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Dossier Pierre Duhem

Anti-Scepticism and Epistemic Humility in Pierre Duhem's Philosophy of Science

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Abstract:
Duhem's philosophy of science is difficult to classify according to more contemporary categories like instrumentalism and realism. On the one hand, he presents an account of scientific methodology which renders theories as mere instruments. On the other hand, he acknowledges that theories with particular theoretical virtues (e.g., unity, simplicity, novel predictions) offer a classification of experimental laws that "corresponds to real affinities among the things themselves." In this paper, we argue that Duhem's philosophy of science was motivated by an anti-sceptical tendency, according to which we can confidently assert that our theories reveal truths about nature while, at the same time, admitting that anti-scepticism should be moderated by epistemic humility. Understanding Duhem's epistemological position, which was unique amongst French philosophers of science in the beginning of the 20th century, requires a careful examination of his accounts of representation, explanation, and of their interrelation.

Keywords:
Pierre Duhem; representation; explanation; natural classification; holism; epistemic humility; realism

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Introduction

Duhem's philosophy of science has justifiably attracted a great deal of attention. His *The Aim and Structure of Physical Theory*, initially published in 1906, offers a comprehensive account of science, its method and its value, which is difficult to classify according to standard categories such as instrumentalism and realism. The key interpretative difficulty relates to the fact that Duhem himself appears to be ambivalent. On the one

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hand, he presents an account of the method of science which renders theories mere instruments for classification of experimental laws and for predictions (cf. 1908a, 333). On the other hand, he acknowledges that theories meeting various conditions (unity, simplicity, novel predictions) are in the process of offering natural classifications of experimental laws, where a classification is natural if it "corresponds to real affinities among the *things themselves* (*les choses elles-mêmes*)" (1906, 26). This ambivalence appears in his partly autobiographical essay "Physics of a Believer" (1905). There he says quite clearly that a theory "constitutes a kind of synoptic or schematic sketch suited to summarize and classify the laws of observation" (1905, 277) and that "physical theory through its successive advances tends to arrange experimental laws in an order more and more analogous to the transcendental order according to which the realities are classified" (1905, 297). Claims such as the above might be hard to reconcile and this had led many commentators to argue that Duhem's views are in some kind of tension.[3] Or that Duhem was trying to canvass a middle position between realism and instrumentalism. Or that he was a 'structural realist'.[4]

In this paper, we will argue for the unity of Duhem's thought. From his early papers dating from the early 1890s to the *magnum opus* of 1906, Duhem occupied a special epistemological position which can be best understood in light of the debates about science, its method and its value, that were taking place among French philosophers of science. Seen in this light, Duhem wanted to achieve two things at the same time. He wanted to show that physical theories have "value as knowledge outside their practical utility" (1908a, 319) *and* that this very assertion can only be justified if the scientist breaks with positivism and endorses a conception of rational judgement that goes beyond the strict confines of logic.[5] At the same time, he wanted to show that the cognitive value of physical theory is not tied to its offering (mechanical) explanations of the experimental laws. It is, nonetheless, tied to its being responsive, in some way, to the reality behind the phenomena it aims to represent.

A key point of this paper is that Duhem's philosophy of science was motivated by an anti-sceptical tendency, admitting at the same time that anti-scepticism should be moderated by epistemic humility: it should come to terms with the history of science and the fact that "it reminds [us] that the most attractive systems are only provisional representations, and not definitive explanations" (1906, 270).[4]

Understanding Duhem's philosophy of science requires a careful examination of his accounts of representation and explanation as well as of their relation. Duhem's account of science rests decisively on the claim that the explanatory parts and the representative parts of theories can be sharply distinguished. We shall argue that they cannot be. The repercussions of this failure for Duhem's account of science will then be explored.

Here is the roadmap. Section "Representation vs Explanation" will discuss Duhem's account of representation, focusing on his novel use of abstraction principles to introduce physical magnitudes and his account of explanation, focusing on his anti-Cartesianism. Then, in section "Disentangling the two Parts of the Theory", we will examine Duhem's two arguments for the existence of a sharp distinction between the representative and the explanatory parts of the theory and we will show, summoning historical evidence available to Duhem himself, that this distinction is shaky and problematic. Section "Re-assessing Duhem's holism" will then offer a new reading of Duhem's anti-atomism (*aka* holism). In section "Natural Classification", we will offer the main argument for the unity of Duhem's thought in light of his views on natural classifications. Finally, in section "Are Relations the Epistemic Limit?" we shall briefly discuss some motivations of Duhem's relationism.

