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Models versus Mathematics?

Duhem's book *The Aim and Structure of Physical Theory* (published in the very first years of this century) remains a surprisingly fertile source of problems for philosophy of science. For example, the " Duhem Problem ", the fact that no single theory is testable " in isolation " from a whole group of auxiliaries, is still a central concern for those involved with the relationship of theory and evidence. Again, Duhem developed an " anti-realist " view of scientific theories which is a focus of renewed attention and to which recent anti-realists have, in my opinion, added little if anything (1). And finally, as the announcement of the theme of this conference pointed out, it was Duhem who introduced into 20th century philosophy of science discussion of the role of *models* in science, and of the comparative importance of model and mathematical considerations in the development of physics.

I must say, however, that I regard Duhem's discussion of models as the least successful part of his book: several claims which ought to have been kept distinct are in fact conflated, and theses which start out clear but clearly wrong are later so qualified as to become not clearly wrong, by virtue of not being clear. It is little wonder then that Duhem's position on the comparative role of models and of mathematics has so often been misunderstood. This misunderstanding has wrought confusion throughout the subsequent debates — at any rate in the methodological and philosophical literature. I thought that the most useful service I could perform at this conference, therefore, was to go back to the beginning, back to Duhem's treatment and try to clarify it by extracting and sharpening the different claims hidden within it. The conclusion that I come to is perhaps somewhat disappointing: that there is rather *less* to this models/mathematics debate than meets the eye.

1. Scientific revolutions and Duhem's alleged instrumentalism

We should begin by understanding what seems to me to be the single most important factor in Duhem's whole intellectual position. Like his contemporary, Poincaré, he saw clearly that if we interpret scientific theories realistically or,

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(1) See *e.g.* B. G. van Fraassen, *The Scientific Image*, Clarendon Press, 1980, and my review of it: J. Worrall, " An Unreal Image ", *Brit. J. Phil. Sci.*, *35*, 1984, 65-80.

as he would have put it, if we interpret them in terms of a metaphysical system, then we must admit that there have been the most radical revolutions in science.

Taking optics as an example, very early science (perhaps pre-science) saw light as some sort of immaterial *effluvium*, early modern science saw it as minute material particles fired machine-gun fashion from luminous sources, early and mid-19th century science saw light as a disturbance in an all-pervading elastic medium, then this was replaced by the idea of light as a changing electromagnetic field, and finally (although this was just after Duhem) by the idea of photons obeying an entirely new quantum mechanics. Science has apparently changed its collective mind about the underlying nature of light quite radically and quite often. But beneath these radical changes at the top, there is steady *empirical* progress: while earlier theories had managed to accommodate the simple laws of reflection and refraction, later theories accommodated these *plus* interference and diffraction effects, then polarisation and double refraction effects, the interrelationship between light and electricity and magnetism, the photoelectric effect and so on.

Recent philosophers have shown that Duhem's claim of strict continuity and accumulation is strictly speaking false even at the empirical level. Fresnel's wave theory for example by no means straightforwardly included the old, simple law of reflection. Indeed that theory entails that strictly speaking the old law is always false. However, Fresnel's wave theory also entails that the difference between the real state of affairs and the prediction of the old law is below the level of observability, except in certain exceptional cases involving very narrow reflecting surfaces. The fact that this is typical — that where a new theory contradicts an old empirical law it " corrects " it rather than replacing it altogether — surely means that Duhem's claim is *essentially* correct. There is essential accumulation at the empirical level, despite radical revolutions at the fully fledged high theoretical level.

Indeed Duhem noticed that this continuity (or rather, essential continuity) standardly extends to the level of the *mathematical equations* entailed by a theory. These too generally manage to " live on " through revolutions. For example, Fresnel's equations for the intensities of reflected and refracted light in various circumstances were carried over completely intact into Maxwell's electromagnetic theory. Of course, in the process the *meaning* of these equations — the interpretation of the theoretical terms involved — changed radically. In Fresnel the optical disturbance represented the distance an element in the elastic solid aether had been moved from its equilibrium position; in Maxwell the " disturbance " was simply the electromagnetic field strength at that point. (Of course, Maxwell tried very hard to produce an interpretation of the electromagnetic field in terms of a mechanical substrate. But the fact is that he failed: later science got used to the electromagnetic field as a separate, mechanically uninterpretable entity.) In other words the mathematical *syntax* lives on despite the change in the semantic interpretation of the theorecical terms involved. Again, Duhem rather overstated the case — generally the continuity at the mathematical level between successive theories is not strict. In this sense, the Maxwell-Fresnel case is very much the exception rather than the rule. The rule is again " essential continuity " — standardly the equations " live on " not in their original generality but rather as " limiting cases ". (The classic example being, of course, Newton's laws of motion as limiting cases of the corresponding relativistic equations.)

