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MODELS IN PHYSICS *

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1. *The Hypothetico-Deductive Method*

THE logical form of the theories of mathematical physics has been described in a previous article ' Operational Definition and Analogy in Physical Theories '[1] where, following N. R. Campbell, F. P. Ramsey and other writers, I have maintained that scientific theories are not constructed solely out of sense-data or out of operational definitions, but are ' hypothetico-deductive '[2] in form ; that is, they consist of hypotheses which may not in themselves have any reference to immediate observations, but from which deductions can be drawn which correspond to the results of experiments when suitably translated into the experimental language. The main point that emerges from such a description of theories is that there can be no set of rules given for the procedure of scientific discovery—a hypothesis is not produced by a deductive machine by feeding experimental observations into it : it is a product of creative imagination, of a mind which absorbs the experimental data until it sees them fall into a pattern, giving the scientific theorist the sense that he is penetrating beneath the flux of phenomena to the real structure of nature. In the present article I shall examine this hypothetico-deductive method further by considering some examples from nineteenth-century mathematical physics—examples which have the advantage for the logician of being comparatively elementary as regards the science and the mathematics, but which demonstrate the typical method of modern science. By means of these examples I hope to bring out two points about scientific hypotheses, the significance of which seems to have been generally overlooked. I shall first state these two points and discuss them in a preliminary way, and then go on to bring evidence in their support from the examples.

I. *Mathematical formalisms, when used as hypotheses in the description of physical phenomena, may function like the mechanical models of an*

* Received 20. viii. 52

[1] This *Journal*, 1952, **2**, 281

[2] See K. R. Popper, *Logik der Forschung*, Vienna, 1935 and J. O. Wisdom, *Foundations of Inference in Natural Science*, London, 1952, pp. 25 ff.

earlier stage in physics, without having in themselves any mechanical or other physical interpretation. What this means can be explained briefly as follows.

If a hypothesis is to be a satisfactory correlation of a group of experimental data, it must be possible to deduce the data from the hypothesis when the symbols in the latter are suitably interpreted. If the hypothesis is to be a useful instrument of further research, however, this is not the only requirement that it must satisfy. In addition it is necessary that the hypothesis itself should be capable of being thought about, modified and generalised, without necessary reference to the experiments, so that it can be used to predict future experience, or to bring hitherto isolated experimental results into the field of application of the hypothesis. This second condition is fulfilled in the case of the mechanical hypotheses typical of nineteenth-century physics, because they are expressed in terms of mechanical models whose behaviour is known apart from the experimental facts to be explained. The billiard ball model of gas molecules, for instance, consists of a collection of balls moving at random and colliding with each other and with the walls of the vessel, and the behaviour of such a system is already known and expressed in a mathematical theory, independently of the experimental results about gases with which it is compared. This means that further ramifications of the theory of colliding billiard balls can be used to extend the theory of gases, and questions can be asked such as 'Are gas molecules like rigid balls or like elastic ones?', 'What is their approximate diameter?', and so on. Progress is made by devising experiments to answer questions suggested by the model.[1] Similar considerations apply to models from electrical theory. Certain atomic hypotheses, for example, are expressed in terms of a model consisting of charged particles moving under their mutual electrostatic attractions, and the theory of such particles is known independently of the atomic phenomena it is used to explain. Now in a great deal of modern physics on the other hand, we are told that we must not ask for picturable mechanical or electrical models such as these, and that only formal mathematical hypotheses[2] are adequate

[1] Hutten expresses this by saying that the model provides unformalised 'semantic rules' for the theory. See his article 'On Semantics and Physics', *Proc. Aris. Soc.*, 1948-9, **49**, 115.

[2] Such as the transformation group theory in quantum physics. See the preface to Dirac's *Quantum Theory*, 2nd. edn., Oxford, 1935, p. vi.

to express the physical theory. The question then arises, what takes the place in these physical theories of the pointers towards further progress which are provided by an easily pictured mechanical model? I shall suggest that what takes their place is provided by the nature of the mathematical formalism itself—any particular piece of mathematics has its own ways of suggesting modification and generalisation; it is not an isolated collection of equations having no relation to anything else, but is a recognisable part of the whole structure of abstract mathematics, and this is true whether the symbols employed have any concrete physical interpretation or not. Examples of the way this works in the extension of hypotheses will be given in what follows.

