REVIEWS

PROFESSOR BOHM'S PHILOSOPHY OF NATURE

I

THIS is a belated review of a highly interesting and thought provoking book.[1] Although dealing with some difficulties of a very specialised theory of today, viz. of the quantum theory, it should yet be of interest to the many non-physicists who want to know about the world we live in as well as about the ideas which are at present being developed for understanding this world. It is often assumed—and the basic philosophy of many contemporary physicists supports this assumption—that within the sciences speculation and ingenuity cannot play a very great rôle as physical theories are more or less uniquely determined by the facts. It is of course also assumed that our present knowledge about the microcosm is determined in exactly this way and therefore irrevocable, at least in its main features. The book shows that this is not correct, it shows that today there exists a clash of ideas about some very fundamental things, that the imposing and perhaps a little terrifying picture of science of an unalterable and steadily increasing collection of facts is nothing but a myth, and that ingenuity and speculation play in physics as great a rôle as anywhere else. It also shows that even now it is possible to present difficult matters in an interesting and understandable way. It shows thereby that the separation, so often deplored, between the sciences and the humanities is due to a false picture, if not a caricature of science. It is this false picture which is attacked throughout the book. More especially, the book contains an explicit refutation of the idea that complementarity, and complementarity alone solves all the ontological and conceptual problems of microphysics; that this solution possesses absolute validity; that the only thing left to the physicist of the future is to find, and to solve equations for the prediction of events which are otherwise well understood. In short, it contains a refutation of the idea that the physicist of the future is bound to be very similar to the more dogmatic of the medieval scholars with the sole exception that Bohr, and not Aristotle, will be his authority in matters metaphysical.

Secondly, the book presents, in qualitative terms, a new interpretation of some microphysical theories, and especially of the elementary quantum-theory of Schrödinger and Heisenberg. It attempts to develop, again in qualitative terms, a general picture of the universe which can give an account

[1] *Causality and Chance in Modern Physics*, by David Bohm. Van Nostrand, New York; Routledge, London; 1957, pp. 170, $5 or 21s.

of statistical phenomena without assuming that they are irreducible. It discusses, on the basis of the picture presented, such fundamental problems of scientific method as the problem of induction, and the problem of the validity of empirical generalisations and of universal theories. Doing this without any discussion of 'ordinary language' or of language systems it (implicitly)[1] refutes another idea that is very fashionable today, viz. the idea that the only fruitful way of discussing more general problems of knowledge is either to analyse 'ordinary' language (whatever that may mean), or to construct formal systems and to investigate their properties.

Having expressed in the above two paragraphs, as I hope I have done, that I consider Bohm's book a major contribution to the contemporary philosophy of nature I must at once add that there are many things in it which I cannot accept and that more especially his discussion of the problem of induction seems to me to be highly unsatisfactory. Bohm's physical ideas are original, refreshing, and sorely needed in a time of complacency with respect to fundamentals. But the philosophical standpoint taken up with respect to both physics and cosmology is traditional, and perhaps even reactionary: it is a curious mixture of the methodological doctrine of inductivism and of ideas which may be found in various dialectical philosophies. This will become evident from a more detailed investigation of the book.

2

In order to enter into Bohm's theory, I will first discuss the Copenhagen point of view. When it was first conceived this point of view constituted an interpretational feat of great importance. One realises this when the historical situation is considered a little more closely. The early quantum theory of Bohr and Sommerfeld, although experimentally very successful, was yet regarded as unsatisfactory by many physicists. Its main fault was seen to lie in the fact that it combined classical and non-classical assumptions in a way that made a coherent interpretation impossible. For many physicists it was nothing more than a stepping stone on the way to a really satisfactory theory, i.e. to a theory which could give us not only correct predictions, but also some insight into the nature and the dynamics of microscopic entities. It is quite true that Bohr, Heisenberg, and others worked along very different lines. Their main objective was not the construction of a new physical theory about a world that existed independently of measurement and observation; their main objective was rather the construction of a logical machinery for the utilisation of those parts of classical physics which could still be said to lead to correct predictions. Quite obviously a theory of this latter type does not admit of a realistic interpreta-

[1] Cf. also the explicit discussion of the merits of conceptual analysis in [156]. (*Note*: numbers in square brackets refer to pages of the book under review.)

tion: the classical signs it contains cannot be interpreted realistically as they are no longer universally applicable. And the non-classical signs it contains cannot be interpreted realistically as they are elements of the logical machinery used for the purpose of prediction, and possess no meaning apart from that usage. However that may be—the philosophical spirit behind the 'Korrespondenzdenken' was by no means shared by everybody. Now the most important thing is that Schrödinger's wave mechanics, which was conceived in an entirely different spirit, and which seemed to present the long awaited new and coherent account of the microscopic entities, encountered peculiar difficulties when the attempt was made to connect it with a universal interpretation of the kind that was applicable to the earlier theories. Any attempt to interpret wave mechanics as descriptive of entities which, although possessing new and surprising features, were still elements of an objective physical universe, any such attempt was found to lead to paradoxical consequences. It was Bohr's great merit that in this situation he developed an intuitive idea, the *idea of complementarity*, which, although incompatible with a straightforward realism, nevertheless gave the physicists a much needed intuitive aid for the handling of concrete problems.