Despite this need to qualify it somewhat, Duhem's position is surely essentially correct, that despite radical discontinuities at the high theoretical or metaphysical level in science, there is essential continuity at the empirical level and even at the level of mathematical equations uninterpreted " from above ". (Of course they are always interpreted " from below " in the sense that the theory ties them eventually, via so-called bridging principles, to empirical laws.) Duhem expressed this idea by saying that while the explanatory or metaphysical part of a theory may be jettisoned altogether as science progresses, the *representative* part is always captured by later theories. Duhem developed a famous metaphor to describe the progress of science, that of a mounting tide:

> Whoever casts a brief glance at the waves striking a beach does not see the tide mount; he sees a wave rise, run, uncurl itself and cover a narrow strip of sand, then withdraw leaving dry the terrain which it had seemed to conquer [...] But under this superficial to-and-fro motion, another movement is produced deeper, slower, imperceptible to the casual observer; it is a progressive movement continuing steadily in the same direction and by virtue of which the sea constantly rises (2).

The transitory, ephemeral, " flashy " but insubstantial waves are of course the explanatory parts of theories; the substantial, less easily discerned and steadily growing tide is constituted by the " representative " parts of theories. As Duhem expressed it, without metaphor:

> When the progress of experimental physics goes counter to a theory and compels it to be modified or transformed, the purely representative part enters nearly whole in the new theory, bringing to it the inheritance of all the valuable possessions of the old theory, whereas the explanatory part falls out in order to give way to another [entirely different] explanation (3).

These passages make clear the conclusion which Duhem draws from this analysis. That science should eschew " explanation " altogether — that the interpretation of the theoretical terms involved in the equations of mathematical physics is not a matter which need, or should trouble the physicist. The *real* scientific theory is only the " representative " part — the mathematical equations, uninterpreted " from above ".

Duhem was not, however, the " instrumentalist " which this claim might make him seem and which he has indeed been so often interpreted as. He emphasised that physical theory had proved able not only to accommodate known

(2) P. Duhem, *The Aim and Structure of Physical Theory*, 38.
(3) *Ibidem*, 32.

empirical laws but also successfully to predict entirely new and hitherto unexpected phenomena. And Duhem acknowledged that no one could witness the proven prophetic abilities of a scientific theory without acknowledging that the theory somehow *reflected* reality. It would be a miracle if a scientific theory could make predictions of this kind which were successful if it did *not* reflect reality. " Reflect " but *not* " accurately describe ". Duhem was not here going back on his anti-realist views. His claim was that a theory which had been predictively successful must be, or be part of, or at any rate approximate, what he called a " natural classification ". Although his account of " natural classification " is undoubtedly murky and obscure, and although I do not propose to try to produce a clearer account here, one can, I think, see what Duhem is getting at. Fresnel's elastic solid aether theory of light, once considered as a fully-fledged description of reality, is now considered to be entirely incorrect. Nonetheless we do not believe that its success both in accommodating already known optical results and especially in predicting hitherto entirely unknown phenomena, like conical refraction, was merely accidental. Fresnel's theory had somehow latched on to some aspects at least of the *underlying structure* of light — even though we now accept that it is not an accurate description of the *nature* of light. Indeed, according to Duhem science should *never* have regarded Fresnel's theory in this way.

2. The heuristic power of explanatory principles

An immediate problem which Duhem's view of the nature of physical theory faced was this. Have not the beliefs underlying the explanatory parts of scientific theory, for all that they may have been jettisoned later, nonetheless played important roles in the *construction* of that theory? Have not " explanatory " " metaphysical " views, in other words, played important *heuristic* roles in arriving at the theories which embody them? Of course *after the event* the interpretative, explanatory part can be sliced off a theory without that theory's losing anything of its empirical content — but the theory might not have been arrived at in the first place had it not been for its inventors' belief in the " explanatory " part.

Duhem faced up squarely to this problem. He was forced to admit, rather reluctantly, that certain explanatory principles had indeed played on occasion an important heuristic role:

> Does this mean that no discovery has ever been suggested to any physicist by this [realist] method? Such an assertion would be a ridiculous exaggeration. Discovery is not subject to any fixed rule. There is no idea so foolish that it may not some day be able to give birth to a new and happy idea (4).

However he also insisted that instead of the " explanatory " ideas playing the leading heuristic role, by far the more usual pattern was that the representative

(4) *Ibidem*, 95.

part of science developed under its own steam and an "explanation" subsequently, and in Duhem's view gratuitously, pasted on top:

> The descriptive part has developed on its own by the proper and autonomous methods of theoretical physics; the explanatory part has come to this fully formed organism and attached itself like a parasite (5).

The standard pattern, then, according to Duhem was for the heuristic push to be provided by the representative part of science itself — without any reference to "explanatory" principles. How exactly could this occur? One suggestion was that a certain lack of formal, *syntactical* symmetry might be spotted in a mathematical equation and then a new term introduced in order to restore symmetry. This would lead to a new representative theory. But Duhem believed that by far the most important heuristic aid in science was *formal* analogy:

> The physicist who seeks to unite and classify in an abstract theory the laws of a certain category of phenomena lets himself be guided often by the analogy that he sees between these phenomena and those of another category. If the latter were already ordered and organised in a satisfactory theory the physicist will try to group the former in a system of the same type and form.
> The history of physics shows us that the search for analogies between two distinct categories of phenomena has perhaps been the surest and most fruitful method of all the procedures put in play in the construction of physical theories (6).

