5. The Aim of Science

To speak of 'the aim' of scientific activity may perhaps sound a little naïve; for clearly, different scientists have different aims, and science itself (whatever that may mean) has no aims. I admit all this. And yet it seems that when we speak of science we do feel, more or less clearly, that there is something characteristic of scientific activity; and since scientific activity looks pretty much like a rational activity, and since a rational activity must have some aim, the attempt to describe the aim of science may not be entirely futile.

I suggest that it is the aim of science to find *satisfactory explanations*, of whatever strikes us as being in need of explanation. By an *explanation* (or a causal explanation) is meant a set of statements by which one describes the state of affairs to be explained (the *explicandum*) while the others, the explanatory statements, form the 'explanation' in the narrower sense of the word (the *explicans* of the *explicandum*).

We may take it, as a rule, that the *explicandum* is more or less well known to be true, or assumed to be so known. For there is little point in asking for an explanation of a state of affairs which may turn out to be entirely imaginary. (Flying saucers may represent such a case: the explanation needed may not be of flying saucers, but of reports of flying saucers; yet should flying saucers exist, then no further explanation of the *reports* would be required.) The *explicans*, on the other hand, which is the object of our search, will as a rule not be known: it will have to be discovered. Thus, scientific explanation, whenever it is a discovery, will be *the explanation of the known by the unknown.*[1]

[1] See the last paragraph of the text, before the final quotation, of my 'Note on

This paper is a revised version of a paper which was first published in *Ratio*, vol. i, no. 1, Dec. 1957, pp. 24–35. A brief discussion of the correction of Galileo's and Kepler's results by Newton's theory was first published in my contribution to Simon Moser (ed.), *Gesetz und Wirklichkeit*, 1949 (see especially pp. 57 f.), reprinted in Hans Albert, *Theorie und Realität*, 1964 (see especially p. 100). An English translation of this paper will be found in Appendix 1 to the present volume.

The *explicans*, in order to be satisfactory (satisfactoriness may be a matter of degree), must fulfil a number of conditions. First, it must logically entail the *explicandum*. Secondly, the *explicans* ought to be true, although it will not, in general, be known to be true; in any case, it must not be known to be false even after the most critical examination. If it is not known to be true (as will usually be the case) there must be *independent* evidence in its favour. In other words, it must be *independently* testable; and we shall regard it as more satisfactory the greater the severity of the independent tests it has survived.

I still have to elucidate my use of the expression 'independent', with its opposites, '*ad hoc*', and (in extreme cases) 'circular'.

Let a be an *explicandum*, known to be true. Since a trivially follows from a itself, we could always offer a as an explanation of itself. But this would be highly unsatisfactory, even though we should know in this case that the *explicans* is true, and that the *explicandum* follows from it. *Thus we must exclude explanations of this kind because of their circularity.*

Yet the kind of circularity I have here in mind is a matter of degree. Consider the following dialogue: 'Why is the sea so rough today?'—'Because Neptune is very angry'—'By what evidence can you support your statement that Neptune is very angry?'—'Oh, don't you *see* how *very* rough the sea is? And is it not always rough when Neptune is angry?' This explanation is found unsatisfactory because (just as in the case of the fully circular explanation) the only evidence for the *explicans* is the *explicandum* itself.[2] The feeling that this kind of almost circular or *ad hoc* explanation is highly unsatisfactory, and the corresponding requirement that explanations of this kind should be avoided are, I believe, among the main motive forces of the development of science: dissatisfaction is among the first fruits of the critical or rational approach.

In order that the *explicans* should not be *ad hoc*, it must be rich in content: it must have a variety of testable consequences, and among them, especially, testable consequences which are different from the *explicandum*. It is these different testable conse-

Berkeley as a Precursor of Mach', *Brit. Journ. Philos. Sc.* 4, 1953, p. 35. (Now in my *Conjectures and Refutations*, p. 174.)

[2] This kind of reasoning survives in Thales (Diels-Kranz[10], vol. i, p. 456, line 35); Anaximander (D.-K. A11, A28); Anaximenes (D.-K. A17, B1); Alcmaeon (D.-K. A5).

quences which I have in mind when I speak of *independent* tests, or of *independent* evidence.