According to this idea properties can be ascribed to a microscopic system only when it interacts with a suitable classical (i.e. macroscopic) piece of matter. Apart from the interaction the system possesses no properties at all. It is also asserted that the totality of classical measuring-instruments[1] divides into pairs of kinds which are mutually incompatible in the following sense: if the system under investigation interacts with a measuring instrument which belongs to one of two mutually incompatible kinds, then all the properties defined by interaction with the other kind will be wholly undetermined. And 'wholly undetermined' means that it would be meaningless to ascribe such a property to the system just as it would be meaningless to ascribe to a fluid a certain value on the Mohs scale of scratchability. It is clear that the *uncertainty relations* now indicate the domain of permissible applicability of classical functors (such as the functor 'position') rather than the mean deviations of their otherwise well defined values in large *ensembles*.

The idea of complementarity can be interpreted in two different ways. It can be interpreted as an attempt to provide an intuitive picture for an existing theory, viz. wave mechanics, and as a heuristic principle guiding future research. This interpretation is undogmatic as it admits the possibility of alternatives, and even of preferable alternatives. A physicist who looks at complementarity in this way will regard it as an interesting fact about quantum theory that it is *compatible* with a relational point of view where

[1] This totality comprises pieces of matter which have not been prepared by a physicist for the purpose of measurement, but which, by accident, as it were, satisfy some very general conditions not to be discussed here.

interaction is a necessary condition of the meaningful applicability of terms which within classical physics (relativity included) are definable without such reference. He will also point out that there exist no satisfactory alternatives. But he will never go as far as to assert that such alternatives will never be found, or that they would be logically inconsistent, or that they would contradict the facts. But Bohr's idea of complementarity can also be interpreted in a different way. It can be interpreted as a basic philosophical principle which is incapable of refutation and to which any future theory *must* conform. Bohr himself most certainly took this stronger point of view. 'Thus rather than consider the indeterminacy relationships primarily as a deduction from quantum mechanics in its current form he postulates these relationships directly as a basic law of nature and assumes . . . that all other laws will have to be consistent with these relationships' [83, referring to Heisenberg]. His assumption was 'that the basic properties of matter can *never* be understood rationally in terms of unique and unambiguous models' which implies that 'the use of complementary pairs of imprecisely defined concepts will be necessary for the detailed treatment of every domain that will ever be investigated' [94]. It is true that some followers of the Copenhagen school have denied that this absolutism is part of complementarity. Thus in a discussion Rosenfeld has asserted that 'nobody thinks of attributing an absolute validity to the principles of quantum theory'.[1] But quite apart from the fact that he himself said in the lecture preceding this discussion that 'every feature' of the theory 'is forced upon us',[2] there is Bohr's explicit statement that 'it would be a misconception to believe that the difficulties of the atomic theory may be evaded by eventually replacing the concepts of classical physics by new conceptual forms'.[3]

3

This dogmatism with respect to fundamental principles is attacked and refuted in Chapter III of Bohm's book. The chapter contains an extremely lucid description of the development of the quantum theory and the various interpretations which have been suggested for it. It explains the resonable elements of the point of view of Bohr and Heisenberg. This point of view is presented with a clarity that is sadly missing in many writers who support Bohr, and with an understanding, and authority that reveals the former follower and expositor[4] of Bohr's ideas. The idea of its final and absolute validity is refuted by showing that all attempts to prove it (as indeed all attempts of a 'transcendental deduction' of physical principles) are circular.

[1] Rosenfeld, *Observation and Interpretation*, ed. Körner, London, 1957, p. 52

[2] Ibid., p. 41

[3] N. Bohr, *Atomic Theory and the Description of Nature*, Cambridge, 1932, p. 16

[4] Cf. Bohm, *Quantum Theory*, Princeton, 1951

Thus, in Heisenberg's 'proof' of the uncertainty principle (which is often used as an argument for its absolute validity)

> it was essential to use three properties; namely the quantization of energy and momentum in all interactions; the existence of these quanta; and the unpredictable and uncontrollable character of certain features of the individual quantum process. It is certainly true that these properties follow from the quantum theory [94].

However in order to show the basic and irrefutable character of the uncertainty principle these features themselves would have to be demonstrated as basic and irrefutable. Quite obviously such a demonstration cannot be achieved by pointing to some theorems of *wave mechanics* (such as von Neumann's theorems) as this would only lead to the further question whether wave mechanics is valid in all domains of experimentation [95]. Nor can it be achieved, as has been attempted by many inductivists, by utilising the fact (if it is a fact) that either wave mechanics, or some part of it, is highly confirmed. In order to see this most most clearly we need only realise that the assertion of the absolute validity of a physical principle implies the denial of any theory that contains its negation. More especially, the assertion of the absolute validity of the uncertainty principle implies the denial of any theory that ascribes to it only a limited validity in a restricted domain. But how could such a denial be justified by *experience* if the denied theory is so constructed that it gives the same predictions as the defended principle wherever the latter has been found to be confirmed by experience?[1]

