FORCES and FIELDS

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Forces and Fields

The concept of Action at a Distance in the history of physics

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Preface

In this book I have traced through the history of physics some of the problems clustering round the question: 'How do bodies act on one another across space?', and I have used the various answers to this question to illustrate the role of fundamental analogies or models in physics, and the ways in which so-called unobservable entities are introduced into it. It has not been my purpose to write an exhaustive history of the subject—this is in any case an unrealisable goal, and the attempt to reach it has had the result too often in the historiography of science of obscuring the principles of historical selection which are always inevitably present. I have selected for most detailed discussion those periods of transition in fundamental physics in which new concepts and ideas have been introduced and made scientifically testable. I have also thought it desirable to make a certain philosophical interpretation of science explicit at the beginning, for this interpretation has no doubt affected both my selection of historical material and my comments upon it. This is after all an acceptably scientific procedure: to state a theory about the nature of science, which is tested by reference to specific historical situations, and then if necessary to modify the theory in the light of the tests. But one thesis which will be argued here with respect to science itself is also assumed to hold in respect to the history of science, namely, that there are no bare and uninterpreted facts; all facts, whether experimental or historical, are interpreted in the light of some theory. In writing the history of science there will always be present, either implicitly or explicitly, some philosophical view of the nature of science. The first chapter, in which the philosophical view adopted here is developed, is however somewhat more technical than the rest of the book, and a reader whose main interest is historical will find that he can omit the details without affecting his understanding of what follows.

The history of a particular theoretical idea, in which the emphasis is rather upon theory-constructing than upon theory-testing, fortunately lends itself to non-technical presentation. The detailed examination of how and why a certain theory is confirmed or refuted by experimental tests is inevitably somewhat technical, and becomes more so in later historical periods for the more advanced sciences. The fact that it has not been possible or necessary here to enter into details about either mathematical or experimental technicalities has inevitably distorted the over-all picture of physics presented, but there are after all many books devoted to this aspect of the history of science, and still too few which trace the history of scientific ideas and their relations with philosophy. There is therefore some justification for trying to redress the balance even at the risk of over-compensation.

It may be felt that, for a historical study of physics, a disproportionate amount of space has been given to pre-seventeenth-century work. Aristotle may have written a book called the *Physics*, but many histories of physics devote all but a brief introductory chapter to the period beginning with Gilbert and Galileo, and do not appear to lose much thereby. But here again the periods selected depend on one's purpose, and where different fundamental theories and the relations between them are to be discussed, those periods which were failures and cul-de-sacs from the point of view of twentieth-century physics may be as important as the cumulative successes of the past three hundred years. There are now signs in physics that the time is ripe for another major transition of fundamental ideas, and if this occurs it may be that the confident physicalism of the modern period will also be seen by future generations to have led in the end to a cul-de-sac, and this in spite of the numerous technical achievements accomplished on the way. In that case we may hope that twentieth-century physics will be spared the judgments of historians whose criteria are wholly in terms of present success.

There is another reason why I have devoted a considerable amount of space to the Greeks, and have made my account of their science somewhat more comprehensive than the accounts of later periods. There are not yet many general histories of science which incorporate those studies of the Pre-Socratics initiated by Cornford and continued by contemporary classical historians. Burnet's interpretation of the early Greek philosophers as forerunners of later science was no doubt sympathetic to the inductivist view of science then current, in which science was seen as a linear progress towards the 'correct' ideas, but many of Burnet's interpretations are now discredited among classicists, and it seems right to try to put this whole most significant period in new perspective in relation to a different view of the nature of scientific advance. I have also tried to bring out the debt which the seventeenth-century natural philosophers owed to Greek ideas, for in spite of their emotionally charged polemics, they did not in fact erase everything and start again with a clean slate, but proceeded in the fashion of all later science, by criticism, testing, and modification of received ideas.

