist' interpretation which holds that in the natural sciences we proceed systematically from observation to theory by some method of generalization, and that we can 'verify', or perhaps even prove, our theories by some method of induction. I have been advocating a very different view here—an interpretation of scientific method as deductive, hypothetical, selective by way of falsification, etc. And this description of the method of natural science agrees perfectly with Professor Hayek's description of the method of social science. (I have every reason to believe that my interpretation of the methods of science was not influenced by any knowledge of the methods of the social sciences; for when I developed it first, I had only the natural sciences in mind,[1] and I knew next to nothing about the social sciences.)

But even the differences alluded to in the first sentence of the quotation are not so great as may appear at first sight. It is undoubtedly true that we have a more direct knowledge of the 'inside of the human atom' than we have of physical atoms; but this knowledge is intuitive. In other words, we certainly use our knowledge of ourselves in order to frame *hypotheses* about some other people, or about all people. But these hypotheses must be tested, they must be submitted to the method of selection by elimination. (Intuition prevents some people from even imagining that anybody could possibly dislike chocolate.) The physicist, it is true, is not helped by such direct observation when he frames his hypotheses about atoms; nevertheless, he quite often uses some kind of sympathetic imagination or intuition which may easily make him feel that he is intimately acquainted with even the 'inside of the atoms'—with even their whims and prejudices. But this intuition is his private affair. Science is interested only in the hypotheses which his intuitions may have inspired, and then only if these are rich in consequences, and if they can be properly tested. (For the other difference mentioned in Professor Hayek's first sentence, i.e. the difficulty of conducting experiments, see section 24.)

These few remarks may also indicate the way in which the historicist doctrine expounded in section 8 should be criticized—that is to say, the doctrine that social science must use the method of intuitive understanding.

In the second passage, Professor Hayek, speaking of

[1] Cp. *Erkenntnis*, III, p. 426 f., and my *Logik der Forschung*, 1934, whose sub-title may be translated: 'On the Epistemology of the Natural Sciences'.

social phenomena, says: '. . . our knowledge of the principle by which these phenomena are produced will rarely if ever enable us to predict the precise result of any *concrete* situation. While we can explain the principle on which certain phenomena are produced and can from this knowledge *exclude the possibility of certain results*, e.g. of certain events occurring together, our knowledge will in a sense be only negative, i.e. it will merely enable us to preclude certain results but not enable us to narrow the range of possibilities sufficiently so that only one remains'.

This passage, far from describing a situation peculiar to the social sciences, perfectly describes the character of natural laws which, indeed, can never do more than *exclude certain possibilities*. ('You cannot carry water in a sieve'; see section 20, above.) More especially the statement that we shall not, as a rule, be able 'to predict the precise result of any *concrete* situation' opens up the problem of the inexactitude of prediction (see section 5, above). I contend that precisely the same may be said of the concrete physical world. In general it is only by the use of artificial experimental isolation that we can predict physical events. (The solar system is an exceptional case—one of natural, not of artificial isolation; once its isolation is destroyed by the intrusion of a foreign body of sufficient size, all our forecasts are liable to break down.) We are very far from being able to predict, even in physics, the precise results of a *concrete* situation, such as a thunderstorm, or a fire.

A very brief remark may be added here on the problem of complexity (see section 4, above). There is no doubt that the analysis of any concrete social situation is made extremely difficult by its complexity. But the same holds for any concrete physical situation.[1] The

[1] A somewhat similar argument can be found in C. Menger, *Collected Works*, vol. II (1883 and 1933), pp. 259–60.

widely held prejudice that social situations are more complex than physical ones seems to arise from two sources. One of them is that we are liable to compare what should not be compared; I mean on the one hand concrete social situations and on the other hand artificially insulated experimental physical situations. (The latter might be compared, rather, with an artificially insulated social situation—such as a prison, or an experimental community.) The other source is the old belief that the description of a social situation should involve the mental and perhaps even physical states of everybody concerned (or perhaps that it should even be reducible to them). But this belief is not justified; it is much less justified even than the impossible demand that the description of a concrete chemical reaction should involve that of the atomic and sub-atomic states of all the elementary particles involved (although chemistry may indeed be reducible to physics). The belief also shows traces of the popular view that social entities such as institutions or associations are concrete natural entities such as crowds of men, rather than abstract models constructed to interpret certain selected abstract relations between individuals.