It follows that neither experience nor mathematics can help if a decision is to be made between wave mechanics and an alternative theory which agrees with it in all those points where the latter has been found to be empirically successful. Now the idea of complementarity is well fitted to the structure of wave mechanics. As we cannot make any restrictions upon the structure of the empirically satisfactory alternatives of wave mechanics it also follows that its interpretation as a basic and irrefutable principle must be given up. Neither mathematics nor experience can be used to support such an interpretation. All this means, of course, that the position of

[1] Quantum mechanics is not the first theory that has been utilised for the purpose of excluding alternatives. Using the fact that certain theorems of Newtonian mechanics contradicted the second law of thermodynamics, Ostwald and Mach argued that a mechanical account of heat was impossible, and that Newton's laws could not be universally valid. It turned out, however, that it was the second law that was not universally valid (fluctuations). Quite clearly the Ostwald-Mach argument suffered from the same deficiency as the more recent arguments of Born, Rosenfeld, and others. They argued: the second law is highly confirmed; classical mechanics contradicts the second law; hence classical mechanics is not universally valid. They overlooked (*a*) that confirmation does not imply truth; (*b*) that the mechanical theory of heat contradicted the second law in a domain in which it had not yet been tested, and in which it was therefore neither confirmed nor disconfirmed.

complementarity is a metaphysical position[1] which can be defended by arguments of plausibility only.

4

So far only the (empirical and logical) *possibility* of alternative points of view has been shown. In Chapter IV of his book Bohm turns to the discussion of some alternatives that have actually been proposed in the literature and he also expounds some of his own ideas. I shall now give an outline of the epistemological background of all these alternatives.

One of the basic assumptions of the orthodox is that 'in our description of nature the purpose is . . . to trace down, as far as it is possible, relations between the manifold aspects of our experience'.[2] For them the facts of experience play the rôle of building stones out of which a theory may be constructed but which themselves neither can, nor should be modified. If we add to this the idea that 'only with the help of classical ideas is it possible to ascribe an unambiguous meaning to the results of observation'[3] (which means that the building stones referred to in the first quotation are classical states of affairs) we arrive at once at the result that a microscopic theory cannot be anything but a device for the prediction of a particular kind of fact, viz. of classical states of affairs. Now it is quite true that this point of view has led to some useful results (example: the dispersion formula of Ladenburg-Kramers; the first investigations of Heisenberg). It is also true that the quantum theory is the first theory of importance which to some extent satisfies the programme of Berkeley and Mach (classical states of affairs replacing the 'perceptions' of the former and the 'elements' of the latter). But it must not be forgotten that there is a whole tradition which

[1] I use here the word 'metaphysical' in the same sense in which it is used by the adherents of the orthodox point of view, viz. in the sense of 'neither mathematical, nor empirical'. That the Copenhagen interpretation is metaphysical in this sense has been asserted, in slightly different words, by Heisenberg who declared in 1930 (*Die physikalischen Grundlagen der Quantentheorie*, p. 15), that its adoption was a 'question of taste'. This he repeated in 1958 in the now more fashionable linguistic terminology (cf. *Physics and Philosophy*, New York, 1958, pp. 29 f). However at the very same place a highly objectionable criticism is found of Bohm's model of 1952. This model, it is asserted, 'cannot be refuted by experiment since [it] only repeat[s] the Copenhagen interpretation in a different language. From a strictly positivistic standpoint' Heisenberg continues 'one may even say that we are here concerned not with counterproposals to the Copenhagen interpretation, but with its exact repetition in a different language'. Is it really the case that Bohm's counterexample against the assertion, made by von Neumann and others, that quantum theory does not allow for the addition of *untestable* hidden parameters (cf. von Neumann, *Mathematical Foundations of Quantum Mechanics*, Princeton, 1955, p. 326) is nothing but the 'exact repetition' of this assertion 'in a different language'?

[2] Bohr, op. cit., p. 18

[3] Ibid., p. 17

is connected with the philosophical position of realism[1] and which went along completely different lines. In this tradition the facts of experience, whether or not they are now describable in terms of a universal theory (such as classical mechanics), are not regarded as unalterable building stones of knowledge; they are regarded as capable of analysis, of improvement, and it is even assumed that such an analysis and improvement is absolutely necessary. Indeed, the new theory of motion which was developed by Galileo and Newton could not possibly be understood as a device for establishing 'relations between the manifold aspects of our experience', the simple reason being that, according to this very theory, observable motion would at best give us an approximation to its fundamental laws. Similarly the atomic theory of the late nineteenth century was not only not suggested, it was even contradicted by what was then regarded as an account of 'experience', viz. classical thermodynamics. This tradition proceeds from the very reasonable assumption that our ideas *as well as* our experiences may be erroneous and that the latter give us at most an approximative account of what is going on in reality. Bohm's own point of view is closely connected with this tradition. Having shown that all the attempts to prove the uniqueness of the Copenhagen interpretation are invalid, he suggests 'to take the field and particle concepts of classical physics as starting points and to *modify* and enrich them in such a way that they are able to deal with the new combination of wave and particle properties that is implied in the quantum theory' [98; my italics]. Such modified concepts, or even a completely new conceptual apparatus which does not any more make use of classical ideas, will of course at first be 'extraphysical' [99] in the sense that it will not be accessible to test with the help of methods available *before* it was conceived. However 'the history of scientific research is full of examples in which it was very fruitful indeed to assume that certain objects and elements might be real, long before any procedures were known that could permit them to be observed directly' [99]. Assumptions of this kind then