No-one could claim a specialist knowledge of the history, philosophy and science of all the periods which I have discussed in this book. But some of the effects of my shortcomings in this respect have certainly been mitigated by the generous help I have received from colleagues and friends, and I should like to acknowledge my debt to them, but without saddling them with any responsibility for the views the book now contains. I should like in particular to thank Professor H. Dingle for his encouragement, and also Dr A. Armitage, Mr D. J. Furley, Dr N. H. de V. Heathcote, Dr H. R. Post, Dr J. R. Ravetz, and Dr G. J. Whitrow, all of whom read individual chapters in manuscript. I should also like to express my debt to Dr J. Agassi for conversations and for his unpublished thesis 'The Function of Interpretations in Science' from which I have learnt to disentangle myself from some stereotyped and false interpretations of the history of science, and to Dr P. W. Higgs for advice relating to quantum field theory. The English edition of Professor Popper's *Logic of Scientific Discovery* appeared too late for me to make detailed reference to it, but my debt to the point of view initiated by the first German edition of 1934, and since expanded in Professor Popper's other writings, will be obvious. In order to avoid a multiplicity of footnotes, I have not made detailed reference to secondary historical sources in the text. I hope, however, that my great debt to them is adequately acknowledged in the Bibliography.

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Chapter I

THE LOGICAL STATUS OF THEORIES

Physics and Epistemology

DURING the last hundred years, the question of the meaning and logical status of scientific theories has become acute. Whereas it had become the fashion since the disputes of the seventeenth century for scientists to regard philosophy as at best irrelevant and at worst a hindrance to their work, it has now become common to find physicists making philosophical judgments about their theories, and terms like 'reality', 'epistemology', 'mental construction' appear in serious expositions of modern physics, and even invade the antiseptic atmosphere of scientific journals.

The immediate cause of this ferment has been the need to understand and interpret the revolutionary developments in physics since the end of the nineteenth century, and the problem has been to make sense of the apparently paradoxical statements which physicists have been led to make in formulating the new theories. But the deeper problem has been concerned with the nature of scientific theories themselves. This is by no means a new problem, and it is relevant to all kinds of theories, whether they appear paradoxical or not, and whether they are controversial or well-established. It is, however, a problem which is liable to break out with renewed vigour during periods of radical scientific change, and when scientific results appear to contradict traditional ideas.

Throughout the history of science, one can trace the influence of two contrasting accounts of what scientific theories are, what kind of information they give about the world, and, closely connected with this, what is the best procedure to follow in developing them. The well-known razor of William of Ockham is a classic example of the empirical, positivist approach, which forbids the postulation of entities without necessity, and the occasion of it was a realist natural philosophy deriving from Aristotle, in which were postulated hidden causes such as undetectable forces causing motion, and invisible *species* conveying radiation. Another example occurs at the beginning of the modern scientific movement, when Copernicus and Galileo were both urged by well-meaning interpreters to adopt a positivist position regarding the motion of the earth: to claim, that is, only the convenience of a calculating device for the heliocentric system, not that it represented a true account of the structure of the world. Here, as with Ockham earlier and Kant later, part of the purpose of distinguishing sharply between the immediate empirical data and theories which might be built upon them, and of denying that such theories have any descriptive value, was to confine the authority of science to the immediately observed, and leave the province of the unobserved to theology, or metaphysics, or whatever else was believed to have access to non-empirical facts.

Newton's view of science, at least in his non-speculative writings and as expressed in the famous 'hypotheses non fingo', is also in the positivist tradition, since he wished to confine theories to what could be *deduced* from phenomena: 'whatever is not deduced from the phenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy'.[1] Newton did not however draw the conclusion that the heliocentric system was a calculating device; for him it was the 'true system of the world', because he regarded Kepler's laws as being deduced from phenomena and 'rendered general by induction', and his own gravitational theory as being a further inference of the same kind. And he, unlike most positivists, was not afraid to speculate as to possible 'hidden causes', although he always did so with apologies and with the implied hope that such causes would eventually be found to be deducible from phenomena, and hence not hypothetical.