It remains to decide the linguistic point, as to whether such abstract mathematical hypotheses are to be called 'models'. It does not seem profitable to try to draw any sharp line between those hypotheses which are expressed as mechanical or electrical or hydrodynamical (wave theories, etc.) models, and those which are purely mathematical. There are many hybrids, like the Bohr model of the atom in which electrons are conceived to jump discontinuously from one orbit to another, a feat which no mechanical particle can be imagined to perform; and again the conception of curvature of three-dimensional space, which cannot be imagined apart from the mathematical formulation, although the imagination can be aided by thinking of a *two* dimensional surface curved in a third dimension. Current physical usage seems to sanction the use of 'model' for all such cases whether physically imaginable or not. For instance, the various cosmological or world-models [1] are sometimes physically imaginable distributions of matter, but the more recent models involve some element, such as Einstein's 'finite but unbounded' space, which cannot be physically imagined, but only expressed mathematically.

However, the main justification for the use of the word 'model' in the wide sense is the fact, which we are attempting to establish here, that theories of a purely mathematical kind may function in essentially the same way as physically imaginable models, in being capable of suggesting further lines of development in the explanation of the experimental facts. It is sometimes possible to label models unequivocally as 'mechanical', 'electrical', 'mathematical', and so on, but more usually a model will be a mixture of several types. In this use

[1] As described for instance in Whitrow's, *The Structure of the Universe*, London, 1950.

the word ' model ' becomes co-extensive with the word ' hypothesis ', but is more suggestive, since it calls attention to the heuristic properties of the early mechanical models, and asserts that these properties must be found in any hypothesis which is scientifically useful.

II. The second point to be illustrated below by examples from nineteenth-century physics is the relation between models and the natural phenomena which they are used to explain. I shall suggest that *most physicists do not regard models as literal descriptions of nature, but as standing in a relation of analogy to nature.* The word ' analogy ', however, needs some discussion, as it is used in a variety of senses.

In the literature of physics two important meanings of ' analogy ' may be distinguished.

(*a*) It may be said that there is an anology between two branches of physics in virtue of the fact that the same mathematical formalism appears in the theory of both. An example is the analogy between the theory of heat and that of electrostatics, which, as Kelvin first showed,[1] can be described by the same equations if one reads ' temperature ' for ' potential ', ' source of heat ' for ' positive electric charge ', and so on. In other words, the mathematical structure of the two theories is the same, and consequently one theory may be used as a *model* for the other, as the theory of heat, which was already worked out, was used by Kelvin as a model for the field theory of electrostatics, which he was developing for the first time.

(*b*) ' Analogy ' may refer also to a model such as the billiard ball model of gas molecules. In this case the term expresses the relation between billiard balls and gas molecules, and the model itself (the billiard balls), being one of the relata, is an ' analogue ' of the gas molecules. When we say in this way that there is an analogy between a model and certain phenomena of nature, we are in some sense asserting an identity of mathematical structure between the model and nature, as in sense (*a*) we are asserting such an identity between two theories. But here the meaning of the assertion is obscure unless we can show how to determine the ' mathematical structure of nature '. All that the physicist can *certainly* determine about nature are experimental results, usually expressed by measurements, and therefore the assertion of an analogy must mean at least that there are resemblances between these results and the model. The resemblances are in fact correspondences between the observed

[1] *Papers on Electrostatics and Magnetism*, London, 1884, p. 1

measurements and certain numbers deduced from the model; for example, if the appropriate calculations based on the theory of mechanics are made about the energy of colliding billiard balls, we can obtain a numerical value which is the same as that shown on the scale of a thermometer placed in a vessel containing a gas. Several such numerical correspondences obtained under various conditions are sufficient in physics for the assertion of an analogy between the model and the experimental facts, the analogy consisting of the correspondence between numerical consequences of the model, and numerical experimental results. It follows that there may be any number of models of the same set of facts, all yielding similar numbers which agree with experiment.