> ultimately lead to new kinds of experiments and thus to the discovery of new facts. In the light of this historical experience [Bohm continues] positivism (i.e. the point of view expressed in the two above quotations) is seen to lead to a one sided point of view of the possible means of carrying out research. For while it recognizes the importance of the empirical data, positivism flies into the face of the historically demonstrable fact that the proposal of new concepts and theories having certain speculative aspects (e.g. the atomic theory) has quite frequently turned out to be as important in the long run as empirical discoveries have been [99].

[1] For this connection cf. K. R. Popper's article 'The Aim of Science', *Ratio*, 1958, I

In this way positivism 'constitutes a dogmatic restriction of the possible forms of future experience' which in the case of quantum mechanics leads to the belief

> that the success of probabilistic theories of the type of the current quantum mechanics indicates that in the next domain it is very likely that we shall be led to theories that are . . . even more probabilistic than those of the current quantum domain [104].[1]

5

More concretely, Bohm's ideas as presented in the book under review may be regarded as an adaptation, to the case of the quantum theory, of the situation described by the classical kinetic theory of matter. The kinetic theory was an attempt to give an explanation, in terms of the motion of small, and as yet unobserved, particles, of the behaviour of thermodynamic systems. According to this theory continuous improvement of the precision of measurements will lead to the following phenomena (we assume that we move outside the domain where relativistic effects become noticeable): as long as we are dealing with large systems the classical laws of motion (and the second law of thermodynamics) will be found to hold with absolute precision. However when experimenting with fairly small systems such as dust-particles which are immersed into a surrounding medium, a completely new type of behaviour becomes apparent. These particles experience random displacements for which no explanation can be given in terms of the movements of bodies of a similar size. The laws describing this type of behaviour are not any longer the laws of classical mechanics. They are purely probabilistic and allow us to predict averages in large *ensembles* rather than individual processes. Within the framework of these laws no reason can be given for the occurrence of a particular movement of a particular particle. It can even be shown [107] that for particles under the conditions described above there exist laws which are formally identical with the uncertainty relationships, the diffusion constant of the embedding medium taking the place of Planck's constant h. But the situation changes again

[1] A terminological remark: quantum physicists have sometimes refused to be called 'positivists' on account of the fact that they accepted the Copenhagen point of view. Thus in *Niels Bohr and the Development of Physics* (London, 1955, p. 22) Heisenberg asserts that 'the Copenhagen interpretation . . . is in no way positivistic. For whereas positivism is based upon the sensual perceptions of the observer . . . the Copenhagen interpretation regards things and processes which are describable in terms of classical concepts . . . as the foundation of any physical interpretation.' This is quite true. However this 'foundation' is again assumed to be 'given' in the sense that it cannot be further analysed or explained, an attitude which to a certain extent still justifies the term 'positivism'.

when we further improve the precision of our measurements or else use experiments of an altogether different type. We shall then find that the random behaviour of the dust particles is explainable in terms of a new set of causal laws referring to very small particles which are the ultimate constituents of the medium in which the dust-particles are immersed. (In the case of the kinetic theory these new laws happen to coincide with the laws of classical mechanics from which we started. However it is necessary to point out, in accordance with Bohm's more general ideas, that this need not always be the case.)

Speaking more generally one may now say that according to the kinetic theory there exist three different levels of experimentation which are characterised by three different sets of laws. There is the macroscopic level where the laws of classical mechanics hold exactly. More precise experiments show then that these laws are not universally valid, and thereby delimit the domain of their applicability. At the same time they lead to a new set of laws governing phenomena which are *qualitatively* different from the phenomena we meet on the macrolevel, as they involve randomness. These new laws in their turn are not universally valid as they can be shown to be the result of the very complex, but again causal behaviour of entities on a still deeper level.

Now it is Bohm's contention that the situation in the domain of the quantum phenomena is similar to the one just described. As opposed to the opinion of the majority of physicists he assumes that the probability laws of the present quantum theory are the result of the very complex interplay of entities on a deeper level, and are therefore neither ultimate nor irreducible. Chapter IV contains a general discussion of various ways in which such a sub-quantum-mechanical level can be conceived. These considerations have been criticised by some members of the Copenhagen circle. One of the most frequent criticisms is that nobody has as yet succeeded in constructing a theory along these lines which can match the customary theory in predictive success. This criticism seems to proceed from the assumption that the existence of a certain theory and the absence of a theory, which is connected with a different 'ideology' as it were, may be regarded as an implicit criticism of the latter. However the fact that this pragmatic criticism can also be directed against the dynamical investigations of Galileo and Kepler (the successful theory being in this case Aristotle's theory of motion) should be sufficient to make its proponents a little more cautious about its force. A second criticism points out that the present theories, and the philosophical structure connected with them, are firmly based upon experience. This criticism has already been dismissed in an earlier part of the present review. Indeed, we have seen that the customary point of view about microphysics cannot produce any empirical or logical argument against a procedure such as Bohm's. And assertions such as 'it is idle to "hope" that the cure of our troubles will come from underpinning quantum theory with some deterministic substratum' can at most be regarded as affirmations of faith.[1]

