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Stathis Psillos

THE COGNITIVE INTERPLAY BETWEEN THEORIES AND MODELS: THE CASE OF 19th CENTURY OPTICS.*

Abstract. The aim of this paper is to revive, articulate, expand and illustrate an approach on models defended by Mary Hesse and Peter Achinstein. The prime characteristics of this approach, which I call the *analogical approach*, are three: First, it focuses on models of physical systems rather than on models of theories; second, it stresses the role of substantive analogies in model construction; and third, it allows that models may be substantive means for discovering the furniture of the world. I also present a case-study on the development of the research in optics during the previous century showing how the analogical approach can capture and explain the basic features of this case. In doing so, however, I suggest that the analogical approach needs to be augmented and improved by taking more into account the role of background theories and theoretical frameworks in suggesting, choosing and evaluating models.

I. Introduction

The role of models in scientific theorising has always been a major issue among philosophers of science. More recently, the interest in models has been boosted due to the work of Nancy Cartwright (1983) and Ronald Giere (1988).

For Cartwright models are devices that are employed whenever a mathematical theory is applied to reality. They are, generally, "specially prepared, usually fictional" descriptions of the system under study, which allow scientists to derive — within the theory — analogues of the messy and complicated phenomenological laws that are true of this system (cf. 1983, 152

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& 158). Consequently, for Cartwright, models do not link directly the theory to the real world, i.e., the world of phenomenological laws. They only link the theory with the fictitious, ship-shaped objects of the model. It is then hardly surprising, Cartwright says, that the theory is true of the model; the model is constructed so that its objects satisfy the laws of the theory. But the model — let alone the embedding theory — cannot be true of the real phenomenological laws that characterise the system. There is always a mismatch between descriptions in models and descriptions of the real phenomenological laws (op.cit., 17-18). Moreover, different and mutually incompatible models are employed, in an equally useful way, to represent phenomenological laws: some models bring out some aspects of the phenomena, whereas some different models bring out some others. Yet, for Cartwright, this patchwork of models is the only available formal representation of the real phenomenological laws.

In a somewhat similar fashion, Giere (1988) conceives of models as abstract non-linguistic entities which are defined on the basis that they satisfy a particular description, e.g., a set of equations. For instance, a paradigm of model is a simple harmonic oscillator. This is defined as the abstract entity that satisfies the force law $F = -kx$. Being abstract entities, models make no claims about the real world. Giere argues though that models are linked to the real world by means of theoretical hypotheses of the form: the real system X is, or is very close to, P, where P is the model. Theoretical hypotheses, in other words, claim a similarity (in relevant respects and degrees) between the model and the real system. On Giere's view, (known as the *semantic view* of theories), theories are entities whose gross structure consists of families of models and theoretical hypotheses. They are, then, a mixture of definitions of abstract entities (i.e., models) and empirical claims (i.e., theoretical hypotheses) that link these abstract entities with the real world, and can therefore be tested.

Despite their obvious importance, a further discussion of and adjudication between the views just sketched would fall outside the scope of the present paper. The aim of this piece is to step back in time and revive, articulate and expand an approach on models defended by Mary Hesse (1953; 1963) and Peter Achinstein (1965; 1968). The prime characteristics of this approach, which I shall call the *analogical approach*, are three: First, it focuses on models of physical systems rather than on models of theories; second, it stresses the role of substantive analogies in model construction; and third, it allows that models may be substantive means for discovering the furniture of the world.

Before I articulate this approach, I put it in a historical perspective by contrasting it with its predecessor, viz. the view that models are interpretations of theories. Having explained the advantages of the analogical approach, I then present a case-study on the development of the research in optics during the previous century and show how the analogical approach can capture and explain the basic features of this case.

In doing so, however, I suggest that the analogical approach needs to be augmented and improved by taking more into account the role of background theories and theoretical frameworks in suggesting, choosing and evaluating models. At the end of the day, scientific theorising is a complicated process that rests on the interplay between background theories, modelling assumptions and the facts to be accounted for by being incorporated into theoretical frameworks.

II. What Models Are Not

According to Rudolf Carnap (1939, 2)

> The material on which the scientist works in his theoretical activities consists of reports of observations, scientific laws and theories, and predictions; that is, formulations in language which describe certain features of facts.

This position is quite striking for it makes no reference to models as part of scientists' theoretical activity. This may sound astounding, given the current proliferation of philosophers' interest in models and their relation to theories. But the truth of the matter is that for most of the logical empiricists models were, mostly and mainly, networks of semantic rules that interpret scientific theories, the latter being primarily conceived of as uninterpreted, or partially interpreted, calculi or axiomatic systems. Being thus, the study of models was implicit in the study of the language of science and, at any rate, nothing over and above this study.[1]

In a similar vein, Richard Braithwaite called a model for a theory T

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[1] So, Ernest Nagel (1960, 90) for instance, described a scientific theory as consisting of three components: (1) "an abstract calculus that is the logical skeleton of the explanatory system, and that 'implicitly defines' the basic notions of the system"; (2) a set of correspondence rules that ground this abstract calculus to observation and experiment; and (3) "an interpretation or *model* for the abstract calculus, which supplies some flesh for the skeletal structure *in terms of more or less familiar conceptual or visualizable materials*" (emphasis added).

> another theory *M* which corresponds to the theory *T* in respect of deductive structure (1962/1970, 269).

The thrust of Braithwaite's view is that *M* is a model for *T* if and only if *M* and *T* are structurally isomorphic. Then a model is just an(other) interpretation of the theory's calculus (cf. ibid.). Moreover, the statements that constitute the model *M* of a theory *T* describe a system that is assumed to be distinct from the system of which it is a model. If a theory *T*, say the kinetic theory of gases, describes a system *X*, say the state of motion of a vast number of molecules, a model *M* of *T*, say the billiard balls model, describes billiard balls in a box and *not* molecules in a gas.

Let us call this view the *received view* on models and note that, on this view, the only requirement and constraint on model-construction is that the model must resemble the theory in formal structure. Apart from this, "it is not necessary that they should agree with the things in any other respect whatever" (Braithwaite, 1953, 91). That is to say, the only constraint for choosing, say, the billiard balls model is that its underlying calculus is just that of the kinetic theory of gases. It is also worth noting that this conception of models was taken as a unifying approach appropriate to characterise all uses of models in scientific practice, i.e., theoretical models as well as scale models and analogies. In fact, Braithwaite suggested his view as

> an attempt to make more precise the notion of a model for a scientific theory widely current in discussions of the philosophy of science (1953, 90).