Duhem goes out of his way to differentiate analogies from models. Analogies are purely formal affairs — similarities of structure rather than of nature. Models involve, as we shall see, interpretation in more fundamental metaphysical terms. This is one of the points at which there has, I think, been a good deal of confusion. The fact that the wave theory of light could be — and to a certain limited extent *was* — developed by analogy with the case of sound has often been used by the advocates of models as a particularly clear case in which Duhem's views came to grief. Nothing could be further from the truth: the light/sound analogy is grist to Duhem's mill and indeed he explicitly cited it as an example bearing out his case:

> Thus, it is the analogy seen between the phenomena produced by light and those constituting sound which furnished the notion of a light wave from which Huygens drew such a wonderful result (7).

It is grist to Duhem's mill because the analogy is, according to him, formal — light and sound are entirely different *kinds* of phenomena, but the idea that they may, *considered abstractly*, share many properties has borne much scientific fruit.

So we need to sound two cautionary notes concerning Duhem's views. First that there is, for him, all the difference in the world between a model and

(5) *Ibidem*, 32.
(6) *Ibidem*, 96.
(7) *Ibidem*, 96.

an analogy. Second, that as we saw earlier, the whole heuristic debate is never going to be a clear cut one: although Duhem's position on the import of scientific theory gave him a vested interest in playing down the heuristic role of metaphysics, he certainly was never going to make the claim that metaphysics played *no* role. Indeed he explicitly branded that claim "an absurd exaggeration". All he was willing to argue was the comparative and less clear-cut claim: that the heuristic role of metaphysics had generally been exaggerated and that of mathematical analogy generally underestimated.

Duhem made exactly the same comparative claim about the heuristic role of models: their role too while certainly not non-existent was considerably smaller than was generally believed. What exactly did Duhem mean by a "model", or more specifically a "mechanical model"? The answer is I think that he was not altogether clear himself. He lumped together under this heading all the sins that he saw inherent in the "English school" of mathematical physics — whose leading representative was William Thomson, Lord Kelvin. I think it is useful to separate out these alleged sins rather carefully.

3. "Mechanical models": Explanation, unity and heuristics

Some people find it helpful in trying to master, say, Fresnel's theory of light, to *visualise* the ether particles successively affected by the disturbance constituting light as attached to spiral springs in the x, y and z directions — springs of equal strengths in the case of free ether and of the ether within isotropic (unirefringent) media, and springs of different strengths in the case of of the ether within birefringent media. Other people find that mechanical models of this kind simply clutter up the scene and feel much happier operating with the abstract mathematical representation of the process. Similarly some find it helpful in trying to understand elementary chemistry, to think of the valency of an atom in terms of the atom's possessing a certain number of coupling hooks. Others do not require this imaginative assistance.

Kelvin was one who found this assistance indispensable:

> I never satisfy myself until I can make a mechanical model of a thing. If I can make a mechanical model, I understand it. As long as I cannot make a mechanical model all the way through I cannot understand [...] (8).

Duhem associated this predilection for mechanical models with a particular cast of mind — so called "broad but weak" minds, which stress the visual imagination above abstract reason. He regarded "broad but weak" minds as typically English — although his paradigm example was the mind of Napoleon Bonaparte. I don't know if Kelvin ever read Duhem (he died I think in 1907) but if he did then as a Scotsman born in Ireland he must have found it particularly galling to be given an *English* mind. At any rate,

(8) W. Thomson, *Lectures on Molecular Dynamics and the Wave Theory of Light*, Baltimore 1884, 270.

Duhem contrasted broad but weak English minds with so-called " strong but narrow " minds which did not need visual, imaginative assistance and operated happily at the abstract logical level and which he regarded as typically French — although one of his paradigm examples was the mind of Isaac Newton.

Duhem clearly regards the " French mind " as superior. But for all his complaining about the English mind, he cannot finally develop a real thesis at this level. Kelvin's statement just quoted is purely a statement about his own psychology — that *he* needs to construct a mechanical model of a process in order to feel that he has understood it. We must presume that it is an accurate statement about his own psychology. Duhem may say that he himself has a different psychology. But neither the Duhemian who finds models of this kind entirely unhelpful *nor* (more significantly) the Kelvinist who finds them essential, actually believes that the springs or the coupling hooks actually exist. This whole question therefore is, *in itself*, of no relevance to the question of how we are to understand scientific theories. It is no relevance to the *logic of science*, but only to the *psychology of scientists*.