6

I leave now the physics of the book and turn to a discussion of the cosmology and methodology developed in it. Both these fields are dealt with on the basis of a generalisation of the situation described by the kinetic theory. The cosmological generalisation, as I understand it, is as follows: the world contains infinitely many levels. Each level is characterised by a set of laws which may be causal, or probabilistic, or both. The validity of these laws need not extend beyond the level to which they belong. When a certain level is left qualitatively new processes appear which have to be described by a new set of laws. Bohm recognises that sometimes these new laws may be general enough to allow for the derivation of the more specific laws of the preceding level (example: special relativity—general relativity; cf. [141]). However he points out—and this must be regarded as a highly important contribution to cosmology—that such a reduction need not always be possible. Let us assume, for example, that the level L_1 of causal laws possesses a substratum L_2 of probability laws which are the outcome of the causal interplay of entities of a level L_3 which in its turn possesses a probabilistic substratum L_4, and so on. Now the fact that the laws of L_2 can be explained by reference to complicated causal mechanisms on L_3 shows that they cannot be entirely random. On the other hand the laws of L_2 are not absolutely causal either as they are limited by the fluctuations which appear upon L_4. A complete explanation of the laws of L_1 (or of any set of causal laws or of probability laws) would therefore have to take into account an infinity of laws and levels. Clearly, then, an explanation of the laws of L_1 in terms of a finite sequence of substrata cannot be regarded as a *reduction* of L_1 to these substrata. Each level, and each set of laws possesses a surplus over and above any finite set of more general laws. It is only if we take all the mutually irreducible properties and laws together that we may hope to

[1] Rosenfeld, in *Observation and Interpretation*, p. 44. In his review of the present book in the *Manchester Guardian*, L. Rosenfeld accused Bohm of contradicting the 'exigencies of sound scientific method' and he described the followers of Niels Bohr (and presumably also himself) as possessing the 'uncommitted, commonsense attitude of the true scientist'. Now first of all an attitude can hardly be called 'uncommitted' if it appeals to the principle that experience alone can be the judge of our theories, and at the same time is singled out neither by experience, nor by mathematics. Secondly the history of science has given ample evidence for the fact that it is 'sound scientific method' not to take experience at its face value, even if it should be expressed in very complicated (classical) terms, but to try to explain it as the result of processes which are not immediately accessible to observation. It is strange indeed to see that Rosenfeld describes as 'uncommitted' the attitude of those who because of their observationalistic bias distort both history and scientific method.

get a complete account of one particular level. This is the way in which Bohm makes physical sense of the idea of emergence and the irreducibility of qualities. At the same time it is suggested, at least by the cosmological model we are discussing at the present moment, that qualities may be reducible after all if only appropriate mathematical instruments are found for the handling of infinities of relatively self-contained experimental domains. The model also suggests a new interpretation of the difficult problems of probability, randomness, and statistical independence. In this interpretation neither the idea of a deterministic law, nor the idea of randomness is given absolute preference [20 f]. The laws of nature, whether they appear in the form of causal laws, or in the form of probability laws are regarded as a Hegelian synthesis, as it were, of the idea of absolute determination (the thesis), and of absolute randomness (the antithesis). This way of describing Bohm's procedure is by no means a mere verbal trick, for it is Bohm's conviction that in all fields the alternative use of opposite sets of concepts is to be preferred to the exclusive utilisation of only one of them.

7

However, the model which we have just described and which plays an important rôle in Bohm's analysis of probability is not the one he uses in his discussion of scientific method. He is ' not even supposing that the general pattern of levels that has been so widely found in nature thus far must necessarily continue without limit '. He admits the possibility that ' even the pattern of levels itself will eventually fade out and be replaced by something quite different ' [139]. The structure of levels, he asserts, is only one way in which the *qualitative infinity of nature* may represent itself to the experimenter. This qualitative infinity of nature is one of the basic postulates of Bohm's cosmology. He incessantly insists upon the ' inexhaustible *depth* in the properties and qualities of matter ' [138] which is such that no finite system of laws and categories can ever express it adequately. Every thing and every process has infinitely many sides to it which are such that at any stage of scientific development they will only approximately be expressed by the laws and the concepts then in use. That such an approximate representation is at all possible is due to the further fact that there exists ' some degree of autonomy and stability ' in the mode of being of the things around us [139]. For example

> we may say that [a] real fluid is enormously richer in qualities and properties than is our macroscopic concept of it. It is richer, however, in just such a way that these additional characteristics may, in a wide variety of cases, be ignored in the macrodomain [155].