The received view, however, fails to show what makes model-construction important. It fails, in other words, to give an adequate answer to the following question: why is it important to use models in scientific theorising? In order to show this, it is relevant to note that the received view evolved and revolved around the problem of the meaningfulness or otherwise of theoretical terms. Giving models for scientific theories was thought tantamount to interpreting scientific theories completely, thereby rendering their theoretical terms meaningful (cf. Braithwaite, 1962/1970, 271; Nagel, 1960, 96).[2] Moreover, if the model was cast in familiar terms, then this was taken to facilitate an understanding of the scientific theory in a way fuller than that based on the theory alone.

But if this is all that there is in the use (and value) of models, then it is too meagre even for the standards of the proponents of the received view. Why not abandon models altogether and go just for theories? After all, as both Braithwaite and Carnap observed, the partial interpretation view of theories did *not* render theoretical terms meaningless. (Here one must notice the difference between being meaningless and being indirectly interpreted.) It rather asserted that theoretical terms are "contextually meaningful", i.e., meaningful as part and parcel of the language of a scientific theory, implicitly defined within the theory and attached to experience by means of correspondence rules (cf. Braithwaite, 1962/1970, 274; Carnap, 1939, 67-68).[3]

Besides, if, as it is customary now, one understands scientific theories literally, i.e., as truth-valued interpreted systems, then no *extra* models are needed to render theoretical terms meaningful. Instead, models are called upon to concretise, specify or approximately realise assumptions about the physical system described by the theory, as for instance is the case with the Bohr model of atom and its embedding atomic theory.[4]

So if models are called upon just to provide interpretation to theories, they become rather subsidiary and eventually useless. A rational reconstruction of theoretical activity and understanding in science, of the way envisaged by logical empiricists, had little space for models. As I hinted already, this was a consequence that Carnap and Braithwaite countenanced. But Nagel wanted more of models in his picture of science, perhaps because

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[2] Braithwaite described this use of models by means of a zip-fastener metaphor. Theories are interpreted moving the zip-fastener upwards, i.e., by first interpreting the lower-level formulas to correspond to lower-level, i.e., observational, hypotheses and then interpreting higher-level formulas, as far as possible, in the lihgt of these observational hypotheses. Models are interpreted moving the zip-fastener downwards, i.e., by first interpreting the higher-level formulas to correspond to higher-level, i.e., theoretical, hypotheses and then moving downwards to lower-level formulas (cf. 1953, 90). So the main advantage of using models was taken to be that in them the epistemological order is reversed.

[3] Carnap went as far to protest against the use of models that intend to make the axioms of a theory "intuitive". He stressed that "It is important to realise that the discovery of a model has no more than an aesthetic or didactic or at best a heuristic value, but it is not at all essential for a successful application of a physical theory" (op.cit., 68). Nor, he thought, is this important for an understanding of physical theories: "Thus we understand 'E' [the electric field vector], if 'understanding' of an expression, a sentence, or a theory means capability of its use for the description of known facts or the prediction of new facts" (op. cit., 68-69).

[4] This is a point that Achinstein (1968,234) made quite clearly when he protested that the Bohr model is not deployed to give meanings to terms like "elliptical orbit", "mass", or "charge", which appear in the central postulates of the atomic theory, but rather to give a set of assumptions about their connections in the atomic stucture described broadly by the atomic theory.

he was more sensitive to the actual use of various substantive models in theory-construction and expansion in science. Hence Nagel (1960, 106-114) dealt quite extensively with how models can be heuristically valuable in constructing theories on the basis of formal or substantive analogies (as in the case of the development of the Schröndiger-equation); in expanding theories in new domains of enquiry (as in the case of the van der Waals equation); and in providing inclusive systems of explanation (as in the case of mechanical models of electromagnetic and optical phenomena).

Yet he, together with Braithwaite, was very keen to stress that the formulation of a theory by means of models is *not* free from dangers. The prime danger was taken to be that the model may be confused with the theory itself, resulting in an identification of the domain of the model with that of the theory (cf. Nagel, 1960, 115-116; Braithwaite, 1953, 93- 94). However, what they suggested was not just an extra caution in the use of models. They did not just warn against some vulgar identification of model with theory. They rather adopted a more radical view which Braithwaite summed up as follows:

> Thinking of scientific theories by means of models is always *as-if* thinking; hydrogen atoms behave as if they were solar systems each with an electronic planet revolving round a proton sun. But hydrogen atoms are not solar systems; it is only useful to think of them as if they were such systems if one remembers all the time that they are not. The price of the employment of models is eternal vigilance (1953, 93).

Of course, this as-if attitude towards models fitted perfectly with the rest of the received view. For models were just thought of as alternative interpretations of a theory's calculus and hence they had nothing to do with the domain of the theory. They were at most instrumental in interpreting or illustrating the theory in more familiar terms; but they were *not* supposed to make factual claims about the domain of the theory.

This as-if attitude, however, poses a second serious question to the received view. (The first one, as we saw earlier, was why use models at all.) This is what Marshall Spector (1965, 125) has called "the question of reality": Can models ever be a tool of discovering the furniture of the world? The received view, trapped in a conception of models that rests on formal analogies, implied a necessarily negative answer to this question. If thinking of models is always an as-if thinking, then there is no possibility that the model can have factual reference in the domain of the theory; the objects of the model can never be identified with the entities that the theory posits.

Apart from being too strong a philosophical position, this view is at odds with scientific practice itself. That is, not only is there no reason to exclude, on *a priori* grounds, the possibility of models revealing the furniture of the world, but there are also cases where the issue of the reality of a model has been raised seriously and helped to fill out the original interpretation of the theory (cf. Spector, 1965/1970, 283).

So, the received view on models failed to adequately answer two important questions: first, why use models in science?; and second, can models give access to the world? In fact, the received view rendered the second question totally illegitimate by making model-construction part of the as-if thinking (cf. Spector, 1965, 125).

In what follows I shall first sketch a more adequate conception of models and their role in scientific theorising based on the views of Achinstein (1963; 1968) and Marry Hesse (1953; 1963). Then, I shall focus on the cognitive interplay between theories and models and highlight my views with the development of nineteenth century optics. Finally, I shall address the issue as to whether models can give access to reality.