These two senses of ' analogy ', to be found in the literature of physics, lead us to adopt the following definition:

> An analogy in physics is a relation, either between two hypotheses, or between a hypothesis and certain experimental results, in which certain aspects of both relata can be described by the same mathematical formalism.

Such a definition is not too far removed from that use of ' analogy ' in modern logic in which it means a relation between entities having some characteristics in common.[1] Thus Mill speaks of an analogy between the moon and the earth, the moon being ' a solid, opaque, nearly spherical substance; . . . receiving heat and light from the sun, in about the same quantity as our earth; revolving on its axis; composed of materials which gravitate, . . .' (op. cit., p. 90). It is convenient for purposes of the logic of physics to limit this use of ' analogy ' as similarity in general, to similarity of mathematical structure.

To say that a model is in general an analogue and not a literal description of nature may in certain cases mean no more than that the model is an idealisation of nature. In classical mechanics, the entities discussed, such as smooth planes, perfectly rigid spheres, and the like, are not literal descriptions of anything to be found in nature, but are simplifications of natural objects arrived at by neglecting all but a few properties of the objects—properties which are selected because they are amenable to mathematical treatment. There is an analogy between a dynamically rigid sphere and a cricket ball in the

[1] e.g. Mill, *A System of Logic*, 10th edn., London, 1879, vol. 2, pp. 87 ff.; Keynes, *A Treatise on Probability*, London, 1921, p. 222

sense that the same mathematical formalism describes the behaviour of both more or less accurately. But the approximation involved in applying mathematics to nature is not the main point which it is desired to emphasise in calling a model an analogue of nature. The main point becomes significant only when the model is used in theories such as atomic physics where the entities discussed, protons, neutrons, etc., are, unlike cricket balls, observable only in virtue of their remote effects : it is as if the properties of cricket balls were known to us, not by seeing and handling them, but only by hearing a sharp impact as a batsman hits out and observing shattered windows. To speak of *atomic particles* at all is to employ a model based on dynamics and electrostatics, and such a model is not simply an idealisation of something which is observed but is too complicated to deal with as it exists in nature ; but is a hypothesis adopted because deductions from it, sometimes very remote deductions, do yield numbers comparable to the experimental measurements. All that can then be said with certainty is that there is a similarity of mathematical structure between the model and the experiments, in other words, there is a relation of analogy in the sense which has been defined above.

The difference between analogy and literal description can be illustrated by another familiar model in physics : that of two gravitating masses, attracting each other along the line joining them by a force inversely proportional to the square of the distance between them. This fulfils the necessary conditions for a model, in that there is a well-worked-out mathematical theory, the theory of potential, which provides rules of manipulation for the concepts. Now when we attempt to use the gravitational model in a theory about a different range of physical phenomena, say the interaction of the fundamental particles, there are two types of question that can be asked about the relation between the model and the natural phenomena which it describes :

(i) Does matter *really* consist of systems of particles acting on one another according to the inverse square law ?

(ii) Is the behaviour of the fundamental particles more *analogous* to a system of gravitating particles than to anything else with which we are well acquainted ? This, if the relation of analogy is taken to mean similarity of mathematical structure in the sense explained above, means in effect, asking whether the gravitational theory provides the most suitable mathematical language system within which to correlate the behaviour of the fundamental particles.

The distinction between the two types of question can be illustrated by the work of some of the physicists of the last century, and it is instructive to inquire which of the two questions they believed themselves to be answering. Nineteenth-century physics is commonly designated ' mechanist ', by which is usually meant that a question of the type (i) above is to be answered in terms of, to use Larmor's phrase, ' the pressures and thrusts of the engineer, and the stresses and strains in the material structures by which he transmits them from one place to another ',[1] as if matter *really* consisted of tiny Meccano model cogs, gyrostats and spiral springs. Now in fact, judging by the incidental remarks about method which are to be found in the writings of the nineteenth-century mathematical physicists (and they are fairly free with their logical comments upon what they are doing), they were often quite clear about the significance of the mechanical models which they constructed to embody their equations. They knew, in other words, that the question to be asked was question (ii) ; their so-called ' mechanistic ' outlook arose, not from a false metaphysics, but from the fact that they were not yet familiar with mathematical languages other than those of classical mechanics and hydrodynamics, and that they therefore tended to express their hypotheses always in terms of those languages.