In spite of the fact that in every real fluid an infinite variety of processes is going on which are not covered by our macroscopic description of it, these processes just so counterbalance each other that relative stability is achieved upon the macrolevel, and the macroscopic description is in this way made applicable within its proper domain of validity. In short: the world is infinite as regards the properties and processes which are present in every part of it. But these properties are arranged in complexes of relative stability which may then be described with the help of scientific theories employing a finite number of concepts only. Every such description is true within a certain domain of validity. On the other hand the presence of further properties which are not covered by the description, and which slightly influence the elements of the complex implies that

> associated with any given law there must be errors that are essential and objective features of that law resulting from the multitude of diverse factors that the law in question must neglect. Thus each law inevitably has its errors, and these are just as necessary a part of its true significance as are those of its consequences that are correct [166].

It is important to repeat that for Bohm the errors referred to in the above quotation are not purely subjective phenomena; they possess an objective counterpart in the way in which the interplay between the elements of the relatively stable complexes as well as the qualities that have been left out delimits the validity of the laws describing the behaviour of the complexes. ' It is clear from the above discussion ' Bohm continues [166], ' that scientific research does not, and cannot lead to a knowledge of nature that is completely free from error. '

8

The application to *scientific method* is now quite straightforward. Nature is such that no law can ever be universally valid. Hence, it is sound scientific method to restrict the laws we find to a certain domain [135]. It is unsound method to apply them outside this domain. And never should we be so bold as to proclaim a certain law as universally valid, i.e. as valid in all domains of experimentation, and under all possible conditions. On the other hand, if we are careful enough in our pronouncements about the applicability of a scientific theory, and if we always restrict it to its proper domain, we do not run the risk of being refuted by new discoveries. For 'a new theory to which the discovery of . . . errors will eventually give rise, does not invalidate the old theories. Rather . . . it corrects the older theories in the domain in which they are inadequate and, in doing so, it helps to define the conditions under which they are valid ' [31]. Only a philosophical idea, and not sound scientific method can lead to the attempt to apply a theory to every possible domain. Thus the assumption ' that all the various levels, all qualitative changes, and all chance fluctuations will, eventually be reducible completely . . . to effects of some fixed . . . scheme of purely quantitative laws . . . is . . . essentially philosophical in character ' [62]. More especially the assumption that Newton's laws are universally valid

> has implications not necessarily following from the science of mechanics itself, but rather from the *unlimited* extrapolation of this science . . . Such an extrapolation is evidently . . . not founded . . . on what is known scientifically. Instead, it is in a large measure a consequence of a *philosophical* point of view . . . [37].

It is this methodological doctrine which I find highly questionable and which I shall attempt to criticise in the following last part of my review.

9

First of all, how does Bohm justify his two basic cosmological principles, viz. the principles of the infinity of nature and the second principle that there exist complexes which are relatively stable over a certain period of time and which therefore allow for the description, in terms of finite sets of laws and concepts, of parts of nature? The principle of the infinity of nature he tries to justify partly by reference to experience which shows us a great variety of qualities; partly by reference to the history of science which shows that every set of laws has at some time been found to be valid in a restricted domain only; and partly by reference to the 'basic spirit of scientific method itself, which requires that *every* feature be subjected to continuous probing and testing' [132]. The principle of the existence of complexes of relative independence and stability is again justified by reference to experience; but it is also justified by some kind of 'transcendental' reasoning according to which in a world of a different structure the concept of a thing would not be applicable and science would be impossible [139 f.]. Now if we look at these arguments we find that they are all unsatisfactory. To start with, Bohm's methodological rules which have been stated above would forbid us to draw consequences from experience and to apply them universally. Yet this is just what is done in the first argument. The appeal to the history of science cannot be accepted either. For it could also have been used by the Aristotelians *against* the assumption that human knowledge gave at most an approximate account of what went on in nature. Thirdly the transcendental argument is not of the slightest use as long as we do not know whether our theories express knowledge or whether they are not only well fabricated dreams. But knowing this would presuppose knowledge of exactly those states of affairs whose existence is to be proved with the help of the argument. And finally the methodological argument is of no help either as it might well be the case that all the tests we carry out with respect to a certain theory lead to its corroboration and thereby to the corroboration of the idea that the world possesses a finite number of basic properties after all. We see, then, that Bohm's two basic principles are not supported by the arguments he uses in their favour. They are not even empirical, or scientific in Bohm's own sense [cf. 166] as he is not prepared to admit that they may be valid in a certain domain only and give way to some kind of mechanicism in all the remaining domains of experimentation. They represent an *absolute truth* which is not capable of improvement by taking into account errors [169 f.]. Yet they are cosmological principles, i.e. principles describing the basic structure of our world. This, then, is my first criticism: that there is not the slightest reason for not treating the most general cosmological principles, such as the principle of the infinity of nature on a par with less general laws. There is not the slightest reason for denying them the status of all the other laws, viz. their provisional character.