III. The Analogical Approach and Beyond

While the received view focused on models of theories, its critics (cf. Achinstein, 1965; 1968, Hesse, 1953; 1963 and Spector, 1965) paid attention to models of physical systems. So, the prime problem-situation in model construction was taken to be the following. Scientists want to investigate a set of phenomena or, more generally, find out about the behaviour of a target physical system X. To this end, they construct a *theoretical model* of X. That is, they employ a set of assumptions (normally of a complex mathematical structure) — let us call them modelling assumptions — which provide a starting point for the investigation of the behaviour of X (cf. Achinstein, 1968, 212). So, the well-worn billiard balls model of gases is a set of assumptions about the motion and collisions of an aggregate of gas molecules (target system X). It ascribes to this system a behaviour which can be approximated and simulated by that of a collection of (perfectly elastic) billiard balls.

Generally, the modelling assumptions involve several idealisations, simplifications and approximations. So for instance, the billiard balls model of gases assumes that gas molecules can be regarded as perfectly elastic spheres obeying Newton's laws. This is clearly an idealisation. Other modelling assumptions involve approximations, such as the duration of collisions between molecules is negligible, or simplifications, such as ignoring the effect of intermolecular forces.

An important question that arises here is this. Is this sort of model-construction arbitrary? In other words, what guides the choice of modelling assumptions for X? This choice is by no means arbitrary. It is guided by *substantive similarities* between the target system X and some other physical system Y. It is in the light of these similarities that Y is chosen to give rise to a model M of X; that is to be the source of a set of assumptions on the basis of which the behaviour of X is to be investigated. So, for instance, the billiard balls model of gases is chosen on the basis that the behaviour of gas molecules contained in a box (target system X) is, to a certain extent and in certain respects, similar to the behaviour of a collection of billiards balls (source system Y).

Let me call this approach to model-construction *the analogical approach* and attempt to capture the dependence of model-construction on substantive similarities between two physical systems X and Y by adopting the locution *model M of X based on Y*. But in order to ward off a possible source of confusion, it is important to distinguish clearly a model M of a system X based on Y from the system Y itself which, being to some extent similar to X, is the source of assumptions for the construction of this model. As I said before, a theoretical model (a model$_1$, in Hesse's terminology) of X is a set of assumptions about X. The system Y (a model$_2$, in Hesse's, I think unfortunate, terminology) is employed, in a way that will become clear in a moment, to give rise to this set of assumptions. Yet, Y is *not*, strictly speaking, a model of X. It is a distinct physical system which is similar to X in some respects. So for instance, the billiard balls model of gases is based on some substantive similarities between the behaviour of a collection of (perfectly elastic) billiard balls (system Y) and that of aggregates of molecules (system X). But whereas system Y is a system of billiard balls, the model of gases based on it is a set of assumptions about gases and *not* about billiard balls.

Following Hesse (1963, 8-9), I suggest that the relation between the source system Y and the target system X is characterised by the existence of (a) some positive analogies, i.e., properties, or relations between properties that both Y and X share in common; (b) negative analogies, i.e., properties, or relations between properties, with respect to which X is unlike Y; and (c) some neutral analogies, i.e., some properties about which we do not yet know whether they are positive analogies, and which may turn out either positive analogies or negative ones. It is these positive and neutral analogies between Y and X that can give rise to a model of X based on Y. These analogies suggest that Y can play a *heuristic role* in unveiling some of the properties of a physical system X. For instance, by trying to explore the space of neutral analogies (i.e., by trying to find out whether or not X possesses more of the properties of Y) we end up with a better knowledge of what X *is* and what it *is not*. It should be then clear that although the existence of negative analogies between Y and X prohibits the identification of Y and X, it does not block the heuristic role of Y. While distinct from X, Y can offer a set of modelling assumptions for X; that is, Y can give rise to a model M of X (cf. figure 1).

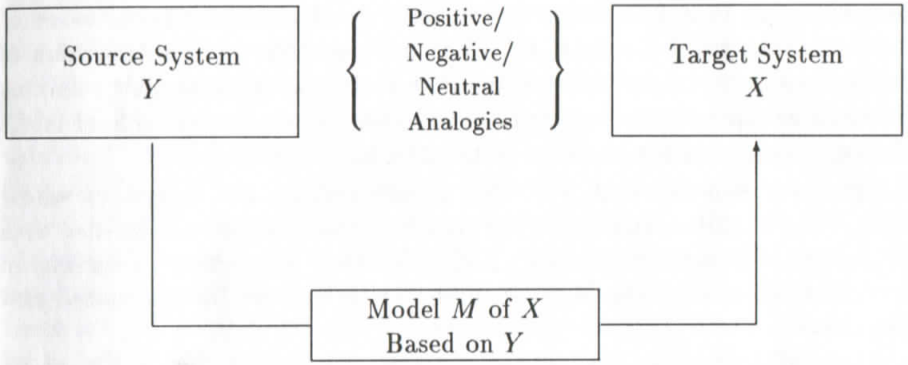

Figure 1

According to the analogical approach, models are indispensable means of scientific theorising, their heuristic value being based on substantive similarities and analogies between different physical systems. As Hesse (1963, 68) stressed, these substantive similarities (or analogies) are of two sorts: formal and material.

A formal analogy between two systems X and Y relates to the mathematical structures that represent the behaviour of X and Y. In many cases the construction of a model M of X based on Y is tantamount to applying Y's mathematical description to X. That is to say, a model M of X based on Y is a set of assumptions that model the mathematical description of X upon that of Y. One must note here that in such a case one need not assume any sameness in properties between X and Y. All that is required is that some elements of X stand in the same relation to one another as the corresponding elements of Y. Such a model M of X based on Y can be useful for understanding X. It may, for instance, suggest embedding the description of X in a broader mathematical structure. Or it may suggest further connections between the elements of X in view of connections that already hold between the corresponding elements in the mathematical description of Y.[5]

Material analogies, what Hesse (1963, 68-69) called pretheoretic material analogies between observables, relate to sameness or similarity at the level of properties. A set of material analogies between two physical systems Y and X alludes to the possibility that one of the systems, say X, can be described, in certain ways and to a certain extent, from the point of view of Y. In particular, it suggests that Y and X may be similar in more respects (covered in the space of neutral analogies) and can therefore furnish a basis for supposing that further similarities can de discovered. Then, a set of material analogies between Y and X is indispensable for the derivation of predictions from a model M of X based on Y, in that it can suggest relations between new properties over and above those already known to hold in X. Hence, it can furnish a basis for testing the model M of X.

We can then say that if Y is a known system, i.e., a system whose behaviour is known, and if Y presents certain substantive similarities with X in terms of properties, relationships between properties, or chunks of structure, then a model M of X based on Y can be further tested and evaluated.