But in fact, there are good reasons, not only for the belief that social science is less complicated than physics, but also for the belief that concrete social situations are in general less complicated than concrete physical situations. For in most social situations, if not in all, there is an element of *rationality*. Admittedly, human beings hardly ever act quite rationally (i.e. as they would if they could make the optimal use of all available information for the attainment of whatever ends they may have), but they act, none the less, more or less rationally; and this makes it possible to construct comparatively simple models of their

actions and inter-actions, and to use these models as approximations.

The last point seems to me, indeed, to indicate a considerable difference between the natural and the social sciences—perhaps *the most important difference in their methods*, since the other important differences, i.e. specific difficulties in conducting experiments (see end of section 24) and in applying quantitative methods (see below), are differences of degree rather than of kind. I refer to the possibility of adopting, in the social sciences, what may be called the method of logical or rational construction, or perhaps the 'zero method'.[1] By this I mean the method of constructing 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 the 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 co-ordinate.[2] An example of this method is the comparison between actual behaviour (under the influence of, say, traditional prejudice, etc.) and model behaviour to be expected on the basis of the 'pure logic of choice', as described by the equations of economics. Marschak's interesting 'Money Illusion', for example, may be interpreted in this way.[3] An attempt at applying the zero method to a different field may be found in P. Sargant Florence's

[1] See the 'null hypothesis' discussed in J. Marschak, 'Money Illusion and Demand Analysis', in *The Review of Economic Statistics*, vol. XXV, p. 40.—The method described here seems partly to coincide with what has been called by Professor Hayek, following C. Menger, the 'compositive' method.

[2] Even here it may be said, perhaps, that the use of rational or 'logical' models in the social sciences, or of the 'zero method', has some vague parallel in the natural sciences, especially in thermodynamics and in biology (the construction of mechanical models, and of physiological models of processes and of organs). (Cp. also the use of variational methods.)

[3] See J. Marschak, *op. cit.*

comparison between the 'logic of large-scale operation' in industry and the 'illogic of actual operation'.[1]

In passing I should like to mention that neither the principle of methodological individualism, nor that of the zero method of constructing rational models, implies in my opinion the adoption of a psychological method. On the contrary, I believe that these principles can be combined with the view [2] that the social sciences are comparatively independent of psychological assumptions, and that psychology can be treated, not as the basis of all social sciences, but as one social science among others.

In concluding this section, I have to mention what I consider to be the other main difference between the methods of some of the theoretical sciences of nature and of society. I mean the specific difficulties connected with the application of quantitative methods, and especially methods of measurement.[3] Some of these difficulties can be, and have been, overcome by the application of statistical methods, for example in demand analysis. And they *have to be overcome* if, for example, some of the equations of mathematical economics are to provide a basis even of merely qualitative applications; for without such measurement we should often not know whether or not some counteracting influences exceeded an effect calculated in merely qualitative terms. Thus merely qualitative considerations may well be deceptive at times; just as deceptive, to quote Professor Frisch, 'as to say that when a man tries to row a boat forward, the boat will be driven backward because of the pressure exerted by his feet'.[4] But it cannot be doubted that there are

[1] See P. Sargant Florence, *The Logic of Industrial Organisations (1933)*.
[2] This view is more fully developed in ch. 14 of my *Open Society*.
[3] These difficulties are discussed by Professor Hayek, *op. cit.*, p. 290 f.
[4] See *Econometrica*, I (1933), p. 1 f.

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 the parameters are themselves in the most important cases quickly changing variables.[1] This clearly reduces the significance, interpretability, and testability of our measurements.

30 THEORETICAL AND HISTORICAL SCIENCES

The thesis of the unity of scientific method, whose application to theoretical sciences I have just been defending, can be extended, with certain limitations, even to the field of the historical sciences. And this can be done without giving up the fundamental distinction between theoretical and historical sciences—for example, between sociology or economic theory or political theory on the one hand, and social, economic, and political history on the other—a distinction which has been so often and emphatically reaffirmed by the best historians. It is the distinction between the interest in universal laws and the interest in particular facts. I wish to defend the view, so often attacked as old-fashioned by historicists, that *history is characterized by its interest in actual, singular, or specific events, rather than in laws or generalizations.*

This view is perfectly compatible with the analysis of scientific method, and especially of causal explanation, given in the preceding sections. The situation is simply this: while the theoretical sciences are mainly interested in finding and testing universal laws, the

[1] See Lionel Robbins, in *Economica*, vol. V, especially p. 351.