10

However it seems to me that this criticism does not yet go to the heart of the matter. For it leaves out one of the most important arguments that Bohm could adduce in favour of the absolute character of his two principles. I did not find the argument in the book, but I trust that it may be constructed along the following lines. Consider a law that is valid in a certain domain only. When this law is properly stated we shall soon discover its limitations. We are able to do so because there exists another domain which is not covered by the law, and whose presence is responsible for the errors it possesses. The conditioned validity of the law and its approximative character are thus wholly dependent upon the objective existence of such other domains. It would then seem to follow that for lack of domains outside the domain of its applicability a statement about 'the infinite totality of matter in the process of becoming' [170] must be unconditionally and absolutely valid. It is this argument which will be the starting point of my second criticism.

It is assumed in this argument that the provisional and approximative character of a scientific law is *wholly* due to the objective limitations of the stability of the entities, or of the domain it describes. We must correct the law not because we had a wrong idea about the properties of the things described. We must correct it because these properties themselves are the relatively stable result of a very complicated interplay of an infinity of processes, and because they are therefore subject to slight changes and transformations. *But if we keep well within the domain of application of the law, then we cannot possibly be mistaken.*

This last principle has the following very interesting corollary: every description of nature that has ever been uttered is true within its domain, and conversely, it exhibits the existence of a domain to which it properly applies. There does not exist any description that is wholly mistaken and without a corresponding reality. Or, to express it differently—when describing our surroundings *we always speak the truth* (relative truth, that is), *and we are also always in contact with some part of reality*. Now this corollary has so little *prima facie* plausibility that I must defend it before trying to show its shortcomings. 'Is it really the case', one may easily feel oneself inclined to object 'that the savage who believes in, and claims to have observed, the actions of ghosts, tribal spirits, and the like is talking about entities which have some kind of existence in a restricted domain?' To this objection the retort may well be that a savage could not have described, or interpreted what he saw as indicating the existence of a ghost, if there had not been a justification for doing so. After all, he does not, *and cannot*, make arbitrary judgments in matters which may be of importance to his well being, and even to his life. Neither for him, nor for us would it be possible

> to choose the natural laws holding within a given degree of approximation, and in a particular set of conditions at will . . . This does not mean that we cannot, in general, make our own choices as to what we will, or will not do. But unless these choices are guided by concepts that correctly reflect the necessary relationships that exist in nature, the consequences of our actions will not in general be what we choose, but rather something different [165].

In short, every theory of the universe, whether mythological or scientific in content, possesses some degree of truth, as the choice of a false theory would lead to undesirable consequences and would therefore be at once abandoned. *Nature itself forces man to speak the truth*, and it also forces him to speak in such a way that his theories have objective reference.

This, then, is the epistemology behind Bohm's belief that every theory, however absurd it may seem at first sight, has some kind of truth in it and correctly mirrors what exists in the universe: the ill success of a theory which is outright wrong and does not describe anything whatever is a corrective which after a very short time forces us to abandon it (if we were ever foolish enough to put it forth). Knowledge is a natural process which leads to a mirroring, in the head of man, of the properties of the universe. The mirror-image may be distorted at the edges. But first of all this distortion is due to a similar objective distortion of the processes in the world. And secondly this distortion does not reach into the centre of the mirror which perfectly represents the situation at a certain level.

I do not believe that this account of our knowledge is a correct one. The simplest reason I can give for this contention of mine is that I believe man to be a little more whimsical and capricious than is assumed in the above picture of him. For in this picture it is assumed that *as a matter of fact* we recognise our mistakes, take them into account, and learn from them how to behave better. It is assumed that this process works like a well lubricated machine so that in the end whatever has been said contains some truth in it. (I suspect that a consistent elaboration of this epistemology will finally lead to the result that errors—subjective errors, that is—are never made: quite obviously Hegel's notorious 'Alles Vernünftigeist wirklich' is here lurking in the background.) But only a little knowledge of history will show that this assumption is factually false for at least two reasons. First, because there are enough examples of men, or of whole groups, who are not prepared to admit that they have been mistaken. And secondly because even death may not be a sufficient reason for changing ideas which have led to it. Quite on the contrary we often find, even in our own times, that ill success of an ill-conceived undertaking, and death resulting from it are both regarded as values and we also find the corresponding assumption that fate will sometimes deal roughly with its protegés. Furthermore, to turn to more theoretical considerations, is it not well known that refuting instances can with some ingenuity always be turned into confirming instances and that there exist elaborate theories which perform this transformation nearly automatically? Quite clearly such theories cannot be said to be in contact with reality and this in spite of their sophistication and in spite of the many fascinating statements they contain. From all this we have to conclude that *nature can never force us to admit that we have been mistaken*. Nor can it force us to recognise our mistakes. A mistake will be recognised as such only if first the conscious *decision* has been made not to make use of *ad hoc* hypotheses and to eliminate theories which do not allow of falsification. It is true that as a matter of historical fact this decision has been made by nearly all great scientists (although the present quantum theory seems to present an exception to this rule). What is of importance here is that they never were, and never could be forced to proceed in that way, either by nature or by society.