At this point it may be useful to inquire what is the relation between a model of a system X and a theory of X. Along with Hesse (1953, 203) and Achinstein (1968, 215; 217), I take it that a theory is a set of literally understood statements which purports to describe correctly, i.e., truly, the behaviour of a particular physical system X. When a theory of X says that X is governed by such and such principles and laws, one must take it literally and state that, according to the theory, X is so-and-so. Moreover, when one suggests a construction as a theory of X, one, I think, suggests that it is plausible to believe that X is the way the theory says it is.

With models, however, things are not so clear-cut. When a theoretical model M is employed to provide a set of assumptions about the physical system X, one does *not* start off with the belief that M provides a literal description of X (cf. Hesse, 1953, 201-202; Achinstein, 1968, 215). As I described it before, a theoretical model M of X based on Y is a heuristic tool for the study of X. It is a set of assumptions about X, where these assumptions have been borrowed from another system Y on the basis of some substantive similarities between Y and X. However, it is not, and should not be taken as, representing X fully and in all its aspects. In fact, a theoretical model is open to all sorts of modifications, in the light of new experience, in an attempt to capture more accurately the phenomena under investigation. So one can employ a model even though one believes it to be only an approximation, or even a simplified, inaccurate and, at any rate, literally false representation of X (cf. Achinstein, 1968, 217). To explore an expression of Giere's (1988, 81) a model M of X based on Y represents X only in certain respects — specified by the positive analogies between X and Y — and to certain degrees — specified by the conditions of approximation and the idealisations employed in the model.

Does this distinction between theories and models throw us back to the claim of the received view that thinking in terms of models is always an as-if thinking? As I said in the previous section this is too strong a claim. In fact, as Hesse (1963, 14) has rightly argued, if a model M of a system X is heuristically successful it should be able to suggest a theory of X, that is a literally understood description of X. My own view is that models do not start off as literal descriptions; but, as I shall try to show in section V, there are, at least in principle, circumstances in which they may well end up as believed theoretical descriptions/explanations of the behaviour of the system they model.

One can rightly object here that theories, like models, may also be seen as approximate, simplified and restricted descriptions/explanations of the phenomena. I think there is some leeway here. The point worth stressing is that a difference between a model of X and a theory of X is one of degree; in fact a difference of degree of belief.[6] A model of X comprises a set of assumptions that are not yet believed to describe X, or even, that are ever going to be believed as giving a full description. On the contrary, a theory of X is the end-product of scientific theorising. When something is advocated as a theory of X, the degree of belief in that it correctly describes/explains X is, generally, high.[7]

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[5] These formal aspects of model-construction have been studied in more detail by Michael Redhead (1980). He has suggested that such mathematical models are the source of the very important process of cross-fertilisation in theoretical physics (cf. op. cit., 149)

[6] As Andrew Powell pointed out to me the difference between a model and a theory may be seen as an intentional one, i.e., a difference relating to our having different epistemic attitudes towards them

[7] These issues are obviously relevant to the debates over scientific realism. For the de-

As my subsequent study of the nineteenth century optics will, hopefully, make vivid, the analogical approach to models and their role in scientific theorising captures central facets of model-construction in science.[8] Unlike the received view, it takes models seriously and explains their heuristic role by reference to substantive similarities between different physical systems. It also allows that models may be substantive means for the discovery of the furniture of the world.

However, an important issue that the critics of the received view did not deal with extensively is this: What aids and guides scientists to spot substantive similarities/analogies between different physical systems? Perceived analogies in observable properties is, surely, one source. But I think that the complete answer here should include more. It is background theories — which exemplify substantial beliefs about the physical world — that guide scientists in adopting specific models, i.e., specific modelling assumptions, in their attempt to understand the behaviour of certain physical systems. The choice of a model M of X based on Y is guided and constrained by background theories as well as by the pretheoretic analogies between Y and X.

In fact, I am going to show that models are presented *within* general theoretical frameworks.[9] The theoretical framework guides the construction of models, the latter being attempts to specify and render concrete the principles of the framework for their successful application to the physical world and the conquest of new physical territories.[10] This function of model-construction is paramount especially when scientists want to specify the description of a physical system so that it falls under the scope of a given theoretical framework. It is also indispensable in investigating the inner structure, composition or mechanism of a physical system whose general dynamical behaviour can be studied by already existing abstract theories. For, models also stand for possible candidates for the constitution and internal structure of the target physical system. Making use of substantive positive analogies between some known physical systems and the target physical system, models are deployed in a heuristic way to investigate whether and to what extent these known physical systems — that fall within the scope of a given theoretical framework — can represent the constitution and internal connections of the otherwise unknown target physical system. They are, therefore, employed to unravel what a more detailed description of the target system could or could not be like. We can generally say that in the foregoing cases, theoretical models are used to enrich the given theoretical framework (cf. Redhead, 1980, 147).

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fence of scientific realism requires that theories, although perhaps strictly speaking false, are nontheless approximately true descriptions/explanations of their domain. I have developed these issues in my (1994a)

[8] I do *not*, however, think it exhausts the role of models in scientific theorising. Cartwright, for instance, has recently pointed out that models are means to concretise the abstract physical concepts (cf. 1993, 268-270). So, for example, the abstract concept of force acquires concrete forms by means of models such as the pendulum, the gyroscope, or two-body system. These models delineate the use of abstract concepts and show the conditions for their application to concrete problem-situations.

[9] This is a view that Achinstein (1968, 215) also expressed, but did not develop.

[10] For a similar, but undeveloped, view cf. Ian Hacking, (1983, 217).

Let me now attempt to substantiate and illustrate these views by looking into the development of the 19th century optics.

IV. *Towards a Dynamical Theory of Light Propagation*

It is well-known that the general dynamical behaviour of a conservative physical system which moves from one configuration to another can be adequately described by Lagrangian Dynamics. The Lagrangian L of a system can be described as the difference $T - W$, where T is the kinetic energy and W the potential energy of system. L can also be expressed as a function of the generalised co-ordinates q of the system and their velocities u (where $u = dq/dt$). Then, the equations of motion of the conservative dynamical system under consideration are

$$\frac{d}{dt}\left(\frac{dL}{du_a}\right) - \frac{dL}{dq_a} = 0. \tag{1}$$

Since $L = T - W$, it follows from (1) that

$$\frac{d}{dt}\left(\frac{dT}{du_a}\right) - \frac{dT}{dq_a} + \frac{dW}{dq_a} = 0.^{11} \tag{2}$$

If we are able to determine

(a) the kinetic energy as a function of some set of parameters q of a system and their time derivatives dq/dt and

(b) the potential energy as a function of the set of parameters q,

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[11] For more details cf. G. Fowles (1986, 276 – 277).

we can then determine the general dynamical behaviour of the system under consideration. The application of this method does *not* depend on the details of the constitution and the internal connections of the system, but rather on its kinetic and potential energies. So, the importance of the Lagrangian method is that it can be used to describe the behaviour and the laws of motion of any dynamical system whose specific dynamical connections are not known.