Although these remarks may perhaps help to elucidate somewhat the intuitive idea of an independently testable *explicans*, they are still quite insufficient to characterize a satisfactory and independently testable explanation. For if *a* is our *explicandum*—let *a* be again 'The sea is rough today'—then we can always offer a highly unsatisfactory *explicans* which is completely *ad hoc* even though it has independently testable consequences. We can still choose these consequences as we like. We may choose, say, 'These plums are juicy' and 'All ravens are black'. Let *b* be their conjunction. Then we can take as *explicans* simply the conjunction of *a* and *b*: it will satisfy all our requirements so far stated.

Only if we require that explanations shall make use of universal statements or laws of nature (supplemented by initial conditions) can we make progress towards realizing the idea of independent, or non-*ad hoc*, explanations. For universal laws of nature *may* be statements with a rich content, so that *they may be independently tested* everywhere, and at all times. Thus if they are used as explanations, they *may* not be *ad hoc* because they *may* allow us to interpret the *explicandum* as an instance of a reproducible effect. All this is only true, however, if we confine ourselves to universal laws which are testable, that is to say, falsifiable.

The question 'What kind of explanation may be satisfactory?' thus leads to the reply: an explanation in terms of testable and falsifiable universal laws and initial conditions. And an explanation of this kind will be the more satisfactory the more highly testable these laws are and the better they have been tested. (This applies also to the initial conditions.)

In this way, the conjecture that it is the aim of science to find satisfactory explanations leads us further to the idea of improving the degree of satisfactoriness of the explanations by improving their degree of testability, that is to say, by proceeding to better testable theories; which means proceeding to theories of ever richer content, of higher degrees of universality, and of higher degrees of precision.[3] This, no doubt, is fully in keeping with the actual practice of the theoretical sciences.

[3] For the theory of *testability, content*, and *simplicity*, and of degrees of *universality* and *precision*, see sections 31 to 46 of my *Logic of Scientific Discovery*, 1959 (first

We may arrive at fundamentally the same result also in another way. If it is the aim of science to explain, then it will also be its aim to explain what so far has been accepted as an *explicans*; for example, a law of nature. Thus the task of science constantly renews itself. We may go on for ever, proceeding to explanations of a higher and higher level of universality—unless, indeed, we were to arrive at an *ultimate explanation*; that is to say, at an explanation which is neither capable of any further explanation, nor in need of it.

But are there ultimate explanations? The doctrine which I have called 'essentialism' amounts to the view that science must seek ultimate explanations in terms of essences:[4] if we can explain the behaviour of a thing in terms of its essence—of its essential properties—then no further question can be raised, and none need be raised (except perhaps the theological question of the Creator of the essences). Thus Descartes believed that he had explained physics in terms of the *essence of a physical body* which, he taught, was extension; and some Newtonians, following Roger Cotes, believed that the *essence of matter* was its inertia and its power to attract other matter, and that Newton's theory could be derived from, and thus ultimately explained by, these essential properties of all matter. Newton himself was of a different opinion. It was a hypothesis concerning the ultimate or essentialist causal explanation of gravity itself which he had in mind when he wrote in the *Scholium generale* at the end of the *Principia*: 'So far I have explained the phenomena . . . by the force of gravity, but I have not yet ascertained *the cause of gravity itself* . . . and I do not arbitrarily [or *ad hoc*] invent hypotheses.'[5]

I do not believe in the essentialist doctrine of ultimate explanation. In the past, critics of this doctrine have been, as a rule, instrumentalists: they interpreted scientific theories as *nothing*

German edn., 1934; fourth German edn., 1971), where the close connection between these ideas is explained.

[4] I have discussed (and criticized) essentialism more fully in my paper 'Three Views Concerning Human Knowledge', where I also refer to my earlier discussions (in the last footnote to section ii); see *Contemporary British Philosophy*, iii, edited by H. D. Lewis, 1956, note 2 on p. 365. (This paper forms now chapter 3 of my *Conjectures and Refutations*, third edn., 1969.)