The general characteristic of a positivist view of science is suspicion and caution with regard to the claims of theories, and this view therefore always assumes importance as a possible solution of difficulties which arise when the theories to which science seems to be committed are in conflict with common-sense (as with Newton's apparent action at a distance), or traditional belief (as with the motion of the earth), or are unduly complex or apparently self-contradictory (as with the incompatible properties of the nineteenth-century aether or twentieth-century fundamental particles). The suggested solution along positivist lines is then to assert that scientific theories which go beyond immediate experience are mere mental constructions, tools for correlating and predicting the results of possible experiments, and not descriptions of physical reality.

[1] *Philosophiae Naturalis Principia Mathematica* (2nd ed., London, 1713), III, General Scholium to Prop. XLII; ed. of F. Cajori, Berkeley, 1947, p. 547

This was the line taken by Berkeley and Kant in relation to Newtonian science, by Mach, Pearson and Duhem in relation to nineteenth-century physics, and by many leading exponents of modern physics, where, as we shall see, the issue is confused by special difficulties.

The two traditional views of scientific theories must now be described more precisely. I shall continue to refer to them as the 'realist' and 'positivist' views respectively, and after discussing their characteristics I shall go on to try to show that neither is adequate as an account of science, and to develop with the aid of recent discussions in the philosophy of science a modified realist view which accords more closely with the actual procedures of scientists.[1]

A Realist View of Theories

The realist view holds that scientific theories are in a straightforward sense literal descriptions of nature. This view is easily and almost unconsciously adopted in the early stages of a science, when the statements that appear in it are hardly more than direct descriptions of observations and the results of experiments, using a minimum of technical expression. Examples would be the description of observed positions of a planet, or the optical properties of mirrors or refracting media. When theories become more complex, however, it seems that some statements are being made which are not subject to immediate test in this way, that is, they are not 'observation statements', because, for example, they may be about events inaccessible in space or time, or on too small a scale to be observed with existing scientific apparatus. The realist account asserts that these difficulties in the way of observational tests are merely accidental, and that such theories are still to be regarded as literal descriptions of entities existing in nature, and to be understood in exactly the same way as they would be understood if they were describing observations. In support of this, the

[1] In 'Three Views concerning Human Knowledge' (*Contemporary British Philosophy*, ed. H. D. Lewis (London, 1956, p. 357) Professor Popper has discussed two views of theories which he names 'essentialist' and 'instrumentalist'. The former does not correspond to what is meant here by 'realist', for, according to Popper, essentialists believe that the purpose of science is to discover essences which are final and not subject to correction by future experience. By instrumentalism he means the thesis that theories are computation rules, and in criticising this view he is interested not so much in the problem of meaning, as in examining what scientists in fact do with theories, and showing that this is incompatible with the belief that they are merely rules. His conclusion, that theories are intended to describe the real, although the scientist never knows whether they are in fact true descriptions, is similar to that we shall reach below on somewhat different grounds.

realist might point out that we *never* restrict the meaning of our statements to what is actually observed; we are confident, for instance, that physical objects remain in existence when we are not looking at them, and it may be argued that the difference between this and the assumption on indirect grounds that there are entities too small or too distant to be observed is one of degree and not of kind.

Consider for example the dynamical theory of gases. This describes gases as being made up of particles moving at random in a containing vessel, and from the mechanics of such particles the properties of gases are deduced and shown to correspond with their actual properties as observed. If these particles are identified with chemical molecules, further correlations between the theory and observations of chemical reactions are possible. Now in the absence of indications to the contrary, it is natural to assume that the theory has shown gases to be literally composed of minute particles, too small to be detected by microscopes, but nevertheless particles of the same kind and obeying the same mechanical laws as marbles or tennis balls.

It is already clear, however, that this similarity between marbles and molecules is not complete, and therefore the use of the word 'particle' is not entirely literal, since while it makes sense, for example, to ask about the size of both marbles and molecules, other qualities such as colour can be predicated only of marbles. A molecule cannot, consistently with the physical theory in which it appears, have a colour, because colour is described in that theory as a function of collections of molecules.