One of the prime targets of theoretical research in optics during the previous century was the formulation of a dynamical theory of the propagation of light, which aimed to yield all laws of the behaviour of light from general dynamical principles concerning the carrier of light-waves, known as the ether. This research programme was developed by Augustin Louis Cauchy, George Green, James McCullagh and George Gabriel Stokes. In the framework of the new electromagnetic conception of light, it was pursued further by James Clerk Maxwell and his followers.

Although, thanks to the pioneering research of Augustin Fresnel, the ether was known to be a conservative system that sustains transversal waves, it is important to stress that the physical constitution and internal connections of the carrier of light-waves were unknown. In view of these facts, the theoretical research in optics developed on the basis of an interplay between general dynamical theories and models of the constitution of the ether.[12]

The theoretical framework that scientists adopted was Lagrangian Dynamics. They considered the carrier of the light waves (target system X) as a dynamical system whose general behaviour can be studied by Lagrangian dynamics and aimed to derive, within this framework, the most general laws of the propagation of light. This was taken to be enough for the development of a dynamical basis of light propagation. The use of Lagrange's method enabled the scientific community to investigate the general dynamical properties and functions of the carrier of the light waves, "leav(ing) out of account altogether the details of the mechanism, whatever it is, that is in operation in the phenomena under discussion" (Larmor, 1893, 399). As we saw before however, the subsumption of light propagation under Lagrangian dynamics required the specification of the kinetic energy–function and the potential energy one. But while the form of the dependence of the kinetic energy on the velocity of the moving bodies is in all cases the same and can be known, the form of the dependence of the potential energy on the position of bodies cannot be generally stated; it depends on the special nature and characteristics of the system under consideration. Hence, the prime task of theorists was to specify a potential energy–function that could adequately describe the behaviour of the ether. To this end they had to employ several modelling assumptions about the nature and characteristics of the ether.

Here is exactly the point where particular theoretical models of the ether proved to be very useful. As we shall see in more detail in the subsequent sections, these models were proposed *within* the framework of Lagrangian dynamics and attempted to specify the energy–function of the ether. Having formulated a potential energy–function for the ether, the next task was to correlate it with some of the known properties of light, i.e., amplitude, intensity and others. Then, the resulting theory was put to the test by examining whether it yields the known laws of light–propagation.

For the purpose of offering a dynamical basis for light propagation, no further specification of the nature of the carrier of light waves was needed. In fact, as we shall see in section IV.2, McCullagh discarded all modelling assumptions as soon as he reached his potential energy–function. For the specification of the potential energy–function, along with the known kinetic energy one, were enough to subsume light propagation under the domain of dynamics and then examine whether the resulting laws of motion could yield the known laws of light propagation. Then, although based on models, the advancement of dynamical theories of light propagation did not require scientists to *believe* that the ether is constituted in the way implied by the model in use.[13]

However, the models employed for the specification of an energy–function did also stand for possible candidates for the constitution of the carrier of light-waves. For instance, the model that Green and Stokes employed rested on the assumption that the energy–function of the otherwise unknown ether (target system X) could be associated with that of an ordinary elastic solid (source system Y). Then a model based on the dynamics of an elastic solid (henceforth, an elastic solid model) was used in a heuristic way to investigate whether the constitution and internal connections of the ether

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[12] For more on this one can see my (1992; 1994).

[13] In view of this situation Joseph Larmor stressed "The division of the problem of the determination of the constitution of a partly concealed dynamical system, such as the aether, into two independent parts. The first part is the determination of some form of energy–function which will explain the recognised dynamical properties of the system, and which may further tested by its application to the discovery of new properties. The second part is the building up in actuality or in imagination of some mechanical system which will serve as a model or illustration of a medium possessing such an energy function" (1894, 417).

could be mapped upon those of an elastic solid. Such a procedure was heuristically valuable for the discovery of what ether (i.e., the target system X) could be and what ether is not.

The heuristic value of an elastic solid model — as opposed to, for instance, models based on the dynamics of liquids — was based on certain *positive analogies* between an elastic solid (source system Y) and the otherwise unknown carrier of light-waves (target system X). In particular, after Fresnel, scientists settled for the view that light- waves are uniquely transversal (cf. Psillos, 1994; forthcoming). This fundamental discovery suggested that the carrier of light waves, irrespective of its actual constitution, had to exhibit specific properties in virtue of which it could sustain transversal waves.[14] A model of such an otherwise unknown carrier of light-waves had to be based on the propagation of a disturbance through an elastic solid. For the latter exhibits properties, such as transversality, which are analogous to the known properties of light propagation. Then, in view of this positive analogy most scientists started attacking the problem of the dynamical foundations of light-propagation "through the analogy with the propagation of elastic waves in solid bodies" (Larmor, 1893, 392); that is, using the features of the propagation of a disturbance in an elastic solids as a set of assumptions about the behaviour of the carrier of the light-waves.

Despite its usefulness, the elastic solid model of the constitution of the ether was not taken as revealing the real constitution of the ether. Here part of the problem lies with the fact that an elastic solid can also transmit longitudinal waves. In fact — and this was the touchstone for the elastic solid model — it followed from the laws of mechanics that when a transversal wave strikes the interface of two media it gives rise to a transversal *and* a longitudinal component. But, since light-waves were known to be purely transversal, the emergence of the longitudinal component presented an important negative analogy between the elastic solid model and the propagation of light. As we shall see later, the successful neutralisation of the longitudinal component turned out to be the most important problem that the elastic solid model faced as a plausible candidate for the constitution of the carrier of light-waves. But, as the next section will show, even when the model was modified so that the longitudinal component is neutralised, the modified model could not yield the known laws of light propagation.

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[14] In mechanical terms, it had to exhibit certain rigidity so as to allow the propagation of light with certain finite velocity and also certain elasticity — or capability of deformation — so as to allow transversal propagation.

The upshot of this section, however, is not to affirm the failure of the elastic solid model to represent the workings of the carrier of the light waves. Rather it is that the construction and choice of models was guided by *both* background theory and material analogies. They were based on the background knowledge that the carrier of the light waves, whatever its nature be, possesses certain properties, viz. capacity to sustain transversal waves, finite velocity of propagation and conservation of energy. Then, Lagrangian dynamics provided the general framework for the theoretical description of light propagation. It was within this framework that specific models were deployed. In fact, these models were concretisations and enrichments of this framework, in that they specified the form of the potential energy–function of a concealed dynamical system such as the ether. But these models of the carrier of light waves would have been useless unless they were based on systems, e.g., elastic solids, that share some positive analogies with the properties of the ether.