2 *Hypothesis in Nineteenth-century Physics*

It would be easy to compile a list of quotations apparently showing that, far from having a naïve faith in the reality of models, many of the great nineteenth-century theorists held a positivist theory of science long before Mach laid down his classic statement of scientific positivism in the 1900s.[2] Fourier, for example, is at pains to point out in the *Théorie analytique de la chaleur* that his equation of the conduction of heat, and all the mathematical results he derives from it, are quite independent of any theory of the physical manner in which heat is communicated from one part of a body to another. All that is necessary is to lay down the principle that the rate of flow of heat between two surfaces of the body is proportional to their

[1] *British Association Report*, London, 1900, p. 618

[2] Comte wrote in the 1850s, but he was not himself a scientist, and although he was undoubtedly read by many scientists it is not clear that he had much direct influence on their philosophy of science.

difference of temperature ; both rate of heat flow and temperature difference being quantities that can be measured at the surface of a body. The only further assumption involved is that the flow and the temperature are continuous within the body where measurements cannot be made, in other words, that heat is analogous to a continuously flowing liquid. But it is not necessary to enquire further into its nature ; nothing would be added to the mathematical description of the observations by doing so : 'In whatever manner we please to imagine the nature of this element [heat], whether we regard it as a distinct material thing which passes from one part of space to another, or whether we make heat consist simply in the transfer of motion, we shall always arrive at the same equations.' [1]

Again, the development of the theory of elastic solids was confused by the molecular hypotheses of the earlier writers. Poisson, assuming definite laws of action between the molecules of a substance, arrived at equations for the stress-strain relationship which implied that the modulus of compression of a solid is always a constant multiple of its rigidity. This holds only for so-called ideal solids where the molecules are displaced regularly with respect to each other under strain. Stokes, on the other hand, always cautious with regard to untestable hypotheses, developed a more adequate theory of elasticity [2] by considering solids to be continuous on the average if the equations are confined to volumes containing many molecules, and by then treating the subject as an extension of the theory of continuous viscous fluids. As in Fourier's theory of heat, the observed phenomena can be explained without entering into further details about exactly how stress is communicated from molecule to molecule. All this seems very similar to modern operationalism, but it will be noticed that in each of the cases quoted above some assumption, namely, of continuity of heat-flow or of stress within material substances, is involved, and the assumption is not a *deduction* from observations—it is properly a hypothesis or model.

3 *Theories of the Luminiferous Aether*

The necessity for hypotheses becomes even clearer when we consider more ambitious theories such as those concerning the transmission of light. It was universally believed that light was transmitted

[1] Fourier, *The Analytical Theory of Heat*, Cambridge, 1878, p. 464

[2] Stokes, *Mathematical and Physical Papers*, I, Cambridge, 1845, p. 75

through space as a wave-motion in a material substance called the aether, but this unobservable concept did not cramp the theory as much as we sometimes imagine. The great problem was to see how a substance, having at least some of the properties of matter as we know it, could transmit a wave-motion having the observed properties of light. In the first instance, efforts were directed towards constructing a *mathematical* formalism which would lead to the correct laws of refraction, reflection, polarisation, and the rest, without necessarily describing a corresponding physical mechanism. Green, in a paper of 1838,[1] introduces his theory as follows :

> . . . we are so perfectly ignorant of the mode of action of the elements of the luminiferous ether on each other, that it would seem a safer method to take some general physical principle as the basis of our reasoning, rather than assume certain modes of action, which, after all, may be widely different from the mechanism employed by nature ; more especially if this principle . . . lead to a much more simple process of calculation.