Kelvin does, however, go on to " objectify " and " generalise " his claim into one which *does* concern the logic of science and with which Duhem also disagreed. Kelvin held that science itself cannot claim to have explained a phenomenon unless it has produced a mechanical theory or mechanical reduction of it:

It seems to me that the test of " do we or do we not understand a particular subject in physics? " is " Can we make a mechanical model of it? " (9).

Not now " I ", notice, but " we " — science in general. Although Kelvin expressed himself in terms of models here, this stemmed, as we'll see, from certain problems. What Kelvin would certainly have liked was a fully fledged mechanical *theory* of matter and the field. Duhem believed that, even if it were feasible, this would not be a *necessary* aim for science.

The two particular examples which Kelvin, writing in the 1880's, had in mind were electromagnetism and heat theory. Kelvin — like Maxwell himself of course — did not believe that science could rest satisfied with a theory in which the electromagnetic field was treated as *primitive*. The electromagnetic field at any point in space had to be further explained in terms of the state at that point of some mechanical substrate. And Kelvin strove mightily to specify such a mechanical substrate. As for heat theory, science again could not rest content with so-called phenomenological thermodynamics in which heat was treated as a primitive — science cannot claim to have *explained* anything if it sticks to this level. Instead explanation requires a mechanical account — this time, of course, in terms of molecular dynamics:

In the [theory of heat], which is based upon the conclusion from experiment that *heat is a form of energy*, many formulae are at present obscure and uninterpretable, because we do not know the mechanism of the motions or distur-

(9) *Ibidem*, 71.

bances of the particles of bodies ... [B]efore this obscurity can be perfectly cleared up, we must know something of the ultimate, or *molecular*, constitution of the bodies [...] (10).

Duhem held that, on the contrary, so long as a theory is both unified and empirically successful, it is totally irrelevant to physics whether or not its primitive terms are interpretable in some allegedly deeper metaphysical framework, like that of mechanism. As it happens, Duhem had various objections to Maxwell's theory, but these definitely did *not* include the lack of a mechanical interpretation of the electric and magnetic field strengths; and, as for heat theory, phenomenological thermodynamics was almost Duhem's *ideal* physical theory — his celebrated aversion to the statistical-kinetic theory was life-long.

It might seem that, in view of the subsequent history of physics, the score here was " Duhem 1: Kelvin 1 ". Science has given up the idea of a mechanistic reduction of the electromagnetic field, but on the other hand the atomic statistical-kinetic theory became a brilliantly successful scientific theory.

But the real issues are deeper than this. First, let's look at the claim about *explanation*: no scientific explanation without mechanistic reduction. A somewhat more abstract version of this same claim was argued later by the English scientist and methodologist N. R. Campbell. Campbell's target was clearly Duhem although he does not mention him by name. Campbell's claim was that for a scientific theory to explain a phenomenon it must do more than logically entail a correct description of that phenomenon, the theory must also exhibit an analogy between that phenomenon and another *more familiar* phenomenon — one whose laws were, in other words, already known. As Campbell said in his book *Physics – the Elements* (subsequently reprinted under the title *What is Science?*):

The explanation offered by a theory [...] is *always* based on an analogy, and the system with which an analogy is traced, is always one of which the laws are known [...] (11).

There are some difficulties with the idea of analogy but clearly identity is one form — in fact the strongest form — of analogy. So Kelvin's particular demand for actual mechanistic *reductions* of electromagnetism and heat theory would certainly satisfy this more general demand of Campbell's.

Clearly however both the specific and the more general demand are incorrect. Neither a mechanistic reduction nor even any explanation in more familiar terms is *necessary* for scientific explanation. In fact Campbell later in his book and having raised the cases of relativity theory and quantum mechanics, candidly admits that the central methodological thesis of the early part of his book is quite wrong:

In recent developments of physics, theories have been developed which conform to the [deducibility condition]. [But in] place of the analogy with fa-

(10) *Ibidem*, 72.
(11) P. Duhem, *The Aim ...*, *quot.*, 96.

miliar laws, there appears the new principle of mathematical simplicity. These theories explain the laws, as do the older theories, by replacing less acceptable by more acceptable ideas; but the greater acceptability of these ideas introduced by the theories is not derived from an analogy with familiar laws, but simply from the strong appeal they make to the mathematician's sense of form (12).

I never understood why, in view of this admission, Campbell did not withdraw, or at any rate totally rethink, his whole book.

The mistake which Kelvin and Campbell both made is worth rooting out — for it is one that is often still made today.