[5] See also Newton's letters to Richard Bentley of 17 Jan. and especially 25 Feb. 1693 ('1692–3'). I have quoted from this letter in section iii of my paper 'Three Views Concerning Human Knowledge' (*Conjectures and Refutations*, pp. 106 f.) where the problem is discussed a little more fully.

but instruments for prediction, without any explanatory power. I do not agree with them either. But there is a third possibility, a 'third view', as I have called it. It has been well described as a 'modified essentialism'—with emphasis upon the word 'modified'.[6]

This 'third view' which I uphold modifies essentialism in a radical manner. First of all, I reject the idea of an ultimate explanation: I maintain that every explanation may be further explained, by a theory or conjecture of a higher degree of universality. There can be no explanation which is not in need of a further explanation, for none can be a self-explanatory description of an essence (such as an essentialist definition of body, as suggested by Descartes). Secondly, I reject all *what-is questions*: questions asking what a thing is, what is its essence, or its true nature. For we must give up the view, characteristic of essentialism, that in every single thing there is an essence, an inherent nature or principle (such as the spirit of wine in wine), which necessarily causes it to be what it is, and thus to act as it does. This animistic view explains nothing; but it has led essentialists (like Newton) to shun relational properties, such as gravity, and to believe, on grounds felt to be *a priori* valid, that a satisfactory explanation must be in terms of inherent properties (as opposed to relational properties). The third and last modification of essentialism is this. We must give up the view, closely connected with animism (and characteristic of Aristotle as opposed to Plato), that it is the essential properties inherent *in each individual or singular thing* which may be appealed to as the explanation of this thing's behaviour. For this view completely fails to throw any light whatever on the question why different individual things should behave in like manner. If it is said, 'because their essences are alike', the new question arises: *why should there not be as many different essences as there are different things?*

Plato tried to solve precisely this problem by saying that like

[6] The term 'modified essentialism' was used as a description of my own 'third view' by a reviewer of my paper 'Three Views Concerning Human Knowledge' in *The Times Literary Supplement*, 55, 1956, p. 527. In order to avoid misunderstandings, I wish to say here that my acceptance of this term should not be construed as a concession to the doctrine of 'ultimate reality', and even less as a concession to the doctrine of essentialist definitions. I fully adhere to the criticism of this doctrine which I have given in my *Open Society*, vol. ii, chapter 11, section ii (especially note 42), and in other places.

individual things are the offspring, and thus copies, of the same original 'Form', which is therefore something 'outside' and 'prior' and 'superior' to the various individual things; and indeed, we have as yet no better theory of likeness. Even today, we appeal to their common origin if we wish to explain the likeness of two men, or of a bird and a fish, or of two beds, or two motor cars, or two languages, or two legal procedures; that is to say, we explain similarity in the main genetically; and if we make a metaphysical system out of this, it is liable to become a historicist philosophy. Plato's solution was rejected by Aristotle; but since Aristotle's version of essentialism does not contain even a hint of a solution, it seems that he never quite grasped the problem.[7]

By choosing explanations in terms of universal laws of nature, we offer a solution to precisely this last (Platonic) problem. For we conceive all individual things, and all singular facts, to be subject to these laws. The laws (which in their turn *are* in need of further explanation) thus explain regularities or similarities of individual things or singular facts or events. And these laws are not inherent in the singular things. (Nor are they Platonic ideas outside the world.) Laws of nature are conceived, rather, as (conjectural) descriptions of the structural properties of nature—of our world itself.

Here then is the similarity between my own view (the 'third view') and essentialism; although I do not think that we can ever describe, by our universal laws, an *ultimate* essence of the world, I do not doubt that we may seek to probe deeper and deeper into the structure of our world or, as we might say, into properties of the world that are more and more essential, or of greater and greater depth.

Every time we proceed to explain some conjectural law or theory by a new conjectural theory of a higher degree of universality, we are discovering more about the world, trying to penetrate deeper into its secrets. And every time we succeed in falsifying a theory of this kind, we make a new important discovery. For these falsifications are most important. They teach us the unexpected; and they reassure us that, although our

[7] As to Plato's theory of Forms or Ideas, it is 'one of its most important functions to explain the similarity of sensible things . . .'; cf. my *Open Society*, chapter 3, section v; see also notes 19 and 20, and text. The failure of Aristotle's theory to perform this function is mentioned there (in the third edn., 1957) at the end of note 54 to chapter 11.

theories are made by ourselves, although they are our own inventions, they are none the less genuine assertions about the world; for they can *clash* with something we never made.

