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DUHEM AND CARTWRIGHT ON THE TRUTH OF LAWS

ABSTRACT. Nancy Cartwright has drawn attention to how explanations are actually given in mathematical sciences. She argues that these procedures support an antirealist thesis that fundamental explanatory laws are not true. Moreover, she claims to be be essentially following Duhem's line of thought in developing this thesis. Without wishing to detract from the importance of her observations, it is suggested that they do not necessarily require the antirealist thesis. The antirealist interpretation of Duhem is also disputed. It is argued that Duhemian points, often understood antirealistically, bear a realist construal, and that antirealist interpretations of Duhem typically run into problems of consistency or of reducing his position to absurdity.

1. PREAMBLE

Physics has long been revered as the foremost science. This view is often ascribed to upholders of the so-called standard view which came under heavy criticism in the 1960s. It is a major feature of the idiosyncratic turn Quine gives to the doctrine of no first philosophy, and it continues to be a major source of inspiration in the philosophy of science. This view was challenged on the basis of the thesis associated (rightly or wrongly) with Kuhn that not even physics is objective, and so is comparable with the social and political sciences. Whatever the therapeutic value of this great levelling thesis and its invigorating effect on the philosophy of science, it has, however, proved difficult to turn the Kuhnian insights into a coherent, defensible positive philosophical doctrine.

This lacuna of positive doctrine raises the suspicion that the critique of the early 1960s wasn't thorough enough. More recently, Nancy Cartwright (1983) has again challenged the view of the natural sciences, physics in particular, as conforming to a preconceived philosopher's picture of impeccable clarity and stately deduction. The focus of her attack is the embodiment of this Cartesian ideal in the DN model of explanation. Now, criticism is no stranger to this model. The unrelenting barrage of attacks since the late 1950s has finally silenced Hempel's incredible line of defense of his *analysis* of the concept of explanation, according to which the putative counterexamples are to be discounted

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because they do not fulfill the requirements of the model! But again it has proved difficult to turn critical intuitions into a positive account, and various views of explanation proliferate, their proponents agreeing on little more than that the DN model is wrong. So, has Cartwright a more promising conception to offer? The messy picture she paints of physical science has surely the lasting value of bringing us closer to the real thing, reinforcing Kuhn's desire for a philosophy based on a more realistic view of science as it is to be found in the hands of its human practitioners. And I concur in the anti-Humean emphasis on causation and capacities (cf. Needham 1988) which she sees as necessary to accommodate the actual use of laws and which is developed further in her 1989 book. But a certain kind of antirealism expressed by the title, *How the Laws of Physics Lie*, of her 1983 book is, I shall argue, neither necessary nor desirable.

In Cartwright (1989), the antirealist jargon is toned down, but the thesis is certainly not rejected. The earlier book is often cited there, and the antirealist thesis in question here is given the following formulation in connection with a discussion of Mill's views:

> the fundamental laws (non-empirical laws, in Mill's terminology) do not hold for the most part, or even approximately for the most part; and conversely, those laws which are more or less true are not fundamental. (Cartwright 1989, p. 174)

A distinction between fundamental and nonfundamental laws in relation to their role in explanation and phenomenological description is central to her thesis and is taken up in the next section, prior to discussing the thesis itself in Section 3. Now Cartwright claims that her thesis captures Duhem's views on explanation in physics, and this brings me to the second theme of this article. She is by no means alone in advocating an antirealist interpretation of Duhem which I wish to dispute. I shall suggest in Section 4 that a certain attitude towards what can reasonably have been regarded as acceptable explanation at the time he was writing does not square with Cartwright's construal of phenomenological description as that which is opposed to what is fundamental and explanatory. Duhem's concern for what laws can reasonably be taken to mean is taken up in Section 5 and distinguished from what a certain brand of realist interprets as considerations turning on a conception of nearness to truth. Taken together, this shows Duhem could not have held the fundamental laws of physics to be false.

2. REALISM AND ANTIREALISM IN CARTWRIGHT

Cartwright rises to the challenge posed by van Fraassen's antirealism. She draws no distinction between the observable and the theoretical on the basis of which van Fraassen (1980) spells out his conception of the underdetermination of theories in terms of which he formulates his antirealist doctrine of 'constructive empiricism'. Rather, she introduces what she claims is the physicist's distinction between what is and is not fundamental. The latter is what, she says, scientists understand by phenomenological laws, quoting a dictionary of physics: "A phenomenological theory relates observed phenomena by postulating certain equations but does not enquire too deeply into their fundamental significance" (Cartwright 1983, p. 1), with the rider that 'observed' here is not to be read as the philosopher's 'directly observed'. 'Phenomenological' has to be understood so that we can make sense of the proceedings of a physics conference being published under the title *Phenomenology of Particles at High Energies* (Cartwright 1983, p. 1).