Philosophers of science have nowadays generally accepted the fact that science cannot adequately be analysed in terms of single, specific, theories. At any one stage in the development of science there will be a hierarchy of accepted statements at different levels of generality. For example in the 1850's it was firmly accepted that light is a wave-like disturbance transmitted through an all-pervading elastic medium. Certain specific properties of the medium — especially as it existed within transparent bodies — were open to conjecture. But allowable conjectures were constrained by the well-entrenched general wave theory — constrained in the very strong sense that allowable conjectures had to be specific versions of the general theory. In Lakatosian terms, there existed in the mid-1800's a wave optics *research programme*. Underlying this programme — part of what Lakatos called the " positive heuristic " — were certain still more general and more deeply entrenched assumptions — of determinism, of mechanism, of continuities and symmetries of various sorts and various conservation principles. This hierarchy of entrenched assumptions supplies a natural pecking order: an indication of which assumption is likely to be modified first in the light of empirical difficulties. Generally speaking, the more specific the theory the more likely it is to be modified first. In this sense science does seem to be a conservative enterprise: when empirical difficulties arose scientists first tried to solve them by making specific adjustments to the particular properties of the aether without for a minute questioning the more general assumption that it existed. Only after a series of failures to do this had occurred did the more general assumption come to be questioned. Even then the still more general " metaphysical " principles which cut across specific research programmes, were still adhered to. Until, that is, further failures finally caused some of *them* to be brought into question.

Principles like those of mechanism and determinism were, however, presupposed by science for centuries. While they *were* presupposed it was natural to regard them as having a dual role — they operate *both* as substantive claims about the world *and* as heuristic principles, requiring that any acceptable theory carry them as an implication. Some of them were presupposed for so long that it became natural to regard them not just as heuristic principles within one research programme, or a succession of research programmes, but to regard them

(12) *Ibidem*, 153.

as *general methodological criteria* — part of the very characterisation of success in science. Every real scientific explanation — every successful scientific theory — had to be mechanistic. This is the mistake which Kelvin made. It is a mistake which Poincaré soon after warned against: to look *always* for a mechanical account "would be to forget the end we seek which is not mechanism, the true and only aim is unity" (13).

On this point Duhem was surely absolutely right. No matter how used science becomes to providing successful theories which have certain characteristics — of being deterministic or mechanistic or whatever — it should never forget that the basic characterisation of success is purely in abstract terms: a theory must be unified and empirically successful. Science for a long period produced theories which were unified and empirically successful and which were at the same time mechanistic. But this did not mean that mechanism was to be written into the basic methodological requirements for scientific success. Should someday a theory be produced which was unified and more empirically successful than any available mechanistic theory but was not itself mechanistic, then science would have no option but to accept it. On this point the subsequent history of science, of course, bore Duhem out entirely.

As for Campbell's more general claim that science only explains if it reduces a phenomenon to something more familiar, this too is wrong. What Campbell regarded as the "new" requirement of simplicity (really unity and empirical success) was not new at all. It is what had basically been operating all along. The "analogy with already known laws" was merely an epiphenomenon. The general mechanistic research programme was successful for *so long* that science got *so* used to explaining phenomena in its terms that it became natural, though mistaken, to regard conformity with it as an aoutright requirement for an explanation. But relativity theory and quantum theory did not rewrite the very requirements of a scientific explanation — instead they produced better scientific explanations that any previous ones *on the very same standard of explanation*.

Here, then, Duhem was definitely right and Kelvin (and subsequently Campbell) definitely wrong. Kelvin nailed his colours so firmly to the mast of mechanism that he made the assumption of mechanism an essential part of scientific success. Duhem rightly held that scientific success was a much more abstract notion — the real criteria are only unity and empirical success. Science had for a long time satisfied those criteria through theories that were also mechanistic but if a theory satisfied these criteria *without* being mechanistic then all well and good: mechanistic "reductions" are not *necessary* for science.

On the other hand, it must surely be granted to Kelvin that the search for mechanical theories had as a matter of fact proved a very successful way of doing science. Mechanism had exhibited great heuristic power. And, so far as one of Kelvin's main concerns — namely heat theory — went, was to go on proving

(13) H. Poincaré, *Science and Hypothesis*, Dover, New York, 177.

successful. Let us, however, postpone further consideration of this point until after we have considered Duhem's specific complaints against models, as such.

One of the reasons that I have identified behind Duhem's rather confused attack on "English physics" is, then, the claim that attempted mechanical reductions, fully fledged mechanical theories are not necessary for scientific explanation and scientific progress. However while such a full mechanical reduction of *all* the properties of matter is what Kelvin was aiming at, what he actually achieved was only a series of partial, incomplete mechanical theories, or *models*. Duhem had even more objections to these.

The principal objection was that by allowing *different* models for different phenomena — but different phenomena *within the same field* — Kelvin and company had destroyed the *unity* of physical theory. "It is" said Duhem, "the English physicist's pleasure to construct one model to represent one group of laws, and another quite different model to represent another group of laws, notwithstanding the fact that certain laws might be common to the two groups". For the "French physicist" on the other hand unity of theory was the primary requirement. Here is the sort of thing to which Duhem objected in Kelvin's approach:

Is the problem to represent the coefficients of elasticity in a crystal? The material molecule is represented by eight spherical masses occupying the vertices of a parallelepipedon, and these masses are connected to one another by a greater or lesser number of spiral springs.