Our 'modified essentialism' is, I believe, helpful when the question of the logical form of natural laws is raised. It suggests that our laws or our theories must be *universal*, that is to say, must make assertions about the world—about all spatio-temporal regions of the world. It suggests, moreover, that our theories make assertions about structural or relational properties of the world; and that the properties described by an explanatory theory must be, in some sense or other, deeper than those to be explained. I believe that this word 'deeper' defies any attempt at exhaustive logical analysis, but that it is nevertheless a guide to our intuitions. (This is so in mathematics: all its theorems are logically equivalent, in the presence of the axioms, and yet there is a great difference in 'depth' which is hardly susceptible of logical analysis.) The 'depth' of a scientific theory seems to be most closely related to its simplicity and so to the wealth of its content. (It is otherwise with the depth of a mathematical theorem, whose content may be taken to be nil.) Two ingredients seem to be required: a rich content, and a certain coherence or compactness (or 'organicity') of the state of affairs described. It is this latter ingredient which, although it is intuitively fairly clear, is so difficult to analyse, and which the essentialists were trying to describe when they spoke of essences, in contradistinction to a mere accumulation of accidental properties. I do not think that we can do much more than refer here to an intuitive idea, nor that we need do much more. For in the case of any particular theory proposed, it is the wealth of its content, and thus its degree of testability, which decides its interest, and the results of actual tests which decide its fate. From the point of view of method, we may look upon its depth, its coherence, and even its beauty, as a mere guide or stimulus to our intuition and to our imagination.

Nevertheless, there does seem to be something like a *sufficient* condition for depth, or for degrees of depth, which can be logically analysed. I shall try to explain this with the help of an example from the history of science.

It is well known that Newton's dynamics achieved a unification of Galileo's terrestrial and Kepler's celestial physics. It is often said that Newton's dynamics can be induced from Galileo's and Kepler's laws, and it has even been asserted that it can be strictly deduced from them.[8] But this is not so; from a logical point of view, Newton's theory, strictly speaking, contradicts both Galileo's and Kepler's (although these latter theories can of course be obtained as approximations, once we have Newton's theory to work with). For this reason it is impossible to derive Newton's theory from either Galileo's or Kepler's or both, whether by deduction or induction. For neither a deductive nor an inductive inference can ever proceed from consistent premises to a conclusion that formally contradicts the premises from which we started.

I regard this as a very strong argument against induction.

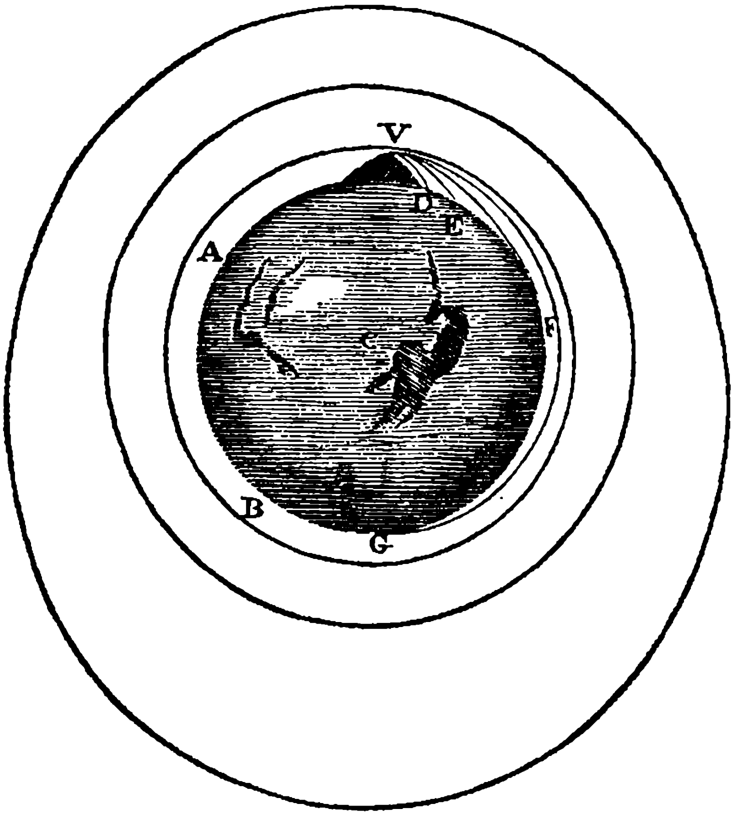
I shall now briefly indicate the contradictions between Newton's theory and those of his predecessors. Galileo asserts that a thrown stone or a projectile moves in a parabola, except in the case of a free vertical fall when it moves, with constant acceleration, in a straight line. (We neglect air-resistance throughout this discussion.) From the point of view of Newton's theory, these assertions are both false, for two distinct reasons. The first is false because the path of a long-range projectile, such as an inter-continental missile (thrown in an upward or horizontal direction) will be not even approximately parabolic but elliptic. It becomes, approximately, a parabola only if the total distance of the flight of the projectile is negligible compared with the radius of the earth. This point was made by Newton himself, in his *Principia*, as well as in his popularized version, *The System of*