The correlate of this is a use of 'theoretical', contrasting with nonfundamental or 'phenomenological', conforming to how physicists use this term. Now Cartwright says that phenomenological laws are true. She is opposing a picture painted by many philosophers according to which phenomenological laws are at best approximately true, standing corrected by various theoretically motivated qualifications. Swartz, who distinguishes between 'near' and 'real' laws, is perhaps an example of such a philosopher. Real laws, for Swartz, might not even be known, and by contrast with what he says scientists call laws, they refer to

> a certain class of truths about this world ... wholly independent of whether anyone successfully discovers, formulates, announces, believes or promotes those truths ... I mean ... those true propositions that lie at the heart of the matter (no pun) and account for the verisimilitude of the pronouncements of science. (Schwartz, p. 4)

Cartwright argues, on the other hand, that corrections are brought in to introduce a wider generalisation, whereas really phenomenological laws are highly specific and their truth is related to this fact. An example would be van de Waals's equation, a distinct improvement over the ideal gas equation gained at the price of introducing two constants which are different for every gas. Other equations give even better descriptions under various conditions, particularly near the critical point (of maximum pressure and minimum temperature where liquid and vapour coexist – see, e.g., Castellan 1964, ch. 3, for details). (It would seem to follow that the ideal gas law is not phenomenological, failing this second criterion of truth by virtue of exact description. Whether this is consistent with how she understands the first criterion of the phenomenological as non-explanatory, which is what I take it 'non-fundamental' comes to on her view, is doubtful. I shall take up this theme in Section 4.)

Phenomenological laws are true, according to Cartwright, because they are accurate though highly specific. Generality, on the other hand, is something we strive for at the fundamental level and for which we pay a price. In order to be applicable in very diverse contexts, fundamental laws must be simple and, at best, approximately true. In fact, she says they are false. The provisions for a picture of the underdetermination of theories somewhat similar to van Fraassen's thus begin to emerge. Indeed, Cartwright goes on to talk of how usual it is to have alternative theoretical explanations (Cartwright 1983, p. 78ff.), no single one of which is the 'right' one (Cartwright 1983, p. 17). The point of theoretical explanation is to summarise and organise by placement in a general setting, but "[t]here is nothing about successful organisation that requires truth" (Cartwright 1983, p. 17).

With lines differently drawn but what looks like a picture of theories of similar general structure, Cartwright seems thus far to be an antirealist of the van Fraassen ilk. But now she goes on to make realist claims about theoretical entities. With what smacks of the realist thesis Putnam offered to counter Kuhnian relativism, she says that theoretical laws may not be true but theoretical entities exist. Taking up van Fraassen's challenge to say when inference to the best explanation works, she shuns talk of the miraculous, however, and speaks instead of causes, arguing that we infer to causes which by their nature exclude competing alternative causes.

Causal statements play an essential role in describing models on the basis of which fundamental laws can be applied, namely, in the simple, approximate, tractable form laid down in the model. For example, in chemistry, application of the Schrödinger equation is usually reduced to a one electron problem in describing the electronic structure of the elements, or the conjugated pi-bonding which characterises the aromatic chemistry of benzene as described by the Hueckel theory. A model is devised in which remaining electrons are envisaged as shielding an 'outer' electron from much of the nuclear charge. Rather than deriving the total description of all the electrons in the atoms of a molecule as theory theoretically directs, much of the detailed description is in effect assumed from the outset by adopting the model, and only a few remaining details, construed in the particular explanation at issue to be of immediate relevance, are derived from this point as best we can. 'Shield' is a causal verb and the example illustrates how causal statements go to make up a description of the model. There is no question of reducing these to regularities supposedly expressed by the laws of physics; they are needed to render fundamental laws applicable to phenomena. Without them, the laws could not be brought to bear on the real world in explanation.

To say otherwise is to appeal to the never-never land invoked by Hempelian talk of explanation sketches, Goodmanian talk of law-like generalisations, Quinean talk of structural descriptions sanctioning our use of disposition terms, and Davidsonian talk of strict laws. It must be appreciated that physical science is not the place where the explanation sketches are, as a matter of fact, cashed in for hard initial conditions and strict regularities. Critics of the DN model as a model of historical explanation are still to be found arguing for a distinction between sciences and humanities on the grounds that the model captures what is the essence of explanation in the former's domain. The mounting dissatisfaction over the last thirty years with the model as an account of scientific explanation should itself have been sufficient to check writers churning out this old story. But the present line of argument parallels one used against the DN model as a model of historical explanation, namely, that it is not, as a matter of fact, employed by practitioners of the appropriate discipline and strengthens this critique with the observation that the model is nowhere to be found in practice.

It might be objected that her position is rather weak, not being based on any kind of demonstration of the unsolvability of differential equations and of the necessity of using approximation methods based on models. Perhaps the science of five hundred years hence will be one where the differential equations formulated in accordance with the recipes laid down by the fundamental laws are all rigorously solved and the applications of laws is no longer the colossal problem which at the moment makes this look like 'pie in the sky'. But science would be in a sorry state were it to be judged by an ideal of scientific worth that is hardly to be found realised by any extant, scientific practice. (Bear in mind that the better a putative explanation conforms to the model is not clearly an indication of a better explanation. The explanation of a broken water pipe, in terms of the fact that all undrained outside water pipes burst in harsh winters, is hardly better than one in terms of the variation in the short-range intermolecular structure of water around the freezing point, although it conforms to the DN model.) The Kuhnian thesis that there is no objectivity in physical science is absurd: physics is a paradigm of objectivity and to deny this merely shows the understanding of objectivity, on which the claim is based, is flawed. *Mutatis mutandis* for an inapplicable criterion of scientific explanation.