The principle in question, from which Green proceeds to deduce the equation of motion of the medium and to derive the laws of reflection and refraction, is the principle of stationary action, which in Lagrange's analytical treatment of Newton's mechanics is the starting point from which the laws of mechanics may be deduced. In other words, although Green makes no attempt to describe the precise mechanical action of the aether, he assumes that its action is mechanical in the sense of Newton's theory, and starts from the most general equation consistent with that assumption. His model is not a physically picturable model of colliding billiard balls or vibrating springs, it is the mathematical formalism of Lagrange's analytical mechanics.

The Irish mathematician MacCullagh indulges in even more obvious wresting of the mathematics in order to make it fit the phenomena, irrespective of whether the resulting hypothesis has any physical meaning or not. He is quite explicit about his method ; in a paper on 'A dynamical theory of crystalline reflection and refraction' [2] he states that his aim is to deduce the laws of propagation and reflection of light in crystals from common principles by the methods of analytical dynamics. He makes one or two limited assumptions about the aether, namely, that its density is constant everywhere, that

[1] Green, *Mathematical Papers*, Paris, 1903, p. 245

[2] MacCullagh, *Trans. Roy. Irish Acad.*, 1848, **21**, 17

it can be regarded on the average as continuous, and that the mutual action of its particles is negligible at distances comparable with a wave-length of light. He then writes down the most general mechanical equation of such a medium in its variational form, derived from the principle of stationary action :

$$\int\left(\frac{\partial^2 u}{\partial t^2}\,\delta u + \frac{\partial^2 v}{\partial t^2}\,\delta v + \frac{\partial^2 w}{\partial t^2}\,\delta w\right) dxdydz = \int \delta \mathrm{V}\, dxdydz.$$

Here the left-hand side refers to the kinetic energy of the displaced particles of the medium, and V is their potential energy depending on their mutual actions and reactions. Now no physical assumption has been made about these actions, and MacCullagh proceeds to find a function V such that the equation will represent what is known observationally of the propagation of light waves in crystals. The function turns out to be one having no simple interpretation in terms of the actions of aether particles, but 'having arrived at the value of V, we may now take it for the starting point of our theory, and dismiss the assumptions by which we were conducted to it'. Of the validity of this procedure MacCullagh says :

> In this theory, everything depends on the form of the function V. . . . But the reasoning which has been used to account for the form of the function is indirect, and cannot be regarded as sufficient in a mechanical point of view. It is, however, the only kind of reasoning which we are able to employ, as the constitution of the luminiferous medium is entirely unknown.[1]

This reasoning uses the formalism of analytical mechanics as a model, generalising the function V, which was originally the potential energy calculated from a mechanical model, to be any function admissable in the variational equation. The form of V derived was not in this case merely a convenient mathematical summary of the phenomena which MacCullagh set out to explain, for he later found that it led to the correct equations for a new set of phenomena, those of total reflection. Thus the mathematical formalism functions in all respects like a model which can be thought about and generalised independently of the data it is being used to explain. Such reasoning from mathematical models is common at this period, especially in MacCullagh's work. Another example is the attempt to formulate a theory for the rotation of the plane of polarisation of light when it

[1] ibid., p. 50

passes (*a*) through a crystal, and (*b*) through a transparent magnetised body. The equations of rectilinear light propagation in a vacuum are the well-known wave equations :

$$\frac{\partial^2 Y}{\partial t^2} = c^2 \frac{\partial^2 Y}{\partial x^2}, \quad \frac{\partial^2 Z}{\partial t^2} = c^2 \frac{\partial^2 Z}{\partial x^2},$$

where Y, Z, stand for the components along the *y*- and *z*-axes respectively of a periodically varying quantity which is associated with the light and which need not be further defined at the moment. MacCullagh showed [1] that the phenomena observed when light passes through a crystal can be deduced from a modification of the above equations :

$$\frac{\partial^2 Y}{\partial t^2} = c^2 \frac{\partial^2 Y}{\partial x^2} + a \frac{\partial^3 Z}{\partial x^3}, \quad \frac{\partial^2 Z}{\partial t^2} = c^2 \frac{\partial^2 Z}{\partial x^2} - a \frac{\partial^3 Y}{\partial x^3}.$$