Is it the theory of dispersion of light which is to be made clear to the imagination? Then the material molecule is found to be composed of a certain number of rigid, concentric, spherical shells held in that position by springs. A multitude of these little mechanisms is embedded in the aether. The latter is a homogeneous incompressible body, inelastic for very rapid vibrations, perfectly soft for actions of a certain duration. It resembles a jelly or glycerine.

Is a model suitable to represent rotational polarisation desired? Then the material molecules that we scatter by thousands in our "jelly" will no longer be built on the plan we have just described; they will be constructed of little rigid shells in each of which a gyrostat will rotate rapidly around an axis fixed to the shell.

But that is too crude a performance for our "crude gyrostatic molecule", so that a more perfect mechanism is soon installed to replace it. The rigid shell no longer contains merely one gyrostat, but two of them turning in opposite directions; ball and socket joints and sheaths to connect them to each other and to the sides of the spherical shell, allowing a certain play to their axes of rotation (14).

Two of the complaints underlying Duhem's remarks have already been dealt with. First, he dislikes what he sometimes called the "industrial" analogues: springs, ball and socket joints, sheaths and so on. But as I already argued, since no one is suggesting that these do any more than *illustrate* the theory,

(14) P. Duhem, *The Aim...*, *quot.*, 82.

no real matter of principle is at issue here, but only one of style. Secondly, Duhem is against the whole idea that a reduction to mechanics is scientifically essential. A point that we have again already discussed. But now there is a new, third element — for which Duhem reserved his strongest complaints.

Although Kelvin was undoubtedly aiming at a full mechanical reduction of matter and the field what he actually achieved was a series of *partial theories* or models — different but overlapping. This means that Duhem's cherished principle of *unity* is endangered or rather seemingly ignored. For Duhem a physical theory was above all an abstract economical and *unified* classification of phenomena. The use of models in Kelvin's method militates against unity and hence is to be deplored.

The fact that the disunity they produced was Duhem's *chief* objection to models is underlined by his otherwise puzzling remarks about *algebraic* models. Duhem, for example, stated:

> Maxwell's *Treatise on Electricity and Magnetism* was in vain attired in mathematical form. It is no more of a logical system than [Kelvin's] *Lectures on Molecular Dynamics*. Like these *Lectures*, it consists of a succession of models, each representing a group of laws without concern for the other models representing other laws [...]; except that these models instead of being constructed out of gyrostats, spiral springs and glycerine are an apparatus of algebraic signs (15).

So, even though his chapter title contrasts " Abstract Theories and Mechanical Models ", the visualisable and even the mechanical aspects can be quite taken away and yet leave Duhem still objecting to the disunity that models introduce.

This aspect of Duhem's criticism of Kelvin brings us closer to the contemporary debate about models. Duhem is arguing that there is no scientific merit in Kelvin's procedure of constructing a series of overlapping, partial theories — given, of course, that if they are different and overlapping then they conflict. Was Duhem right?

First we should separate two different senses of models. In *one* case we may have a fully-fledged theory which is unified and considered to be accurate but which is mathematically intractable. Scientists may then " use a model " in the sense that they introduce assumptions which are " known " to be false, because they contradict the theory. But these assumptions make the situation tractable from the mathematical point of view. To take the obvious example: the Newtonian n-body problem has, of course, no closed solutions and hence, although the theory says that the orbit of Mars, for instance, is affected by all the bodies in the solar system — indeed strictly by all the bodies in the universe — no prediction of the orbit of Mars can be strictly deduced from this theory. A simplified model — in the *most* simplified case, a model which pretends that *only* the Sun and Mars exist — may, however, be mathematically soluble and may yield consequences which are approximately correct. The model assumptions —

(15) *Ibidem*, 86.

which, notice, are actually *known* to be false — " fill the computation gap ". The legitimacy of this method surely depends on whether or not the fully-fledged theory itself gives us reason to believe that the effects which the model ignores are relatively small (16).

It is a second sense of model which Duhem criticises. Consider a case in which, instead of a fully-fledged scientific theory like Newton's theory, we have only a general theoretical framework and no very clear idea of how to go about adding to that framework the extra assumptions necessary to produce a specific scientific theory. This was Kelvin's position. He had a general framework supplied by his mechanistic outlook, but various difficulties stood in the way of producing specific, though still universal, theories of the field and of matter within that framework. The specific assumptions needed to produce a full theory would be referred to as *models* in two different sets of circumstances:

a) In the first, while the general framework is firmly entrenched, the specific assumptions, initially at any rate, are highly conjectural. In this case no reasons why we should regard the specific assumptions as actually false may be known. Hence such a model *may* subsequently be elevated to the rank of theory.

b) In the second type of case, the specific assumptions *are* " known " to be false. This second case may itself arise in two different ways. Because no general theory can be constructed, a series of partial theories, *known* to be over-simplified — that is, strictly false, may be developed, each of which deals reasonably satisfactorily with some but not all phenomena. Or, some theory may be initially introduced as a universal conjecture but then turn out to have only partial success — some successful predictions but equally some failures. The same fate befalls subsequent attempts, and the outcome is a series of specific theories which are successful only in part: they each successfully deal with some phenomena, but not with others. Each set of specific assumptions will then be downgraded, definitely confirmed in its status of *model*, or perhaps " *mere* model ".