3. THE IMPLICATIONS OF MODALITY

The force of this critique of the DN model is not easily denied; the question is whether it is best accommodated by the thesis that the laws of physics 'lie'. Might it not instead reflect on the concept of truth and the nature of what is held true? Cartwright alludes to both themes, ascribing to van Fraassen and Duhem the view "that truth is an external characteristic of explanation" (Cartwright 1983, p. 89), which she contrasts with her contention that "causal explanations have truth built into them . . . an existential component, not just an optional extra ingredient" (Cartwright, 1983, p. 91). But now the thesis that theoretical entities exist must involve the truth of existential statements. And what sense can be made of existential statements in the absence of truths about the things said to exist? Cartwright has given us such truths, namely, causal statements about these entities and causal histories describing their behaviour that go into constructing models of phenomena. These causal statements are certainly not phenomenological truths on her account of the latter notion, and I think it is clear that she must mean them as theoretical truths.

An anti-Humean theme running throughout Cartwright (1983) is introduced in Essay 1, where it is argued that constant conjunction is not all there is to causation. This is quite a popular theme these days, having been taken up by a number of authors. For her part, Cartwright argues (Essay 3) that fundamental laws must be interpreted as ascribing causal powers. As things stand, it is false that two bodies of mass m and M, distance r apart, sustain a force of magnitude GmM/r^2 as Newton's law would have it. If bodies are charged, too, there will be a contribution of electric force. Now it's no good, she says, trying to build a qualification into the gravitational law along the lines "All bodies of mass ... and not subject to electromagnetic forces and suchlike, attract one another". Such modification is pointless because the result would never bear on any actual phenomena and, thus, would be useless as an explanatory tool. What the law says, Cartwright suggests, is that bodies have the *power* of attracting one another in a certain way; that if the gravitational attraction *were* the only force exerted, then the force *would* be .... These modal statements then underlie the causal statements used in model construction. An important step in the argument is that, in general, there aren't laws available to deal with the combination of several factors in the production of actual phenomena. Part of the job of model building is to cope with the introduction of factors described by separate, simple, fundamental laws. These laws gain purchase on the phenomenon when interpreted in this causal fashion.

This is, she admits, all very sketchy.

> [E]ven inside the model, derivation is not what the D-N account would have it be, and I do not have any clear alternative. This is partly because I do not know how to treat causality. The best theoretical treatments get right a significant number of phenomenological laws. But they must also tell the right causal stories .... often, once the causal principles are understood from a simple model, they must be imported to more complex models which cover a wide variety of behaviour .... But I do not have a philosophic theory of how it is done .... We need a theory of explanation which shows the relationship between causal processes and fundamental laws we use to study them, and neither my simulacrum account nor the traditional covering-law account are of much help. (Cartwright 1983, pp. 161–62)

And of the "abstract relation between capacities and properties" discussed in her later book, she says, "I cannot yet give a positive account of what it does mean" (Cartwright 1989, p. 9). Much remains to be done then, but she hopes to have pointed the way and her claims should be evaluated in this light.

Causal statements are not reducible to regularities; the modal idiom has to be taken seriously. Subjunctive conditionals have to be taken at face value and not as though the modal element can be whisked away. This line of thought is not so much an argument for the falsity of fundamental laws as an argument against interpreting these as regularities and for interpreting them instead as modal statements – modal truths, albeit of uncertain form. It is a dogma of the received view that the laws of science are straightforward generalisations, whose logical form is readily stated in predicate logical terms with some as yet unstated general restrictions on allowed predicates. But the formulations to be found in the scientific literature do not wear any such form on their sleeves, and the interpretation of scientific discourse is, like any other, subject to the needs of adequate regimentation. I interpret Cartwright to be arguing that the regimentation should reflect and be governed by the actual manner in which the scientist applies his laws when explaining phenomena. I interpret the force of her arguments to the effect that fundamental laws are not regularities but modal statements of some kind.

4. OBSERVABILITY, INTELLIGIBILITY AND EXPLANATION

Cartwright claims to follow Duhem in developing her antirealist simulacrum model and the thesis that the laws of physics 'lie', and opposes him in her realistic thesis about the existence of theoretical entities. It was suggested in the last section that the latter thesis does not sit happily with the former, and I now want to go on to question the interpretation of Duhem. Let me say first, however, that the antireductionism Cartwright seems to espouse in opposing the metaphysics of the DN account does seem to be in the spirit of Duhem's antireductionist (cf. Nye 1976, pp. 262–65), holistic attitude, which I therefore try to retain without the antirealist thesis.