II

To sum up: at the back of Bohm's theory of knowledge there is the idea that facts and decisions both obey the same kind of laws, i.e. the laws of the material world in which we live. It is the idea that the development of moral codes, or of the laws which govern the non-moral behaviour of the members of a society, or that the development of knowledge is nothing but an aspect of the development of this material universe. This idea implies that neither the moral behaviour, not the social behaviour, nor even the status of our knowledge can be changed on the basis of an explicit decision. It is quite impossible to entertain a point of view which has no reference to any facts whatever. And it is equally impossible to introduce a new moral system unless it is somewhat related to situations already existent. This doctrine of *naturalism*[1] can be given various forms. It exists in a form which allows for the accommodation of the most revolutionary changes by simply asserting that these changes had already been prepared by the development either of the material universe or of society. In this form the doctrine is nothing but a verbal manoeuvre. Another form of the doctrine decrees that some existing pieces of knowledge, or of morals are unchangeable, because a change would amount to nothing less than a change of the unalterable

[1] For an excellent discussion of this doctrine, its history, and its shortcomings see K. R. Popper, *The Open Society and Its Enemies*, Princeton, 1954, Chap. V.

course of events and of the laws which govern the universe. In this form the doctrine has very often been held by the defenders of the *status quo*. The simple logical point that decisions are never derivable from facts should show that in all its forms naturalism is based upon a logical fallacy. Now Bohm's own doctrine, although related to the doctrine of naturalism, is more detailed and less radical. He seems to admit that at times ideas may be invented which have very little to do with the facts. What he contends is, however, that these ideas will very soon be eliminated by a kind of natural selection which works either against those who hold them (they die), or against the ideas themselves (they are given up). That is, Bohm allows for deviations, but at the same time he assumes the existence of a corrective mechanism which quickly eliminates pipe-dreams and falsehoods. Now I want to show that although the doctrine in this form allows us to say that we sometimes speak the truth, it nevertheless does not give us any indication whatever as to which particular point of view expresses the truth. This we see when we ask the following important question: how long does it take this mechanism to eliminate a false hypothesis? Most certainly the length of time will depend upon the frequency with which the theory is tested, upon the decisiveness of the tests as well as upon the intention, on the side of the scientist, to take refutations seriously. Laziness and *ad hoc* manoeuvres may extend the periods of correction indefinitely. And the scientist, or whoever else is defending a certain point of view, need not perish in the course of events as he may well be careful enough to avoid tests which endanger his personal safety (there are numerous examples of this kind in the socalled 'primitive' societies). Furthermore, who says that we shall at once stumble upon a refuting instance? But if this is so then Bohm's idea of the self-correcting character of knowledge does not help us at all to distinguish truth from falsehood. For all we know *all* our ideas may be quite thoroughly mistaken.

Now if this is the case, and if it is further admitted that we are able to discover our errors when trying to apply the ideas we possess (provided of course, we have first *decided* to give them a form in which they are testable, and we have also decided to take refutations seriously) then the only path open to us is that we must attempt relentlessly to falsify our theories. As we do not know which part of them is true, in what domain they are true, and whether they are true at all, we must attempt the falsification under all possible conditions. Testing them under all possible conditions means assuming *first* that they are *universally valid* and *then* trying to find out the limitations of this assumption. It is this fact that we never know to what extent our theories are correct which makes us first apply them universally. If we use a theory in this way we by no means assume, as Bohm seems to think (cf. his criticism of mechanicism, discussed above) that the theory *will be found to be correct* in all domains. The universal application of a theory means rather that *we are prepared to collect refuting instances from all domains.*

The reason why I cannot accept Bohm's methodology of caution and why I prefer to it the methodology of falsification as it has been developed by Popper is therefore that the methodology of caution assumes the existence of things we know for certain, whereas I believe on the basis of the above consideration that this is much too optimistic a view of the status of our knowledge.

These, if I understand the book correctly, are the criticisms which I think must be made. But let me at once repeat that I do not therefore think the book to be of lesser value. Quite on the contrary, it is the repeated discussion and criticism of various points of view which leads to an advance of knowledge, and not the repetition of plain statements in which nobody can find any fault. To have in this way contributed to the theory of knowledge, and also to have shown the unity of (physical, philosophical etc.) knowledge is the great merit of the present book.

P. K. FEYERABEND

RETROSPECTIVE MIRACLES
OR
BETTING AFTER THE RACE

THERE are hardly any subjects today whose status as science is seriously claimed but disputed. Whatever we may think of particular work done in the field, few would deny psychology to be a legitimate field of science. Astrology, on the other hand, is rejected for general as well as particular reasons (lack of communicable agreed procedure, extreme vagueness of predictions, etc.). The authors of the present book[1] are prominently associated with the claim for scientific status of parapsychology.

The first two chapters of this book make it pleasantly clear that there can be no doubt about the *nature* of the claim. While the definition of the subject may be awkward (embracing telepathy, clairvoyance, precognition, and psychokinesis), definition of *any* science is liable to be awkward, and perhaps not important. The authors make it abundantly clear, however, that they share the accepted notions of what constitutes a science. They do claim parapsychology to be a science.

The reader is now eager to read the evidence. Here he is disappointed. There are only some remarks (pp. 46-49; 58-63) with reference to publica-

[1] *Parapsychology*, by J. B. Rhine & J. G. Pratt. Oxford, Blackwell, 1957. Pp. 220. 37s. 6d.