Following the same procedure, Airy [2] showed that the behaviour of light passed through a magnetised body can be deduced from

$$\frac{\partial^2 Y}{\partial t^2} = c^2 \frac{\partial^2 Y}{\partial x^2} + b \frac{\partial Z}{\partial t}, \quad \frac{\partial^2 Z}{\partial t^2} = c^2 \frac{\partial^2 Z}{\partial x^2} - b \frac{\partial Y}{\partial t},$$

or any similar pair of equations in which the last term is an odd differential with respect to *t*, and an even differential with respect to *x*, the exact form depending on the amount of rotation of the plane of polarisation of the light, which can be determined by experiment. Airy remarks :

> I offer these equations with the same intention with which Prof. MacCullagh's equations were offered ; not as giving a mechanical explanation of the phaenomena, but as showing that the phaenomena may be explained by equations, which equations appear to be such as might possibly be deduced from some plausible mechanical assumption, although no such assumption has yet been made.[3]

4 *Maxwell and Kelvin*

Both MacCullagh and Airy seem to regard this mathematical theorising as inadequate : for them it is only the first step towards a truly mechanical theory. At that stage in the theory of light the

[1] MacCullagh, *Trans. Roy. Irish Acad.*, 1837, **17**, 461
[2] Airy, *Phil. Mag.*, 1846, **28**, 469
[3] ibid., p. 477

mere multiplication of equations was certainly inadequate, because, although they could be generalised to a limited extent to cover closely related types of observation, there was nothing like a complete mathematical theory resting on a few simple axioms from which all light phenomena could be derived. At this time, however, physicists could not conceive of such a general theory unless it were embodied in a mechanical model, and efforts were directed towards constructing ever more complicated mechanical systems which would correspond to the equations arrived at by mathematical reasoning from the observations. But it is fairly clear that these mechanical models were intended only as a guide to thought, they were known to be analogies and not literal representations of the structure of nature. Thus Maxwell's famous vortex theory in which matter (or aether—Maxwell himself was in doubt) consisted of a distribution of vortices along lines of magnetic force, connected by perfect rolling friction with 'idle wheels' whose linear motion constituted electric current, was not regarded by him as 'a mode of connexion existing in nature':

> The attempt . . . to imagine a working model of this mechanism must be taken for no more than it really is, a demonstration that mechanism may be imagined capable of producing a connexion mechanically equivalent to the actual connexion of the parts of the electromagnetic field. The problem of determining the mechanism required to establish a given species of connexion between the motions of the parts of a system always admits of an infinite number of solutions.[1]

Thomson (later Lord Kelvin) was an inveterate model-maker, whose suggestions were sometimes over-ingenious, and sometimes considerable aids to progress. For instance, he compared the aether to a homogeneous air-less foam, having *negative* compressibility (that is, increase of pressure leading to increase of volume), but this

[1] Maxwell, *Treatise on Electricity and Magnetism*, 2, Oxford, 1881, p. 427. Maxwell had an exceptionally clear idea of the function of hypothesis and model in physical theories. The first few pages of his paper 'On Faraday's Lines of Force' (*Scientific Papers*, I, p. 155) are worth studying from this point of view, but the relevant passages are too long for quotation here. H. Poincaré showed in detail in the introduction to his *Electricité et Optique* (Paris, 1890) that if functions T (the kinetic energy) and V (the potential energy) can be derived from the observed co-ordinates of a system, such that they satisfy the principles of the conservation of energy and of least action, then there is always an infinite number of mechanisms which would reproduce the phenomena as observed.

model was superfluous, for in the case of the propagation of light through pure aether (' empty ' space) Maxwell's mathematical theory provided all the model required, although Kelvin to the end of his life never reconciled himself to purely mathematical theories in physics. Again, he constructed a model of the type of aether at which MacCullagh had arrived by the mathematical reasoning described above. This aether has the property that the potential energy V depends only on the rotation of the volume elements and not on their compression and distortion. It is difficult to imagine any physical model having such a property, but here is Kelvin's model as described by Whittaker :