It was in situation *b*) that Kelvin, of course, found himself. Let's remind ourselves of a familiar example of such a situation. A dynamical theory of gases was sought to explain thermal and thermochemical phenomena. The idea that gases consist of molecules in motion provides a general framework for theories but hardly in itself constitutes a specific theory. For this we need specific assumptions, for example about the structure of the molecules. Maxwell suggested a model — the so-called billiard ball model (though it would undoubtedly have been given a more prestigious name had it proved fully successful). This model did turn out to have one major success: it predicts the subsequently

(16) For a much more systematic treatment of the different senses of the term "models" as used in physics, see M. L. G. Redhead, "Models in Physics", *Brit. J. Phil. Sci.*, *31*, 1980, 145-163. My treatment is indebted to Redhead's.

verified, but at the time startling, fact that the viscosity of a gas is independent of its density. However it also has many faillures: for example, it predicts wrongly that viscosity varies with the square root of the temperature. Other, more complicated models had other successes and other failures. We are left in a confused situation. The general framework can deal with a whole range of phenomena but only through a sort of " bag of tricks " — a series of different partial theories or models which if proposed as general theories would unambiguously contradict one another.

Duhem in several passages tried to land Kelvin with the claim that this situation in which models have proliferated is entirely satisfactory. Indeed such proliferation is supposed positively to appeal to the " English mind " — lending science the extra charm of variety. If this were true, then of course Duhem would have every right to criticise the modellers on the grounds that they surrender entirely the ideal of a *unified* physical theory. But of course it is *not* true: Duhem here definitely cheated.

It is quite clear from reading Kelvin that he regarded the diversity of his models of matter and field as an entirely unwelcome feature which had been forced on him by the complexity of the phenomena. The models he proposed were, as he himself frequently said, " not to be accepted as true in nature ". This was partly because these models involved the unrealistic " industrial " elements mentioned earlier, but also, and more importantly, because even once these purely illustrative analogical parts had been removed, the models remained partial — and indeed mutually contradictory if proposed as *general* theories. They were the best Kelvin could do in the short term, the *long term* aim was undoubtedly to produce a *general* theory which superseded all the models. Indeed in the end Duhem himself admitted that Kelvin was working in

the hope that these ingeniously imagined models may indicate the road which will lead in the remote future to a physical explanation of the material world (17).

And Duhem cites — without demurring from it — an important passage from Poincaré about contradictions, or rather about theories which *would* contradict one another were they not restricted to disjoint domains by artificial barriers. Said Poincaré:

> We should not flatter ourselves on avoiding all contradiction [...]. Two contradictory theories may, in fact, provided that we do not mix them and do not seek the bottom of things, both be useful instruments of research. Perhaps the reading of Maxwell would be less suggestive if he had not opened so many new, divergent paths [...] (18).

These concessions by Duhem seem to me to take all the heat out of the debate. He concedes that no one is arguing that the unity of physical theory should be discarded *as an ideal*. Kelvin is simply pointing out, if you like, that

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(17) P. Duhem, *The Aim...*, *quot.*, 85.
(18) H. Poincaré, *Electricité et optique*, 2 vols., Paris 1901, vol. I, *Les théories de Maxwell et la théorie électromagnétique de la lumière*, "Introduction", ix.

the way forward to a unified and empirically complete theory *may be* through a series of disunified empirically incomplete models. The way forward in science *may* be to go ahead with constructing partial models in the hope that they may each have some success and that, in the long term, a synthesis can be achieved which inherits all the successes. This is the sort of thing which Gell-Mann had in mind in his famous metaphor. Gell-Mann compared model building to a technique in French cuisine in which a piece of pheasant, for example, might be cooked between two slices of veal which are then discarded. A simplified partial model will inevitably be discarded in the future but might in the meanwhile teach us something which is retained in the eventual general theory.

The claim that models *may* be useful is such a weak one that even Duhem had finally to agree with it:

> Let us admit frankly that the use of mechanical models has been able to guide certain physicists on the road to discovery and that it is still able to lead to other findings (19).

On the other hand the claim that proliferating models *always* lead to success is such a strong one that no one would ever make it — its falsity can safely be conceded to Duhem. There is, of course, no guarantee that *any* heuristic method or indeed any research programme will lead to success. At this level there is an unavoidably intuitive element in physics and a question of luck. Those who committed themselves to the programme to produce a mechanical reduction of the electromagnetic field were unlucky — but surely they were not "irrational". There was no convincing reason *in advance* why they were bound to be unlucky. On the other hand, those who committed themselves to the programme to produce a mechanical theory of heat *were* lucky — but this was genuine luck, their success could not have been rationally predicted in advance. The only safe, though methodologically very disappointing, conclusion is that in this respect at least scientists should just be allowed to "do their own thing". There are undoubtedly cases in which formal mathematical considerations have led the way in science and in which models have only subsequently been added *post hoc* — like, as Duhem put it, parasites. On the other hand, there are equally undoubtedly cases in which "modelling" has been productive of a general, fully-fledged and accepted theory.