[8] What can be deduced from Kepler's laws (see Max Born, *Natural Philosophy of Cause and Chance*, 1949, pp. 129–33) is that, for all planets, the acceleration towards the sun equals at any moment k/r^2, where r is the distance at that moment between the planet and the sun, and k a constant, the same for all planets. Yet this very result formally contradicts Newton's theory (except on the assumption that the masses of the planets are all equal or, if unequal, then at any rate infinitely small as compared with the mass of the sun). This fact follows from what is here said, in the text following note 10, about Kepler's third law. But in addition, it should be remembered that neither Kepler's nor Galileo's theories contains Newton's concept of *force*, which is traditionally introduced in these deductions without further ado; as if this ('occult') concept could be read off from the facts, instead of being the result of a new interpretation of the facts (that is, of the 'phenomena' described by Kepler's and Galileo's laws) in the light of a completely new theory. Only after the concept of force (and even the proportionality of gravitational and inertial mass) has been introduced is it at all possible to link the above formula for the acceleration with Newton's inverse square law of attraction (by the assumption that the planets' masses are negligible).

the World, where he illustrates it with the help of the figure reproduced on this page.

Newton's figure illustrates his statement that, if the velocity of the projectile increases, and with it the distance of its flight, it will 'at last, exceeding the limits of the earth, . . . pass into space without touching it'.[9]

Thus a projectile on earth moves along an ellipse rather than



a parabola. Of course, for sufficiently short throws, a parabola will be an excellent approximation; but the parabolic track is not strictly deducible from Newton's theory unless we add to the latter a factually *false* initial condition (and one which, incidentally, is unrealizable in Newton's theory since it leads to absurd consequences) to the effect that the radius of the earth is infinite. If we do not admit this assumption, even though it is *known to be false*, then we always get an ellipse, in contradiction to Galileo's law according to which we should obtain a parabola.

A precisely analogous logical situation arises in connection with the second part of Galileo's law which asserts the existence

[9] See Newton's *Principia*, the *Scholium* at the end of section ii of Book i; p. 55 of the 1934 edn. (Motte's translation revised by Cajori). The figure, from *The System of the World*, and the quotation here given, will be found on p. 551 of this edn.

of an acceleration *constant*. From the point of view of Newton's theory, the acceleration of free-falling bodies is never constant: it always increases during the fall, owing to the fact that the body approaches nearer and nearer to the centre of attraction. This effect is very considerable if the body falls from a great height, although of course negligible if the height is negligible as compared with the radius of the earth. In this case, we can obtain Galileo's theory from Newton's if we again introduce the *false* assumption that the radius of the earth is infinite (or the height of the fall zero).

The contradictions which I have pointed out are far from negligible for long-distance missiles. To these we may apply Newton's theory (with corrections for air resistance, of course) but not Galileo's: the latter leads simply to false results, as can easily be shown with the help of Newton's theory.

With respect to Kepler's laws, the situation is similar. It is obvious that in Newton's theory Kepler's laws are only approximately valid—that is, strictly invalid—if we take into account the mutual attraction between the planets.[10] But there are more fundamental contradictions between the two theories than this somewhat obvious one. For even if, as a concession to our opponents, we neglect the mutual attraction between the planets, Kepler's third law, considered from the point of view of Newton's dynamics, cannot be more than an approximation which is applicable to a very special case: to planets whose masses are equal, or, if unequal, negligible as compared with the mass of the sun. Since it does not even approximately hold for two planets if one of them is very light while the other is very heavy, it is clear that Kepler's third law contradicts Newton's theory in precisely the same way as does Galileo's.