> 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]

Of course, neither Kelvin nor anyone else believed that such contraptions pervaded all space from the interior of molecules to the furthest stars. Kelvin's own attitude towards the significance of these models is shown by his discussion of molecular models in his *Baltimore Lectures*.[2] Here he constructed models to illustrate the interaction of aether and matter, and they were more than amusing exercises in the interpretation of equations—they were in fact valuable stepping-stones to the electron theory of matter which was developed at the end of the century, and so to the atomic models still used in modern quantum physics. It is interesting to see how, towards the end of the century, electrical models gradually took the place of mechanical ones even in Kelvin's work, a process discernible in the *Baltimore Lectures* between the time they were delivered in 1884 and finally published in amended form in 1904. The main object of these lectures was to discuss various theories of anomalous dispersion of light, involving models of the luminiferous aether. Dispersion is the change in velocity of light, different for different wave-lengths,

[1] Whittaker, *History of the Theories of Aether and Electricity*, Edinburgh, 1951, p. 145. This history does not itself attempt to analyse methodology, but is an invaluable source-book for the logician of nineteenth-century physics.

[2] W. Thomson, *Baltimore Lectures*, Cambridge, 1904

which takes place when light is passed through certain material media, and it is most reasonably explained as the result of natural frequencies of vibration of the molecules of the matter itself interacting with the vibrating aether particles, or whatever constitutes the light wave. Kelvin accordingly proposes (in 1884) what he calls a 'crude mechanical model' of molecules, consisting of concentric vibrating spheres of different densities connected by springs to each other and to the rigid lining of an ideal spherical cavity in the aether. He shows that such a model will account for the dispersion phenomena, and remarks that it could be put 'not in a rude mechanical model form, but in a form which would commend itself to our judgement as presenting the actual mode of action of the particles of gross matter whatever they may be upon the luminiferous ether'.[1] At the end of the published form of the lectures in 1904, this dynamical model is replaced by the hypothesis of electrical attraction of an atom on its 'electrion' (charged particle) when the latter is displaced from the centre of the atom, and of an attraction between the electrion and the surrounding aether proportional to their relative acceleration. Kelvin remarks:

> It is interesting to see that every one of the formulas . . . are applicable to both the old and the new subjects: and to know that the solution of the problem in terms of periods is the same in the two cases, notwithstanding the vast difference between the artificial and unreal details of the mechanism thought of and illustrated by models in 1884, and the probably real details of ether, electricity and ponderable matter, suggested in 1900–1903.[2]

5 *Summary*

The conclusions to be drawn from this discussion of the significance of models in nineteenth-century physics may be summarised as follows:

(*a*) The types of models used are very varied—there are purely mechanical structures containing a whole workshop full of balls, rods springs, and flywheels; there are continuous elastic solids; there are vortices and so on drawn from the theory of hydrodynamics; and there is the mathematical formalism of dynamics itself functioning a as model.

[1] W. Thomson, *Baltimore Lectures,* Cambridge, 1904, p. 13
[2] ibid., p. 467

(*b*) In many cases the real progress was made in terms of a mathematical model: the mechanical model was then added only as an afterthought in the mistaken belief that it endowed the mathematics with a respectability it would not otherwise possess.

(*c*) On the whole, and especially by Maxwell, models were understood to be analogues and not literal descriptions of nature.

6 *Characteristics of Models*

We are now in a position to attempt a definition of ' model ' as it is used in physics. The examples given above make it clear that such a definition must be general enough to include mathematical formalisms as well as mechanical or other physically imaginable models. Models have two sorts of characteristics which may be called respectively *formal rules* and *pointers*, and a description of these will serve as a partial definition.