Having thus outlined the general framework of the relations between theories and models in the nineteenth century optics, let us see three more concrete cases of theoretical research.

IV.1 Green: Modelling the Unknown Ether

The scientist associated most with the elaboration of the elastic solid-like model was Green.[15] He, however, suggested the difference between the investigation of the general dynamical behaviour of light in terms of Lagrangian dynamics and the particular models which may be called forth in order to help uncovering the constitution of the ether. He pointed out:

> 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 physical principle as the basis of our reasoning, rather than to assume certain modes of action, which after all, may be widely different from the mechanism employed by nature (1838, 245).

Based on the positive analogy between the propagation of elastic disturbances in a solid and the propagation of light, Green set out to investigate

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[15] For a more elaborate account of Green's theory, cf. R.T. Glazebrook (1885, especially 159-163) and Kenneth Schaffner (1972, 46-58).

the former in order to find *the extent* to which it can give rise to an adequate dynamical model for the latter.

Green's objective was the specification of the potential energy–function ϕ of the propagation of disturbances in elastic solids (cf. 1838, 245). To this end, he applied the Lagrangian method to the dynamical system underlying the propagation of elastic waves in solids and determined the most general equation of wave motion in solids. He found that in the case of a solid whose density ρ is taken as unity this equation has the general form

$$\int\int\int\{\frac{d^2u}{dt^2}\delta u + \frac{d^2v}{dt^2}\delta v + \frac{d^2w}{dt^2}\delta w\}dxdydz = \int\int\int \delta\phi dxdydz \qquad (3)$$

where u, v, w, are the Cartesian components of the displacement vector $\vec{r}$; $\delta u, \delta v$, and δw are small displacements of these components; and $\delta\phi$ is the exact differential of the characteristic energy–function of the system (being a function of the positions only).

Having chosen an ordinary elastic solid as his source system, Green correctly assumed that the value of ϕ for a volume element $\delta\tau(= dxdydz)$ is a function of its *deformation*, i.e., change of form (shape/volume) of $\delta\tau$. He then specified the equation of motion in the case where the disturbance strikes the interface of two media, which, for the Cartesian component u of the displacement vector $\vec{r}$, had the well known form

$$\frac{d^2u}{dt^2} = B\nabla^2 u + (A - B)grad_x divu \qquad (4)$$

where A and B are constants. Green also arrived at the boundary conditions which must be satisfied in the interface of the two media (cf. 1838, 255-6).

However, the foregoing wave equation (4) gives two solutions, one corresponding to a transversal wave propagated with velocity $\sqrt{B}$ and another corresponding to a longitudinal wave propagated with velocity $\sqrt{A-B}$. In view of the fact that light-waves are purely transversal, Green realised that if this equation were to describe the propagation of light, he had to specify the coefficients A and B so that the part responsible for the *generation* of the longitudinal component of the wave-motion becomes ineffective. Background knowledge suggested that the ratio A/B had to be greater than $4/3$ if the medium were to be stable. This meant that A must be different from B. Then, the only way to fix the coefficients A and B so that the longitudinal component turned out ineffective was that A/B be a very large quantity. In particular, Green suggested that A tends to infinity, and B is much smaller than A. Hence, $A - B \approx A$. Then, since $\sqrt{A-B} = \sqrt{A}$, it followed that longitudinal waves were transmitted with infinite velocity $\sqrt{A}$ and they were undetectable (cf. 1938, 246).

Green had therefore shown a kind of modification that can be carried out in an elastic solid (source system) in order to neutralise *but not eliminate* the longitudinal wave in the resulting elastic solid model of the ether (target system). Yet, apart from this *ad hoc* way of fiddling with the coefficients, no adequate explanation of the neutralisation of longitudinal waves was offered. As Stokes (1862, 176) stressed in his report on the dynamical theories of optics:

> Although the theory [i.e., Green's] is perfectly rigorous, (...) the equations [determining the constants A, B] are of the nature of forced relations between the constants, not expressing anything which could have been foreseen, or even conveying when pointed out the expression of any simple physical relation.

Nevertheless, the real problem that the elastic solid model faced was its inability to yield the known laws of the propagation of light; in particular, Fresnel's laws of reflection.[16] Hence, it was unable to provide a set of assumptions constituting a dynamical basis for these laws (cf. Whittaker, 1951, 142; Doran, 1975, 156). This fact meant that whatever the character of the carrier of the light-waves was, it could not be an elastic solid of an ordinary sort (cf. Glazebrook, 1885, 169; Larmor, 1893, 395). The set of assumptions that this model employed, in particular its energy–function, could not be a set of assumptions for the constitution of the physical system underlying the propagation of light, for they clashed with experimentally established laws. Green's model, however, was heuristically valuable in suggesting what the ether is not.

IV. 2 McCullagh's Rotational Ether

McCullagh (1839), independently of Green, suggested that the Lagrangian method can be used for the description of the dynamical behaviour of light.

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[16] It is worth noting that this failure related to the negative analogy between the propagation of disturbances in elastic solids and light waves. In Green's model, in the case where the incident light is polarised at right angles to the plane of incidence, it is impossibile to satisfy all the boundary conditions without assuming that the reflection of light generates longitudinal waves (cf. Whittaker, 1951, 140). So one cannot simply suppress the negative analogy in order to create a "suitable" model.

He also developed the characteristic equation of motion for the propagatio of light, which in vectorial notation had the form

$$\int \frac{d^2\vec{R}}{dt^2}\delta\vec{R}d\tau = \int \delta V d\tau \qquad (5)$$

where $d\tau$ is a volume element $dxdydz$, and V is such that its integral ove a volume element is the potential energy of the system, and the density ρ is taken as unity.

McCullagh's aim was the specification of the function V for the system underlying the propagation of light. However, the set of modelling assumptions he used was different from Green's. He first defined an abstract vectorial quantity $\vec{L}(= X, Y, Z)$ such that

$$\vec{L} = curl\vec{R} \qquad (6)$$

where $\vec{R}$ is the well-known displacement vector (i.e., Green's $\vec{r}$). Then he focused on the propagation of light in crystalline media and assumed that $\vec{L}$ is a function of (i) the *angle of rotation* of a volume element $d\tau$ of the carrier of light-waves with respect to a co-ordinate system set along the principal axes, or axes of elasticity, of the crystal and (ii) the *angle of deformation* of a volume element $d\tau$. So, he determined the characteristic energy–function V (as a function of $\vec{L}$):

$$V = -\frac{1}{2}(a^2X^2 + b^2Y^2 + c^2Z^2). \qquad (7)$$

McCullagh then stated:

> 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. Supposing, therefore, in the first place, that a plane wave passes through a crystal, we shall seek the laws of its motions from equations (5) and (7), which contain everything that is necessary for the solution of the problem (1839, 156).