This can be easily shown as follows. Newton's theory yields for a two-body system—a binary star system—a law which astronomers often call 'Kepler's law' since it is closely related

[10] See, for example, P. Duhem, *The Aim and Structure of Physical Theory*, 1906; English translation by P. P. Wiener, 1954, Part II, chapter vi, section 4. Duhem says more explicitly what is implicit in Newton's own statement (*Principia*, Book I, proposition lxv, theorem xxv), for Newton makes it quite clear that in cases where more than two bodies interact, Kepler's first two laws will be at best only approximately valid, and even this in very special cases only, of which he analyses two in some detail. Incidentally, formula (1), below, follows immediately from Book I, proposition lix, in view of Book I, proposition xv. (See also Book III, proposition xv.)

to Kepler's third law. This so-called 'Kepler's law' says that if m_0 is the mass of one of the two bodies—say, the sun—and if m_1 is the mass of the other body—say, a planet—then, choosing appropriate units of measurement, we can derive from Newton's theory

(1) $$a^3/T^2 = m_0 + m_1,$$

where a is the mean distance between the two bodies, and T the time of a full revolution. Now Kepler's own third law asserts that

(2) $$a^3/T^2 = \mathit{constant}$$

that is to say, the same constant for *all* planets of the solar system. It is clear that we obtain this law from (1) only under the assumption that $m_0 + m_1 =$ constant; and since $m_0 =$ constant for our solar system if we identify m_0 with the mass of the sun, we obtain (2) from (1), provided we assume that m_1 is the same for all planets; or, if this is factually *false* (as is indeed the case, since Jupiter is several thousand times larger than the smallest planets), that the masses of the planets are *all zero as compared with that of the sun*, so that we may put $m_1 = 0$, *for all planets*. This is quite a good approximation from the point of view of Newton's theory; but at the same time, putting $m_1 = 0$ is not only strictly speaking false, but unrealizable from the point of view of Newton's theory. (A body with zero mass would no longer obey Newton's laws of motion.) Thus, even if we forget all about the mutual attraction between the planets, Kepler's third law (2) contradicts Newton's theory which yields (1).

It is important to note that from Galileo's or Kepler's theories we do not obtain even the slightest hint of how these theories would have to be adjusted—what false premisses would have to be adapted, or what conditions stipulated—should we try to proceed from these theories to another and more generally valid one such as Newton's. *Only after we are in possession of Newton's theory can we find out whether, and in what sense, the older theories can be said to be approximations to it.* We may express this fact briefly by saying that, although from the point of view of Newton's theory, Galileo's and Kepler's are excellent approximations to certain special Newtonian results, Newton's theory cannot be said, from the point of view of the other two theories, to be an approximation to their results. All this shows that logic, whether deductive or inductive, cannot possibly make the step from these theories to Newton's dynamics.[11] It is only ingenuity which can make this step. Once it has been made, Galileo's and Kepler's results may be said to corroborate the new theory.

Here, however, I am not so much interested in the impossibility of induction as in *the problem of depth.* And regarding this problem, we can indeed learn something from our example. Newton's theory unifies Galileo's and Kepler's. But far from being a mere conjunction of these two theories—which play the part of *explicanda* for Newton's—*it corrects them while explaining them.* The original explanatory task was the deduction of the earlier results. Yet this task is discharged, not by deducing these earlier results but by deducing something better in their place: new results which, under the special conditions of the older results, come numerically very close to these older results, and at the same time correct them. Thus the empirical success of the old theory may be said to corroborate the new theory; and in addition, the corrections may be tested in their turn—and perhaps refuted, or else corroborated. What is brought out strongly, by the logical situation which I have sketched, is the fact that the new theory cannot possibly be *ad hoc* or circular. Far from repeating its *explicandum*, the new theory contradicts it, and corrects it. In this way, even the evidence of the *explicandum* itself becomes independent evidence for the new theory. (Incidentally, this analysis allows us to *explain the value of metrical theories*, and of measurement; and it thus helps us to avoid the mistake of accepting measurement and precision as ultimate and irreducible values.)

I suggest that whenever in the empirical sciences a new theory of a higher level of universality successfully explains some older theory *by correcting it*, then this is a sure sign that the new theory has penetrated deeper than the older ones. The demand that a new theory should contain the old one approximately, for appropriate values of the parameters of the new theory, may be called (following Bohr) the '*principle of correspondence*'.