An immediate problem is the significance and understanding given to the notion of phenomenological laws. It seems that pure or phenomenological thermodynamics, as it is variously called, provided something of a paradigm for Duhem, but this theory is surprisingly not mentioned by Cartwright, who discusses only statistical thermodynamics. Perhaps Cartwright's criterion of accuracy (though not necessarily at the price of generality) applies to this application of 'phenomenological', but I especially want to suggest in this section that her first criterion, of being merely descriptive and devoid of explanatory import, does not apply in this case. It is not possible to pursue in any detail here why pure thermodynamics is reasonably called phenomenological; but it certainly cannot be because the term stands opposed to 'theoretical', even though the theory makes no appeal to microstructure. The concern with macroscopic phenomena, and thereby with some reasonable sense of what is observable – which doesn't mean with positivist accounts of observability – is relevant. In this sense, the phenomenological would not, as Cartwright says, be a matter of what philosophers have called the directly observable. It remains to develop a detailed systematic account, but for present purposes I shall be arguing from a few intuitions in the way Cartwright does. The point I want to make about her first criterion is a preliminary to considering Duhem's particular views. This point is to suggest that a turn-of-the-century view, which Nye (1972, Introduction) argues were typically antiatomistic pro continuous theories of matter, *need* not be assimilated to positivistic views, and can be considered to be motivated by a concern to provide explanation and understanding comparable to the atomist's, but animated by the conviction that thermodynamics, for example, does the job better. Hopefully, this will serve to clear the air of the automatic connection that historians often make between antiatomistic and positivist views, and that philosophers often assume between realism and explanation by appeal to microstructure, and provide a background against which Duhem can be more favourably presented as a realist in the next section.

The idea that explanation is really only provided by appeal to microstructure is encouraged by standard textbook presentations of thermodynamics, where formulae are explained directly in terms of molecular properties. For example, consider the relation between the specific heats of gases at constant pressure, C_P, and constant volume, C_V (the specific heat of a quantity of material is the quantity of heat required to raise the temperature of the material one degree), which was one source of problems which plagued the kinetic theory during the nineteenth century. From the first law, the following expression is derived:

$$(1) \qquad C_P - C_V = [P + (\partial E/\partial V)_T](\partial V/\partial T)_P .$$

The first term on the right corresponds to the work produced by expansion at pressure P per unit temperature increase. The second term, involving the internal energy E, is often explained in textbooks in molecular terms, as the energy required to pull molecules further apart, against the intermolecular attractive forces. This makes it easy to understand why C_P is greater than C_V. At constant pressure heat taken from the surroundings goes three ways: (i) producing work in the surroundings as a result of expansion; (ii) providing energy to separate the molecules; and (iii) increasing the chaotic motion of the molecules. But only (iii) is reflected in an increase in temperature.

It might be said that the molecular interpretation is introduced for purely pedagogical reasons,[1] thermodynamics having a notorious reputation in the physical chemistry curriculum. Some more advanced tracts develop the subject independently of ideas taken from statistical mechanics, but the impression remains that many authors view reduction to microproperties as providing the only real source of explanation. This tradition goes back to the beginnings of the subject. Thus Maxwell, commenting on Clausius's formulation of the second law, arrived at after modification of its unsatisfactory predecessors, says,

> in order to understand it we must have a previous knowledge of the theory of . . . entropy, and it is to be feared that we shall have to be taught thermodynamics for several generations before we can expect beginners to receive as axiomatic the theory of entropy. (Maxwell, p. 668)

He goes on to say "It is probably impossible to reduce the second law of thermodynamics to a form as axiomatic as that of the first law, for we have reason to believe that though true, its truth is not of the same order as that of the first law" (Maxwell, p. 669). This is because "we have reason for believing the truth of the second law to be of the nature of a strong probability, which, though it falls short of a certainty by less than any assignable quantity, is not an absolute certainty" (Maxwell, p. 671).

What begins as a difficult, highly abstract concept is reduced on this treatment, first, by providing a mechanical interpretation which is presumed more easily understood (and therefore explanatory?) and, then, rendered less secure by showing that the law which gives the concept its significance is, on this interpretation, merely "of the nature of a strong probability". Seen in this light, phenomenological thermodynamics would be phenomenological in Cartwright's sense. Objections have been raised to the reduction, such as those based on the reversibility problem. Referring to Poincaré (1893), this is the second of what Nye (1976, pp. 254–55) calls "two observational arguments" advanced against the kinetic theory. Nye's surprising choice of terminology suggests indirectly that what counts in favour of phenomenological thermodynamics has nothing to do with explanation. But the idea that the issue is a matter of an opposition between explaining and understanding, on the one hand, and observational adequacy, on the other, is misleading. It misrepresents phenomenological thermodynamics as understood by Duhem, who sees it as providing explanations and not itself standing in need of atomic explanations.