When theoretical science becomes more complex than in this example, the naïve realist view just described breaks down irreparably. Even in the nineteenth century this can be seen to be happening, and the leading theoretical physicists were not unaware of it. It is well known that most physicists then regarded mechanics as the basic physical theory, and explanation in other fields, namely, heat, light, electricity and magnetism, was thought to be incomplete unless it could be given in terms of the mechanics of rigid, elastic, or fluid bodies. But what is not so often realised is that these mechanical models were hardly ever regarded as literal descriptions of entities existing in nature. In many cases it would have been fantastic to regard them as such. Here for instance is a description of Kelvin's model of the luminiferous aether:

'Suppose . . . that a structure is formed of spheres, each sphere being in the centre of a tetrahedron formed by its four nearest neighbours. Let each sphere be joined to these four neighbours by rigid bars, which have spherical caps at their ends so as to slide freely on the spheres. . . . Now attach to each bar a pair of gyroscopically mounted flywheels, rotating with equal and opposite angular velocities, and having their axes in the line of the bar ; . . . the structure as a whole will possess that kind of quasi-elasticity which was first imagined by MacCullagh.'[1]

This aether was supposed to pervade all space from the interior of molecules to the furthest star, but of course neither Kelvin[2] nor anyone else believed that indefinitely small and indefinitely numerous replicas of this model extended throughout the whole of space and pervaded all matter. It had already become clear that the language which described the model was not being used literally to describe the world. Maxwell, whose incidental remarks in scientific papers show him to have been an exceptionally clear thinker about the logical significance of theories, says that such mechanical models must not be taken as modes of connection existing in nature, but are only intended to show that a mechanism can be imagined which is equivalent to the electromagnetic connections of the aether.[3] He goes on to remark that there is generally an infinite number of mechanisms corresponding to a given electromagnetic system. Any such mechanism is neither postulated to exist nor is it unique. It is merely that some of its properties correspond with the observed features of nature, but beyond the observations it does not necessarily correspond with nature in any literal sense.

With the advent of modern theories of matter and radiation it became impossible to maintain a realist view in the old mechanical sense. In quantum theory, for example, it is impossible to find a single mechanical model to represent either atomic structure or radiation. The theory itself will be described in more detail in a later chapter ; its relevance here is that the difficulties to which it led gave support to the positivist view of theories, and this view is still quite widely thought to be a necessary epistemological consequence of quantum physics.

[1] E. T. Whittaker, *History of the Theories of Aether and Electricity*, I, London, 1951, p. 145

[2] Kelvin did indeed remark, 'I never satisfy myself until I can make a mechanical model of a thing. If I can make a mechanical model I can understand it' (quoted from the original edition of the *Baltimore Lectures*, 1884, by S. P. Thompson, *Life of William Thomson*, II, London, 1910, p. 835), but he nowhere claimed that the model was necessarily identical with the thing.

[3] *Treatise on Electricity and Magnetism*, II, Oxford, 1873, p. 416

Operationalism

Parallel with these developments in physics went a good deal of detailed philosophical discussion which sought to interpret physics from a positivist standpoint. Ernst Mach put forward a phenomenalist theory of science, according to which theories are not attempts to explain phenomena by describing a real world which somehow causes the phenomena, but are merely shorthand accounts of the phenomena themselves, effecting an ' economy of thought ' in dealing with them. Russell at one stage of his thought tried to eliminate theoretical concepts from science altogether by defining them explicitly in terms of direct observations, or sense-data, so that their status would be simply that of a shorthand, replaceable in principle by longer descriptions of the sense-data themselves. Thus, according to Russell,[1] when we speak about light waves, we are really using the term as shorthand for a series of observation statements about measurements of reflections, refractions, spectral shifts and so on. Various operational theories have had the same programme of defining theoretical concepts explicitly in terms of the laboratory operations required to measure their numerical values, and according to this view of science, all concepts which cannot be so defined are meaningless, leading to the asking of unanswerable questions, and should therefore be eliminated from scientific theory. The view gained some plausibility from two developments of nineteenth-century physics which culminated in Einstein's special theory of relativity: first, the impossibility of finding any experimental procedure for measuring the absolute velocity of the earth through the aether, and second, the absence of any clear meaning of the concept of simultaneous times at distant points of space, a concept which had been tacitly presupposed to be meaningful in Newtonian mechanics. In the first case the lack of possible operations for measuring absolute velocity led to its abandonment as a meaningful concept within the framework of special relativity. In the second case an explicit ' operational ' definition for simultaneity was provided in terms of observers equipped with clocks and light-signalling apparatus, and it was then found that the acceptable definition differed from Newtonian assumptions in that two times at spatially separated points might be simultaneous for some observers but not for others. Absolute simultaneity therefore lost its meaning within special relativity.