Fulfilment of this demand is a sufficient condition of depth, as I said before. That it is not a necessary condition may be seen from the fact that Maxwell's electromagnetic wave theory did

[11] The concepts of force (cp. p. 198, note 8, above) and of action at a distance introduce further difficulties.

not correct, in this sense, Fresnel's wave theory of light. It meant an increase in depth, no doubt, but in a different sense: 'The old question of the direction of the vibrations of polarized light became pointless. The difficulties concerning the boundary conditions for the boundaries between two media were solved by the very foundations of the theory. No *ad hoc* hypotheses were needed any longer for eliminating longitudinal light waves. Light pressure, so important in the theory of radiation, and only lately determined experimentally, could be derived as one of the consequences of the theory.'[12] This brilliant passage, in which Einstein sketches some of the major achievements of Maxwell's theory and compares it with Fresnel's, may be taken as an indication that there are other sufficient conditions of depth which are not covered by my analysis.

The task of science, which, I have suggested, is to find satisfactory explanations, can hardly be understood if we are not realists. For a satisfactory explanation is one which is not *ad hoc*; and this idea—the *idea of independent evidence*—can hardly be understood without the idea of discovery, of progressing to deeper layers of explanation: without the idea that there is something for us to discover, and something to discuss critically.

And yet it seems to me that within methodology we do not have to presuppose metaphysical realism; nor can we, I think, derive much help from it, except of an intuitive kind. For once we have been told that the aim of science is to explain, and that the most satisfactory explanation will be the one that is most severely testable and most severely tested, we know all that we need to know as methodologists. That the aim is realizable we cannot assert, neither with nor without the help of metaphysical realism which can give us only some intuitive encouragement, some hope, but no assurance of any kind. And although a rational treatment of methodology may be said to depend upon an assumed, or conjectured, aim of science, it certainly does not depend upon the metaphysical and most likely false assumption

[12] A. Einstein, *Physikalische Zeitschrift*, **10**, 1909, pp. 817 f. The abandonment of the theory of a material ether (implicit in Maxwell's failure to construct a satisfactory material model of it) may be said to give depth, in the sense analysed above to Maxwell's theory as compared with Fresnel's; and this is, it seems to me, implicit in the quotation from Einstein's paper. Thus Maxwell's theory in Einstein's formulation is perhaps not really an example of *another* sense of 'depth'. But in Maxwell's own original form it is, I think.

that the true structural theory of the world (if any), is discoverable by man, or expressible in human language.

If the picture of the world which modern science draws comes anywhere near to the truth—in other words, if we have anything like 'scientific knowledge'—then the conditions obtaining almost everywhere in the universe make the discovery of structural laws of the kind we are seeking—and thus the attainment of 'scientific knowledge'—almost impossible. For almost all regions of the universe are filled by chaotic radiation, and almost all the rest by matter in a similar chaotic state. In spite of this, science has been miraculously successful in proceeding towards what I have suggested should be regarded as its aim. This strange fact cannot, I think, be explained without proving too much. But it can encourage us to pursue that aim, even though we may not get any further encouragement to believe that we can actually attain it; neither from metaphysical realism nor from any other source.

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BIBLIOGRAPHICAL NOTE

The idea here discussed that theories may *correct* an 'observational' or 'phenomenal' law which they are supposed to explain (such as, for example, Kepler's third law) was repeatedly expounded in my lectures. One of these lectures stimulated the correction of a supposed phenomenal law (see the 1941 paper referred to in my *Poverty of Historicism*, 1957, 1960, footnote on pp. 134 f.). Another of these lectures was published in Simon Moser's volume *Gesetz und Wirklichkeit* (1948), 1949, and is translated as Appendix 1 in the present volume. The same idea of mine was also the 'starting-point' (as he puts it on p. 92) of P. K. Feyerabend's paper 'Explanation, Reduction and Empiricism' (in Herbert Feigl and Grover Maxwell, editors, *Minnesota Studies in the Philosophy of Science*, **3**, 1962) whose reference [66] is to the present paper (as first published in *Ratio*, **1**, 1957). Feyerabend's acknowledgement seems to have been overlooked by the authors of various papers on related subjects.