To illustrate this, consider again the relation between the specific heats of gases expressed by (1). Joule's experimentally determined law for gases states that $(\partial E/\partial V)_T = 0$, and this, together with the ideal gas law $V = nRT/P$, allows us to conclude from (1) that $C_P - C_V = nR$, a positive number. But a purely thermodynamic argument can be given to explain why C_P is greater than C_V on the basis of the second law. In outline, this runs as follows. The thermal expansion coefficient, α, is defined by

(2) $$\alpha = (\partial V/\partial T)_P \cdot 1/V,$$

and the compressibility coefficient β by

(3) $$\beta = -(\partial V/\partial P)_T \cdot 1/V.$$

The cyclic rule in the form $(\partial V/\partial P)_T(\partial P/\partial T)_V = -(\partial V/\partial T)_P$ yields

(4) $$(\partial P/\partial T)_V = \alpha/\beta.$$

Now, substituting the equilibrium condition $TdS = đQ_{\text{rev}}$ (determined by the second law) in the first law quickly leads to

(5) $$(\partial E/\partial V)_T = T(\partial S/\partial V)_T - P,$$

an expression for one of the terms in (1). The so-called Maxwell relations (obtained by identifying cross derivatives like $\partial^2 A/\partial V \partial T = \partial^2 A/\partial T \partial V$ of functions of state in accordance with Euler's criterion) give $(\partial S/\partial V)_T = (\partial P/\partial T)_V$, so from (4) and (5) we have

(6) $$(\partial E/\partial V)_T = T\alpha/\beta - P.$$

Substituting this and (2) in (1) yields

(7) $$C_P - C_V = [P + T\alpha/\beta - P]V\alpha = TV\alpha^2/\beta.$$

Since T, V, α and β are all positive, $C_P > C_V$.

The kinetic theory is not needed, then, to obtain this result. Why shouldn't it be considered an explanation? Whatever the merits of the reduction to mechanics, it is gratuitous to represent those turn-of-the-century scientists who took seriously objections raised against the kinetic theory as motivated by a positivistic obsession with observability and with no concern to gain understanding on a par with Maxwell's. I doubt that much of the resistance to the atomic hypothesis at the end of the last century was primarily motivated by misgivings about unobservability, even when apparently expressed as such, at least in any sense of what is directly observable and associated with traditional empiricist or positivist views. The primary question was one of intelligibility. Now this is, of course, a necessary condition of observability in any reasonable sense of the term. But it seems that the converse was also understood, so that unintelligibility could be expressed by unobservability. We find Maxwell himself doing this. He describes as unobservable ("contradictory to all our experience") what he evidently finds unintelligible when presenting a theory of William Thompson's in which gravitational attraction is explained on the basis of an incompressible fluid occupying all space which is either generated and emitted by all material bodies at a constant rate and flowing off to infinity, or absorbed and annihilated by material bodies at a constant rate with the deficiency constantly restored from infinite space. Maxwell says,

> in either of these cases, there would be an attraction between any two bodies inversely as the square of the distance . . . . But the conception of a fluid constantly flowing out of a body without any supply from without, or flowing into it without any way of escape, is so contradictory to all our experience, that an hypothesis, of which it is an essential part, cannot be called an *explanation* of the phenomenon of gravitation. (Maxwell, p. 489)

Similarly, the enemies of atomism had ample grounds on which to question the intelligibility of the atomic hypothesis. For instance, perfectly hard, inelastic atoms would produce infinite forces on impact, as Kant had maintained. Nye (1976) classifies this as the first of her "two observational arguments". This argument prompted kinetic theorists to define their gases as made up of perfectly elastic spheres. But Maxwell fully appreciated the paradox of endowing atoms with "the very property for the explanation of which, as exhibited in aggreggate bodies, the atomic constitution was originally assumed" (Maxwell, p. 471), and criticised his fellow atomist Boltzmann for suggesting that a body could be both rigid and elastic (Brush 1967, p. 161ff.). Again, how could there be a truly elementary particle carrying a finite electric charge, as an electron was said to be? The mutual repulsion between the parts would render it unstable unless it is not elementary after all and there are counteracting internal forces holding it together . . . and so on.

It seemed that the atomic hypotheses typically necessitated the postulation of subatomic structure without holding out any hope of providing a way of stopping there. No wonder the view was taken that it is a mistake to embark on what seemed to be a regress, this feature normally being taken to nullify explanatory claims. And where the nature of the hypothesised atoms did not immediately call for further explanatory substructure, the explanatory value of the hypothesis could again be questioned. The homogeneous atoms Dalton postulated to 'explain' the laws of constant and multiple proportions do nothing of the sort. If it is a mystery[2] why these laws are true for macroscopic quantities of material, it is equally mysterious why substances should comprise minimal, homogeneous lumps of the same substance which combine with others as indivisible wholes. Postulating inexplicably minimal atoms renders nothing more intelligible, and it is surely a virtue of the approach of Wollaston, and of later advocates of thermodynamic theories, that they were held to be neutral with respect to atomic hypotheses.[3] The all too familiar assessment of Wollaston as a short-sighted positivist resisting what might be considered to be the beginnings of the atomic theory of valence misses this eminently reasonable critique of Dalton's theory. There is now convincing empirical reason for definitely postulating atomic theories, which is one factor in the light of which Duhem is now criticised. But that is to be distinguished from *a priori* atomism which, whatever its heuristic virtues, shares epistemologically the fate of most *a priori* claims. The neutral stance is to refrain from introducing postulates of no empirical and, hence, explanatory value.