In other words, McCullagh made the important observation that once the energy–function was determined he could dispense with the actual details of the constitution of the system underlying the propagation of light and attempt to derive the laws of behaviour of light by using the most general principles he had just demonstrated.

He indeed succeeded in deriving the laws of reflection and refraction, thereby offering the first dynamical account of these laws. The general feature of his theory, however, was that the medium whose dynamical behaviour he described (i.e., the target system) could *not* be modelled by using an ordinary elastic solid as the source system (cf. also Whittaker, 1951, 142-143; Harman, 1982, 26). For, the vector $\vec{L}$ representing the light–disturbance could not possibly be modelled as the displacement in a medium which transmits vibrations by elasticity in the manner of an ordinary elastic solid. As we saw in the previous section, the potential energy–function (Green's function ϕ) that characterises the vibrations in an ordinary elastic solid depends on the *deformation in shape and size* of a volume element $d\tau$ of the medium. However, McCullagh's potential energy–function V was dependent on the *rotation* of a volume element $d\tau$ of the medium; i.e., it was an energy–function uncharacteristic of ordinary elastic solids. Consequently, McCullagh's dynamical account of the propagation of light could not be modelled by the set of assumptions pertaining to an elastic solid. The elasticity involved in McCullagh's account was purely rotational; it could not possibly be the elasticity of an ordinary elastic solid.

Although McCullagh's theory, (based on principles (5) and (7)), yielded the correct laws of optics, he was unable to provide a known physical system which could illustrate the rotational medium he was committed to. As Larmor observed, this led to the neglect of the theory of rotational ether (cf. 1894, 415).[17]

McCullagh's case is quite instructive. On the one hand, he was willing to totally jettison any modelling assumptions concerning the realisation of his energy–function and build his theory based exclusively on the arrived at energy–function. On the other hand, his theory of *rotational ether* fell into neglect because he did not offer a physically realisable model of it. In fact, it is arguable (cf. Psillos, 1994) that before Maxwell's mature theory of the electromagnetic field, the provision of an actual physical situation which exemplified the properties of the carrier of light waves was taken as *sine qua non* for the adequacy of any account of the propagation of light.[18]

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[17] McCullagh's theory was recovered later by G.F. FitzGerald (1878), (1880) who realised that the energy–function V was analytically identical with the one advanced by Maxwell. As soon as this was observed, McCullagh's theory fell in as a chapter of Maxwell's theory, facilitating the latter in the derivation of the correct laws of optics within the new electromagnetic theory of light. In fact, the physical system that could model McCullagh's ether was no other than Maxwell's electromagnetic field (cf. FitzGerald, 1878; Stein, 1982, 315).

[18] For a detailed account of Maxwell's use of models cf. Margaret Morrison (forthcoming).

IV. 3 Stokes and the Elastic Jelly

This last remark can be reinforced by looking into Stokes's attempts to make a physically realisable case for an ordinary elastic model of the ether. Stokes worked within the elastic solid model but he was aware of the fact that an important *neutral analogy* between the elastic solid model and the ether could, apparently, be best accounted for within the otherwise inadmissible fluid models. This neutral analogy related to the motion of solid bodies through the ether: if the all–pervading ether was modelled on the basis of an elastic solid then it would be difficult to accommodate the translatory motion of planets within it. After all, how can a solid body — such as a planet — penetrate without resistance another solid?

In a series of papers on the possible constitution of the ether Stokes tried to address this issue on behalf of a physically realisable elastic solid model. The physical way to state the problem was this: was the ether like an ordinary fluid or did it possess some properties not existing in ordinary fluids? (cf. 1848, 8) The mathematical way to state the problem was this: what sort of known mathematical model is to be used in the discovery of the properties of the ether? If the ether was treated as a fluid then the mathematical model had to be such that internal pressures of the medium are normal to the common surface of two portions whose mutual action is considered. If the ether was treated as an elastic solid, the internal pressures would be in general oblique, and hence they would always have a component tangential to the interface of two portions (cf. 1849, 281).

Stokes observed that in view of the well-established fact that light-waves are uniquely transversal, *"so far as the motions which constitute light are concerned"* he had to opt for assumptions based on the propagation of elastic waves in solids (ibid.). This meant he was "absolutely obliged" to suppose the existence of a tangential force during the propagation of light waves. Yet, he observed, this obligation did not entail

> that the ether is to be regarded as an elastic solid when large displacements are considered, such as we may conceive produced by the earth and planets, and solid bodies in general, moving through it (ibid.).

But how could there be a medium which possesses properties known to be there in an elastic solid and others which are incompatible with an elastic solid? It is at this point that the usefulness of *physically realisable* models becomes clear. For, if there is a realisable physical system such that it possesses these seemingly contradictory features, then Stokes could argue that there is nothing physically inadmissible in having a carrier of the light–waves which shares some properties of an elastic solid and yet also exhibits fluid-like properties.

The physical system which could model the seemingly contradictory properties that the carrier of light-waves should have was an *elastic jelly*. Yet Stokes was quick to warn his readers that

> the following illustration is advanced, not so much as explaining the real nature of the ether, as for the sake of offering a plausible mode of conceiving how the apparently opposite properties of solidity and fluidity which we must attribute to the ether may be reconciled (1848, 12).

So, in effect, Stokes warned his readers not to take his model as explanatory but rather as illustrative of the physical admissibility of such a medium. His construction was like this: Take a piece of elastic jelly. This jelly is an elastic solid, in that it possesses rigidity and elasticity. Dissolve the jelly in a little water and then keep watering it down. In the course of this process, the jelly becomes thinner and thinner and eventually it will be fluid. "Yet", Stokes pointed out, "there seems hardly sufficient reason for supposing that at a certain stage of the dilution the tangential force whereby it resists constraint [i.e., the characteristic of its solidity] ceases all of a sudden" (ibid.). So, the diluted jelly would be solid enough to resist deformation and fluid enough to permit the motion of solid bodies through it. That is to say, given this model,

> we may conceive the ether to be, a fluid as regards the motion of the earth and planets through it, an elastic solid as regards the small vibrations that constitute light (1848, 13).