Formal rules. The model has a necessary internal structure which may consist simply of the axioms and rules of inference of a mathematical formalism, as in the case of mathematical models, or which may have in addition a number of axioms suggested by the empirical laws of some physical process, as in the case of mechanical or electrical models. For example, if a simple pendulum is introduced into a mechanical model, this is equivalent to adding to the model the axiom ' the law of motion of this part of the model is $\ddot{x} + n^2x = 0$ '. The law is empirical if it refers to an actual, physical, pendulum, but the model in this case is an idealisation of the physical process, in which the empirical laws of the process are regarded, for the purposes of this particular model, as axioms of a deductive system. This is why a great deal of modern mathematical physics has the appearance of a purely deductive system, for much of it is concerned with working out the formal consequences of a model whose mathematical and physical characteristics are for the time being assumed as axioms.

Pointers. A model is not simply a system of formal rules, for it carries with it suggestions for its own extension and generalisation. If any formal system is looked at as one possible set of axioms and rules of inference, selected out of an infinite number of possible, and internally consistent, sets, then there will be certain other systems which can be reached from the first system by making simple generalisations and additions in the axioms of the first system. In the case of a mathematical model, whose formal rules may have no immediate physical interpretation, these generalisations and modifications will be suggested by the mathematics itself, just as the function V in MacCullagh's equations started as a physical potential and was generalised to mean any function mathematically admissable in the equations. In the case of a mechanical model the formal rules have been derived from the empirical behaviour of some mechanical process, and additions to these rules will be suggested by taking account of certain features of the empirical process which were previously neglected. For example, the elastic sphere model of gas molecules is derived from the rigid billiard ball model by taking account of the fact that actual bodies are not perfectly rigid, and adding the appropriate equations of elasticity to the formal rules of billiard ball motion.

The pointers are contained in a haze of mathematical and physical associations surrounding the model, some of which will be misleading and some of which will be useful for further progress. The obvious pointers to follow in any given case seem to be those which suggest new formal rules which are easily worked out in terms similar to those of the original set. For instance, it would not be helpful to extend the billiard ball model of molecules by taking account of the fact that actual billiard balls have some colour, because colour is not one of the concepts which occur in the theory of colliding bodies; in fact, the laws of motion of black billiard balls are the same as those of balls which have the same size, shape and mass, but which happen to be red. Elasticity on the other hand, is a concept which does occur in the theory of collisions and is therefore a suitable candidate for a more general theory of gas molecules. But it is not always easy to see without further experiment which characteristics of a model can be exploited in a more general theory and which are irrelevant and misleading. For example, it was not possible to see before the Michelson-Morley experiment that the properties of the medium in which the waves moved were irrelevant to the wave model of electromagnetic phenomena. Speculation upon the nature of this medium would, and did, seem a natural way of attempting to extend the wave model until further experiment showed the phenomena to be consistent with some formal aspects of the wave theory, but not with the assumption of a medium having the properties of any known material substance. There is bound to be a certain haziness about the pointers of a model; they may suggest any one of an indefinite number of modifications of the formal rules of a model, and the process of picking out relevant and useful ones is precisely the process of scientific research, for which no rules can be given.

Not only has each model an indefinite number of pointers, but since there is an indefinite number of ways of adding to the mathematical structure which forms the basis of the analogy, there is also an indefinite number of different models of any given physical situation, each having the same set of formal rules, but having different pointers, some of which may contradict the others. The particle and wave models of light have sets of formal rules which are analogous to each other and to certain elementary properties of light, but the particle model points to further properties, for example, atomicity and rectilinear propagation, which, without further elaboration of both models such as has occurred in the quantum theory, contradict properties such as interference and diffraction, which are suggested by the wave model. In practice the simplest and most familiar model will be the one that is tried first, but what is still left entirely mysterious by this account of the use of models in physics, is the fact that simple and familiar models are so successful so often. Short of some metaphysical postulate of the unity of nature there is no *a priori* reason why light should behave in the least like particles or waves, or why the fundamental particles (even the name indicates how far analogy permeates our thinking) should behave like gravitating planets or electrified pith-balls, or indeed in any way that can be described by existing mathematical theories. We ought to be prepared for, rather than surprised at, the inadequacy of familiar models in much of modern physics.

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