Perrin's discovery that Brownian motion can be observed in such a manner as to be precisely explicable by the kinetic theory was granted to be direct experimental proof of the atomic hypothesis by steadfast opponents such as Ostwald, if not Mach.[4] But this meant that the unintelligibility arguments, which continued to infirm the explanatory value of atomic theories, must somehow be overcome. Now we have the quantum theory, which at least blocks the formulation of these arguments. Whether it has thereby rendered the atomic hypothesis intelligible is the problem of interpretation which many would regard as unresolved. And to the extent that the difficulty of interpretation renders it unclear how the theory can reasonably be held true, this must surely reflect on its explanatory adequacy. Isn't Maxwell's rejection of Thompson's explanation of gravitation a rejection on the grounds that it can't be held true? At any rate, Duhem, I shall argue in the next section, is best understood as holding only that which can be held true can be held to explain.

As a final point on observability and intelligibility before turning to that, it is salient to note that microbiology deals with what, on views like van Fraassen's, are unobservable entities. But antirealist challenges to what is held true about bacteria and viruses are hardly ever heard. It might be said that this is because they are not really unobservable. This view might be held for various reasons; but a particularly important one, I suggest, is the absence of considerations comparable to those which make the atomic realm seem so utterly strange. This, I think, is why many of us are prepared to say we see bacteria through a microscope but not that we see a crystal structure in an X-ray photograph. We have acclimatised ourselves to abstract ideas of entropy and a finite but unbounded universe; but the atomic domain remains beyond the pale. Smallness is not really an issue here, and I find it difficult to see that mere size sets any limit determining a modal notion of observability of any interest in the philosophy of science.

5. DUHEM

I've suggested that the opponents of atomism of the last century didn't in general rest their case on grounds of unobservability understood in traditional empiricist or positivist terms. Certainly Duhem's well-known holistic conception of experimental import is at odds with traditional empiricist-inspired distinctions between the observable and the theoretical.[5] Cartwright would seem to agree that this leaves room for a notion of phenomena and phenomenological laws couched in terms used to describe phenomena. I shall argue that Duhem's development of these notions does not allow the schism between explanation and truth that she and others attribute to him.

I shall not reveal passages where Duhem explicitly talks about explanation and truth but, rather, I shall emphasise features he stressed which are simply not accommodated by the usual instrumentalist, antirealist interpretation of his philosophy. Indeed, it is true that he expressed himself in such a way that explanation was the sole domain of the atomic factory builders, which is opposed to what he called natural classification.[6] Taking a cue from his terminology, we can see how one might be led to say that "he opposed . . . theoretical laws whose only ground is their ability to explain", and that he might be likened to van

Fraassen in holding "that the theory is true is a gratuitous additional assumption" (Cartwright 1983, p. 88). On the other hand, I would call his notion of incorporation in the natural classification, illustrated by classical thermodynamics, an explanation; his rejection of gratuitous additional assumptions I characterise as a rejection of purely metaphysical assumptions unrelated to phenomena; and I see no reason to think he did not hold successful natural classification as the truth.[7] In this sense Cartwright departs from Duhem with her thesis that "[t]here is nothing in successful organisation that requires truth". He might be criticised for underestimating to what extent efficient organisation conflicts with the plain truth, but it would not be true to say he did not address the problem. He well understood that the mathematical formalism often suggests matters of detail which have no bearing on phenomena and which are not reasonably understood as essential to the intended interpretation of the laws and principles they are used to convey. This is highly relevant to contemporary discussions of scientific progress and associated notions, and excludes realism of the miracle-argument-cum-verisimilitude variety. My thesis is that he was a moderate realist in the sense of maintaining that whatever statements are regarded as meaningful parts of the body of scientific theory are held to be true.

It is interesting to note that Bogen and Woodward (1988) have recently made a convincing case for distinguishing between data and phenomena, the former being what is ordinarily said to be observed, perceived or detected, and the latter what is explained. Phenomena are inferred or identified from data as what is systematically, consistently and reliably reproducible, independent of any explanations that might be offered for the actual appearance of the data. They illustrate this by criticising a passage of Nagel's in which he suggests a sentence like 'Lead melts at 327° C' reports what is observed. Melting points are determined by making a series of measurements from which the phenomenon of lead's melting at $327.5 \pm 0.1°$ C is inferred. The body of data itself has features not explained by whatever explanation might be offered for the phenomenon itself, but, for example, by reference to impurities, time taken to establish thermodynamic equilibrium, how the thermometer is read and corrected, etc.

Bogen and Woodward's conception of phenomena is in fact very similar to Duhem's, who has an extended discussion with many examples of how theory is brought to bear in a detailed consideration of the sources of error, after which an "interpretation substitutes for the concrete data really gathered by observation abstract and symbolic representations which correspond to them by virtue of theories admitted by the observer" (Duhem, p. 147). Duhem takes these theories presupposed by the conducting of experiments and use of instruments to include those explaining the data in Bogen and Woodward's sense, as well as those explaining the phenomena of primary interest. But reproducible features of data (systematic errors) themselves count as phenomena. Duhem stresses that the identification and characterisation of phenomena is done with an appropriate application of the qualification 'approximately', in terms of which he talks of the 'physical meaning' of laws (Duhem, p. 215).