At the time when he was beginning to develop relativity theory, Einstein himself, influenced by Mach, was inclined to interpret

[1] ' The Relation of Sense-Data to Physics ', *Mysticism and Logic*, London, 1918

scientific theories according to the positivist view. Later, however, he modified his opinion considerably, and spoke of theories as free constructions of the mind, not restricted by any necessity of *explicit* definition in terms of observations.[1] But many of his scientific disciples did not follow him in his later philosophy of science. It was Eddington, who gave the first detailed account of relativity theory in English in his *Mathematical Theory of Relativity* in 1923, who also gave in the Introduction to that book one of the first clear statements of operationalism : ' A physical quantity is defined by the series of operations and calculations of which it is the result '.[2] In 1927 P. W. Bridgman gave what is still the classical account of this view of science in his *Logic of Modern Physics*, and although Bridgman himself has since considerably modified his earlier views, this book has remained a typical expression of the approach of many scientists. Lip-service was also paid to the principle of operationalism by some philosophers, including those of the Vienna Circle in their early writings, although it was immediately clear that strict adherence to it would require very drastic reformulations of existing physical theories, including, for example, that of the succeeding chapters of Eddington's own *Mathematical Theory of Relativity*. Bridgman did in fact give some examples of the kind of reformulation required, and claimed that certain purely technical puzzles in classical physics were cleared up by the operational technique,[3] but it was quantum theory that provided the worst conceptual puzzles, and here leading physicists began to use the epistemological language of operationalism, and to claim that certain formulations of quantum theory did correspond to the criterion of operational definition, and thereby eliminated paradoxical and meaningless talk about quantities which could in principle not be measured.

However, the programme of eliminating non-operational concepts from physics was subjected to damaging criticism and has now been abandoned in its original form by practically all philosophers of science including Bridgman and the former members of the Vienna Circle. The programme broke down for three main reasons. First it was shown by F. P. Ramsey and others [4] that it is not possible either in existing scientific theories, or in artificially constructed examples, to define all the concepts in terms of operations.

[1] See, for example, his ' Reply to Criticisms ' in *Albert Einstein : Philosopher-Scientist*, ed. P. A. Schilpp, Evanston, 1949, p. 665

[2] *Mathematical Theory of Relativity*, Cambridge, 1923, p. 3

[3] cf. *The Nature of some of our Physical Concepts*, New York, 1952

[4] F. P. Ramsey, *Foundations of Mathematics*, London, 1931, p. 212 ; R. B. Braithwaite, *Scientific Explanation*, Cambridge, 1953, Chap. iii ; M. B. Hesse, ' Operational Definition and Analogy in Physical Theories ', *B.J.P.S.*, II, 1952, p. 281