In view of this physically realisable situation, Stokes had showed how a neutral analogy could turn into a positive one. But it would be contrary to what he stated to claim that he took this model literally as actually *identical* to the ether. Instead, he called for a "suspension of judgement" as the real constitution of the carrier of light-waves, since no adequate evidence was yet available (1848, 12).

Stokes case, however, shows that models played also the role of visualising physical situations which could exhibit the known properties of the carrier of light-waves. This role was mainly psychological, yet it was taken very seriously in assessing the merits of each model — at least before Maxwell's mature work on the electromagnetic field (cf. Psillos, 1994).

In 1862, 13 years after his first papers on the dynamical behaviour of light, Stokes referred to the ether as a "mysterious" entity, "of the very existence of which we have no direct evidence" (1862, 172), thereby emphasising the heuristically valuable role of models in the investigation of its workings. He stressed that all theorists in optics, including himself, had mathematically treated the ether "as a single vibrating medium" (op.cit., 180). He, therefore, emphasised the difference between a general dynamical theory of this single vibrating medium and the particular models which may be used to unravel its structure by means of positive analogies. For, the foregoing assumption about the ether, formulated in terms of Lagrangian dynamics, was enough for an investigation of the general dynamical properties of the carrier of light-waves, independently of any particular model of its constitution.

V. Concluding Remarks

I think the foregoing study of the development of the nineteenth century optics is quite characteristic of the research patterns in theoretical physics. Generally, the end of scientific theorising is the production of theories that describe and explain some phenomena under investigation. But, in view of what my study showed, scientific theorising is a complicated process that rests on the interplay between, on the one hand, background theoretical frameworks and theoretical principles and, on the other hand, models that attempt to concretise and enrich these principles in view of incorporating the phenomena under investigation into the framework.

As the research strategy of Green, McCullagh and Stokes suggested, theoretical physicists start with a network of general physical and mathematical principles — expressing their current background beliefs about the physical world — in an attempt to describe the most general behaviour of the target physical system X. These principles, however, need concretisation which is effected by the choice of specific modelling assumptions. Different scientists may employ different modelling assumptions. Green and McCullagh, for instance, modelled the potential energy–function of the carrier of light waves by employing different assumptions about its internal structure. But, as the analogical approach to model-construction has suggested, the choice of modelling assumptions is not arbitrary. It is based on substantive similarities between the behaviour of the target system X and other physical

systems that exemplify this general behaviour. So, the choice of a family of elastic solid models, instead of a family of liquid models, was based on the presence of substantive similarities between the propagation of elastic waves in solids (source system) and the propagation of light through the ether (target system).

The resulting models of the target system X are then tested against the phenomena to be explained. Failure to square with known laws of the phenomena, as for instance was the case with ordinary elastic solid models of the ether, renders them inadequate accounts of the behaviour of the target system. But, even then, models have played a rather significant heuristic role. They have shown what the behaviour of the target system X is *not* like, and have therefore opened up the path for trying alternative accounts. When, on the other hand, a model succeeds in yielding the known laws of the phenomena under investigation — as, for instance, in the McCullagh case — it promotes an understanding of the behaviour of X, even though this understanding may stretch only up to a certain extent and a certain degree.

But I must stress here that, as all the protagonists of my case study stressed, the use of models does not mean or entail that a particular model should be, or indeed is, taken as *identical* to (i.e., as representing fully in all respects) the physical system it purports to cast some light on. In fact, the scientists whose research we considered employed models heuristically and yet they did not commit themselves to the truth of their modelling assumptions — especially in the light of failures to account for the phenomena. In general, I think one of the messages of my case-study is that models have a different *epistemic function* from theories. Theories express the scientists' beliefs about the physical reality which they set out to penetrate but models, at least initially, do not.

Having said all this, one may ask the following question: Do the views explored in my study entail that one can *never* be a realist about a theoretical model? — where by realism about a theoretical model I mean the attitude in which one believes that a model M correctly represents a physical system X.

In order to avoid a possible misunderstanding, I must stress the following. The upshot of my case-study is that for the scientists involved in this research programme there were no good empirical and theoretical grounds for taking a realist stance towards the particular models of the ether. Nowhere did they say that one should always think of models in an as-if fashion. Nor did they suggest that there could be no circumstances under which they would understand models as adequate representations. Their view was that in the face of particular *empirical and theoretical reasons* such as the presence of persistent negative analogies (Green), or the *ad hoc* nature of the suggested modifications (Stokes), or the lack of independent support to the modelling assumptions (McCullagh) they could not adopt a realist stance towards their models.

Hence, I think one *can*, in principle, take a realist stance towards particular models. For, although scientists do not start off with the assumption that a particular model gives a literal description of a physical system X, there may be circumstances in which a model M of X should end up being understood literally and being believed as an adequate representation of X. These circumstances relate to the accuracy with which a given model represents the underlying structure that the model stands as a candidate for. If there are reasons to believe that this representation is accurate, one can adopt a realist attitude toward a model. So, for instance, when testing a model M, it may happen that the neutral analogies between the target physical system X and the source system Y on the basis of which the model M of X is constructed turn out to be positive. The finding of more positive analogies, matched with a persistent lack of negative analogies, may be a good starting point for checking the possibility that M correctly represents the nature of X. Amassing more evidence, such that novel correct predictions for X derived from M, may be enough to convince the scientific community that M represents X correctly.[19]

But, one may say, even if the representation is only approximate, there may be circumstances in which one can as well adopt the view that the model gives an *approximately correct* characterisation of the target system. I think a warranted high degree of confidence in the adequacy of the representation that a scientist expresses depends on how good the *evidence* she possesses is. This evidence may be such that it warrants a high degree of belief that the physical system X does not substantially differ from the description given in the model M. For instance, it may warrant that although the model only approximates the behaviour of X, it describes some of the essential features of the behaviour of X. Then, if the warranted degree of confidence in the adequacy of the representation is high, one can take a realist stance towards a model. Eventually, I think there is, in principle a point — *being a function of the available evidence* — where a model of X can give rise to a theory of X.

The point, then, I want to stress is this. It is a matter of specific empirical investigation as to what warrants a realist or a merely heuristic understanding of a particular model. Someone cannot start, prior to good empirical reasons, committing herself to believing in the identity between models and reality. Taking a realist attitude towards a particular model is a matter of having evidence warranting the belief that a specific model gives us an accurate representation of an otherwise unknown physical system in all or most of its respects.

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[19] Needless to say that there is always space for a comparative evaluation of models in terms of how adequately they represent the target system.

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Department of Philosophy
King's College London, U.K.