4. Models and mathematics in harmony not conflict

Duhem, then, tried to cast formal, mathematical considerations, on the one hand, and realistic, model considerations on the other as competitors or rivals. But in the end the conflict fizzles out — at most one is left arguing only about the comparative importance of the heuristic roles played by the two, and since neither role is negligible this argument seems of little significance. This

(19) P. Duhem, *The Aim ...*, *quot.*, 99.

conclusion can be taken one stage further I believe: it is not clear that, in practice, formal and "realistic" considerations are as readily separable as Duhem seems to have held; these two sorts of considerations are instead very closely interwined.

I do not have the time to develop this thesis in any detail here but will instead conclude by sketching very roughly a few of the points which underlie the thesis.

a) First of all, a good deal of "pure" mathematics is itself model-based. The classic example is, of course, geometry, which according to Einstein constitutes "one of the oldest physical theories". While according to Newton:

geometry is founded in mechanical practice and is nothing but that part of universal mechanics which accurately proposes and demonstrates the art of measuring (20).

Euclidean geometry is undoubtedly an idealisation, but nonetheless an idealised attempted description of *real physical space*.

b) Let us consider a case in which, according to Duhem, abstract mathematical considerations led the way. His idea was that progress was often achieved by trying out in some new area *equations of the same form* as ones that had already proved successful in some quite different area. And for an example he gives Huygens's and later Young's development of the wave theory of light through formal analogy with the theory of sound. His reason for insisting on the formal nature of the analogy was that sound and light are quite different *sorts* of things. While this is surely so, it is also surely true that no *mere* formal considerations guided Huygens and Young. They held the *realistic theory* that light is a disturbance in a continuous mechanical, elastic medium. It was this that, of course, legitimated their exploitation of the mathematical results already achieved in the theory of sound — in so far as these results did not depend on any assumption about the air which did not carry over to the aether. The idea strikes me as wild that a scientist might simply decide to try out some formal equations from area A in area B without believing that, though different, area A and area B possess *real* similarities. And if so, then realistic and formal considerations simply go hand in hand. Did Fresnel instinctively resort to a $\sin(2\pi/\lambda)(x - vt)$ as the equation for his optical displacement because the analogous equation had already been developed for sound waves? Perhaps, but certainly not for purely formal reasons; but instead because his *realistic theory* was that the vibrations of the light source set up small disturbances of the aether particles from their equilibrium positions and that the aether was an elastic medium just like the air. It *followed* that sound and light waves would be formally indistinguishable and that therefore he could exploit the existing mathematics for sound.

(20) I. Newton, *Principia*, "Preface".

c) Of course, matters need not always be as clear cut as this. A scientist may only have succeeded in formulating his realistic claims rather vaguely when he looks round for some mathematical theory in which to express them. The resulting mathematical expression being much more precise will have consequences which his original vague ideas did not. Thus mathematics creates so-called "surplus content" — but again this extra content will immediately be physically interpreted.

d) One particularly clear cut way in which this can happen is that some term crops up in the mathematical expression of the theory which has no immediately obvious physical interpretation — yet such an interpretation is sought and leads to a theory with increased content. (This possibility and indeed the whole question of the heuristic role of mathematics in physics, has been studied in much greater depth than I can go to here by my colleague Elie Zahar.) One famous example concerns Fresnel's equations for the relative intensities of reflected and refracted light. In the case of internal reflection within transparent media at angles greater than the critical angle, Fresnel's equations contain certain imaginary quantities. Insisting on a real, physical interpretation of these quantities, Fresnel in fact interpreted them as signifying a certain change of phase: an interpretation which led to the famous (and of course successful) prediction of the creation of "circularly polarised" light by two internal reflections of plane polarised light within a Fresnel rhomb.

Elie Zahar has expressed the view that:

> the relationship between mathematics and physics is best described in dialectical terms as a to and fro movement between two poles. One moves from physical principles to idealising mathematical assumptions, then back to some more physics; then forward to fresh mathematical innovations with ever increasing surplus structure (21).

I would only add that this to and fro movement occurs at such speed as to make all claims about mathematical or model considerations leading the way difficult to become excited about. In physical discovery it is not, as Duhem wanted to suggest, a question of mathematics *versus* explanatory physical principles or models, but instead a question of the two energetically interacting in the difficult attempt to prise open Nature's secrets.

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(21) E. ZAHAR, "Einstein, Meyerson and the Role of Mathematics in Physical Discovery", *Brit. J. Phil. Sci.*, *31* 1980, 1–43.