The notion of wave-function in quantum physics for example cannot be defined in terms of observational concepts, although the argument can proceed in the reverse direction : if wave-functions are postulated, certain properties of matter and radiation can be deduced, and these are the properties which are observed. No-one would now claim that explicit definitions of such concepts as wave-function are carried out in quantum theory. The second objection to the operational programme is that the possibility of explicit definitions is not generally one of the considerations which weigh with scientists in judging a good theory, although it may sometimes be useful to pay attention to such possibilities when considering what concepts may safely be abandoned, as in the case of absolute velocity and simultaneity. Absence of any possible operational definition *permits* but does not *compel* elimination of a concept, and it sometimes happens that a concept has been retained unnecessarily because it has been assumed to have a direct operational meaning, although this is later found not to be the case. Thus it was not clear before the Michelson-Morley and similar experiments that the 'unobservable' properties ascribed to the aether by nineteenth-century physics were in contradiction with the empirical facts, and when the experiments had been performed, the aether was abandoned, not primarily because it was unobservable, but because of these contradictions. The fact, however, that the aether was in some senses unobservable, meant that it could be abandoned without conflict with other empirical facts. The technique of operationalism certainly played a useful part in clarifying cases like these. But the third and most damaging objection to its general application [1] is that if explicit definitions of theoretical concepts in terms of observations were possible, the theory would become useless because incapable of growth. Theories must have 'open texture', in Waismann's phrase, that is, a fringe of meaning not defined by observation, otherwise the whole meaning of the theory would change whenever it was desired to incorporate into it observations of a novel kind, and it is precisely the function of theories to assimilate such new observations without the meaning of the theories being radically altered. We shall return to this point below.

The Hypothetico-Deductive Method and Falsifiability

In dealing with these difficulties, the positivist view of theories passed into a second phase. In this theories are described as

[1] cf. N. R. Campbell, *Physics, the Elements*, Cambridge, 1920, Chap. vi ; F. P. Ramsey, loc. cit. ; F. Waismann, 'Verifiability', *Logic and Language* (1st series), ed. A. G. N. Flew, Oxford, 1952, p. 117 ; R. B. Braithwaite, loc. cit.

hypothetico-deductive systems, that is, as consisting of hypotheses [1] in the form of postulates and deductions from the postulates in which some, but not all, of the statements can be interpreted as observation statements and confirmed or refuted by experiment. Since it is now admitted that not all the statements of the hypothesis can be given direct empirical meaning by being translated into observation statements, some further conditions have to be satisfied by the non-observational, or theoretical, statements if they are to qualify as meaningful parts of science.

Clearly, such theoretical statements cannot be conclusively verified, for the logical form of the hypothetico-deductive system is: '*A* (hypothesis) implies *B* (observation), and *B* is true', and these premisses do not permit the inference: '*A* is true'. If *B* is false, however, it does follow that *A* is false, and it may be said that theories are conclusively falsified if their consequences are contradicted by observation. So instead of a criterion of empirical verification which the early logical positivists had demanded as the condition for the meaning of a statement, a search began for criteria of falsifiability which would admit genuine scientific theories to be meaningful, but would eliminate non-empirical metaphysics and pseudo-science.[2] The criteria are intended to ensure that a theoretical statement, though not directly verifiable by observation, does nevertheless have empirical consequences, in the sense that some prediction about the world can be deduced from it, together with other theoretical statements, in such a way that the non-fulfilment of the prediction would show one or more of the theoretical statements to be false. The condition would thus eliminate statements which could never be shown to be false by any empirical happening whatever. For example, if an opponent of Newton's gravitational theory had wished to maintain that it is really the intelligent souls of the planets which direct them in their orbits, no future observation of the orbits could show this statement to be false, since it would always be open to anyone who asserted it to say that the planetary souls had willed whatever orbits happened to be observed. The

[1] The word 'hypothesis' is not used here, or in what follows, to denote a doubtful and tentative theory, but to mean that part of *any* theory, however well established, which is not immediate description of observation, nor deduced from such description, and which may logically therefore be false. The notion of 'immediate description of observation' will be examined further below.

[2] Popper has pointed out ('Philosophy of Science: a personal report', *British Philosophy in the Mid-Century*, ed. C. A. Mace, London, 1957, p. 155) that when, in the 1920s, he introduced the notion of falsifiability as characteristic of science, he saw it, not as a criterion of *meaning*, in the fashion of the positivists, but as a criterion of *demarcation* between science on the one hand, and myth, metaphysics and pseudo-science on the other. It does not follow for Popper that non-falsifiable statements are meaningless.