By contrast with the situation as he depicts it in the physical sciences, Duhem considered that an experiment in physiology might well yield "a raw description of the facts" (Duhem, p. 182). (He also talks of "concrete", "concrete and obvious" and "practical" facts which are contrasted with the "precise and rigorous" facts with which physics deals; e.g. Duhem, pp. 147–53.) He would agree with Bogen and Woodward's description of data as "what is uncontroversially observable" (Bogen and Woodward, p. 314) so long as science is seen to be concerned with phenomena which "[f]or the most part . . . cannot be perceived and, in any case, the justification of claims about the existence of [which] does not turn, to any great extent, on facts about the operation of the human system" (Bogen and Woodward, p. 350). Duhem claimed the task of science is to save the phenomena in this sense, whereas Bogen and Woodward say van Fraassen sees the aim of science more in terms of saving the data – what can be observed, understood as a function of the operation of the human perceptual system. Perhaps at the time Plato coined the phrase, saving the phenomena for astronomy was much the same as saving the data, just as Duhem, in effect, claimed was the case in physiology in his time. But however that may be, the distinction cannot be ignored in modern science in general and Duhem's account of it in particular. Laws are thus employed in inferring and identifying phenomena in line with Harman's use of his notion of inference to the best explanation,[8] giving the most coherent account of all the data. Duhem means they are taken as meaningful insofar as they fit this role, which can only be understood as holding them to be true. His account of theory and explanation cannot possibly be assimilated into van Fraassen's antirealist attitude of suspended belief towards laws with explanatory pretensions.[9]

This conclusion is reinforced by the fact that Duhem developed the above considerations of the interpretation of phenomena into a critique of Poincaré's conventionalism, which was an aspect of his structural realism. In the extreme case, theoretical statements are, according to this doctrine, taken to be immune from experimental falsification; they are conventions. But this, Duhem argued, is to consider the statements in isolation, when they become "mathematical statements deprived of all physical meaning" (Duhem, p. 215). If this point is not heeded, then even simple laws like that of multiple proportions – asserting that elements combining to form a compound in the mass ratio $a : b : c : \ldots$ combine to form any second compound in the mass ratio $na : mb : lc : \ldots$ where $n, m, l, \ldots$ are integers – never mind the more advanced and highly general theories Poincaré talked of, degenerate into mathematical truisms. For any conceivable experimental result can be approached arbitrarily closely by some choice of integers n, m, l. But understood as laws with physical meaning, the simple mass ratios in the two compounds to which the law of multiple proportions refers must be understood to a degree of approximation commensurate with the accuracy of experimental procedures. Arguments like those used to reduce the principles of Newtonian mechanics to definitions are based on the premise that experimental import can be represented in the form of direct tests. But this is a fabrication which merely deprives laws of meaning, treating them for all intents and purposes as formal objects with purely instrumental significance.

> There can be no adequation between the precise and rigorous theoretical fact and the practical fact with vague and uncertain contours such as our perceptions reveal in everything. That is why the same practical fact can correspond to an infinity of theoretical facts. (Duhem, p. 152)

As meaningful statements about physical reality, laws are tempered by a degree of approximation commensurate with the fineness of distinction allowed by experimental procedure, and so cease to express purely 'theoretical facts' in acquiring meaning. The descriptions of phenomena they offer are the result of rounding off by due consideration of systematic and random error.

I think, then, that David Stump wrongly understood Duhem when he said in his recent article:

The major issue between Poincaré and Duhem is precisely whether or not fundamental laws [a term he takes over from Cartwright] are neither true nor false because they are always approximate, a view that Poincaré rejects, insisting that fundamental laws are (or can be) true. (Stump, p. 340)

Duhem's notion of the approximate character of laws is precisely that in virtue of which they are meaningful and therefore carry a truth value; otherwise, they would be what pure mathematicians call pure formalism (and what scientists call pure mathematics). Note that Duhem's example of the law of multiple proportions is presumably a phenomenological and not a fundamental law on the Cartwrightian view Stump adopts. His interpretation would thus have Duhem denying the truth even of such statements, which would be absurd by Cartwright's lights.

Just as Stump's interpretation gives no reasonable weight to Duhem's talk of meaningfulness, Peter Clark's rendering of Duhem's views is equally uncharitable. He actually says, in the course of expounding his instrumentalistic interpretation, that Duhem held that

the general physical principles are purely systems of classification, decisions between rivals being based upon the convention of simplicity and economy. They are contentless, neither true nor false, yet they imply (have as logical consequences) laws which have content. But deduction is content decreasing, which requires that if the general principles have zero content, so must the physical laws which follow from them. (Clark, p. 92, fn. 218)

But if a writer can be represented as unable to see this point, the possibilities of interpretation multiply arbitrarily. Surely this consequence is better seen as a criticism of the instrumentalist interpretation.

The instrumentalist interpretation of Duhem is nurtured by the grand style in which he dismissed atomic hypotheses as metaphysical twaddle. In "preparing himself for perhaps too easy a success", Louis de Broglie says in his preface that "the passages in which [Duhem] exposes almost to derision the notion of the electron . . . have since received cruel refutation" (Duhem, pp. x-xi), and contemporary realists are all too ready to dismiss Duhem's views on these grounds. But I have tried to argue that scepticism towards atomic hypotheses was well motivated in the last century precisely on the kind of grounds contemporary realists seem to think theories of microstructure are nowadays justified as the literal truth, namely, in terms of the quality of the explanation they provide. Certainly, Duhem went over the top when he ridiculed the mental crutches the weak but broad-minded English scientists needed to conduct their deliberations. But I think we might reasonably speculate that his view would have been different had he lived to see the empirical significance of microphysical theories. There seems to be nothing in principle in his conception to preclude admitting atomic phenomena once they cease to be purely metaphysical speculations and provide a genuine explanatory role in empirical research. Direct observation, as I have said, was never a criterion of admissability. It was never possible to distinguish by any kind of direct observation between the quantity of sugar and the quantity of water in an aqueous sugar solution, but the thermodynamic treatment of solutions he helped to develop relies upon the distinction. Difficulties of interpretation of the quantum mechanical formalism might raise particular problems for the extension and application of Duhem's notion of meaningfulness, and provide stumbling blocks for realism. But if we are talking about this man's conception of science, his invective against atomism cannot reasonably be used as the basis for ascribing to him antirealist views on the grounds that microphysics has since acquired a different status.

Essentially the same criticism Duhem directed against Poincaré can be directed against the latter-day arguments for the so-called incommensurability thesis which insists that Kepler's laws of planetary motion and Galileo's law of fall are, so far from deducible from Newton's laws, actually inconsistent with them. Lack of entailment here is not so much evidence for change of meaning as failure to take reasonable considerations of significance into account at all. Duhem draws attention to the purely formal contradiction between Kepler's and Newton's laws as these are usually formulated (Duhem, pp.193–94), but argues that distinctions among consequences of the latter beyond the degree of approximation imposed by all the other principles involved in its having any empirical significance at all, exceed their import. If the received view is to be faulted for identifying a rigid, invariant meaning on which its account of theory reduction depends, the meaning variance thesis places too much stress on the merely formal aspects and equally fails to locate the actual content of theories.

These purely formal conflicts may be seen as the result of efficient organisation. Duhem is saying that, considered purely formally, theoretical statements can make no claim to truth. But he is also saying that, considered as such, they are devoid of physical meaning and not part of the body of science at all. They are certainly not part of his natural classification with which he opposes explanation by appeal to purely metaphysically-grounded hypotheses. The mess involved in implementing our more abstract theories is part of a continually evolving process of interpretation, which may be in a pretty poor state and give no good grounds for thinking all the pieces will fall into place under some as yet unwritten unificatory scheme of superlaws. Perhaps Duhem's talk of the natural classification mistakenly suggests otherwise; but I cannot see any reason for thinking he did not view natural classification as that which should be held true.

NOTES

[1] Cf. Nye (1976, p. 201), who points out that proponents of the atomic hypothesis who followed Cannizzaro in extolling its pedagogic virtues were quite reasonably opposed by the opponents of atomism for endangering the status of knowledge of nature by introducing a mental crutch which "would easily be transformed into an ontological *thing* by a new generation of students".

[2] Knight (p. 1) says Dalton made such a claim in a letter to Berzelius.

[3] Philosophers and linguists have tried to capture the intuition of the continuity of matter in analyses of mass terms as homogeneous, defined in terms of a cumulative and a distributive reference condition. See, e.g., Bunt, Röper, and Lønning. I have tried to show how this intuition can be substantially retained in a formulation which is neutral with respect to atomic hypotheses in an article 'Stuff', forthcoming.

[4] Brush 1968, pp. 208–11.

[5] This is why Quine referred to it in connection with his rejection of the second of the 'Two Dogmas of Empiricism' in the footnote added to the reprint in Quine (1961).

[6] We have seen how natural it has seemed to some to conflate the term 'explanation' with 'atomic explanation'. In opposing the idea that the aim of science must be to provide atomic explanations, Duhem can hardly be faulted for trying to follow his opponents' usage as far as possible.

[7] This regards his Cartesian conception of God the guarantor of truth, which Duhem whisked off into an appendix and was therefore, perhaps, not even regarded by him as essential to his main line of thought.

[8] Harman, ch. 10.

[9] Van Fraassen's position presupposes some kind of semantic realism in terms of which he talks of the literal meaning of laws whose truth we can never know, and Cartwright's argument from her second criterion of truth by exact description for phenomenological laws to the falsity of theoretical laws is apparently based on a similar conception of truth. But Duhem leaves no room for any such distinction between semantic realism and epistemological antirealism.

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