[3] For a useful discussion see Needham (2011).

[4] Fábio Leite (2017) offers a nice summary of the various interpretative accounts of Duhem's philosophy of science. For an account of Duhem as canvassing a middle way between realism and instrumentalism, see Psillos (1999, 34-37).

[5] This point has been emphasized by Maiocchi (1990, 398).

[4] Our notion of "epistemic humility" must be distinguished from the notion discussed by Kidd in his (2011); and more generally, from its use in recent debates concerning whether Duhem was a virtue epistemologist. Kidd talks of "intellectual humility" as virtue and takes it to refer to some inherent limitation of "the scope of human mind", circumscribing "the proper acknowledgment of the cognitive capacities appropriate to human beings, and of the proper epistemic ambitions open to us". We take 'humility' to capture the epistemic modesty concerning the reach and extent of scientific knowledge, which is driven mostly from patterns of theory-change in the history of science.

Representation vs Explanation

'Representation' is a technical term for Duhem. It captures a specific method of constructing physical theories, which is based on abstraction. Roughly speaking, a theory is said to be representative if it is constructed in such a way that its basic concepts are abstracted out of experimental facts and its basic principles—which connect the basic concepts of the theory—symbolize a set of experimental laws. The very idea of 'representation' for Duhem is connected with symbolism: a representation is always symbolic.

To be more precise, Duhem took it that there are four fundamental operations in a physical theory which renders it a *representation* (of a set of experimental laws):

(1) the definition and measurement of physical magnitudes;
(2) the selection of hypotheses;
(3) the mathematical development of the theory;
(4) the comparison of the theory with experiment.

Let's take a closer look at the four steps.

Abstraction Principles

The first step consists in determining the physical concepts which will stand for basic physical properties, viz., the simple elements from which every other physical property can be derived. These properties are extracted from a set of experimental laws, corresponding to the range of phenomena that the theory aims to describe (1892a, 6). As such, the fundamental building blocks of the theory must be closely connected to observations or experiments. The physical concepts are then translated into mathematical symbols, whose adequacy depends only on the features that the empirical property exhibits and that the physicist wants to capture.

The key method Duhem introduces for the specification of the basic concepts is abstraction (1892a, 3-4). Here is his example. Warmth is an empirical property of bodies. Bodies can be as warm as others or more or less warm than others. These features, however, though "essential to the concept of *warmth*, do not permit the measurement of the object of this concept—that is, to regard it as a *magnitude*". And yet, the relation of being *as warm as*, which holds between actual physical bodies given in experience, has the properties of being reflexive, symmetric, and transitive. It is, in modern terminology, an equivalence relation. Duhem observes that a more rigorous physical concept (and a corresponding magnitude) can be introduced on the basis of this equivalence relation, viz., temperature. As he (1892a, 3) put it:

> We make two equal values of temperature correspond to two points that are as warm as each other. We make two unequal values of temperature correspond to two points that are not equally warm, and in such a manner that the higher value of temperature corresponds to the warmer point.

This move allows, among other things, the transition from a qualitative property to a quantitative one. Even though it makes sense to say that body A is as warm as, or warmer than, body B, it does not make sense to assert that the warmth of the body C is equal to the warmth of body A plus the warmth of body B. Not so for temperature, since this is a magnitude which is additive. In fact, the concept of temperature has excess content over the concept of warmth, since the concept of temperature involves an extra quantitative assumption, according to which to each point of a body can be assigned a definite value of temperature – an assumption that permits additivity.

The concept of temperature, then, is introduced on the basis of an *abstraction principle* over a set of physical bodies and an equivalence relation among them (*being as warm as*). Duhem is clear that this procedure is essentially generalizable. What was said about temperature "could be repeated—at least in its essentials—about all definitions of magnitude that we find at the beginning of any physical theory whatsoever" (1892a, 4). Note that this is a far-reaching approach. It shows that Duhem was appealing to what came to be known as abstraction principles in order to introduce new physical concepts and corresponding magnitudes.[6]

This definition of physical magnitudes by abstraction out of equivalence relations among the properties of empirical bodies makes possible the statement of mathematical-quantitative physical laws about the magnitudes thus defined. In particular, it makes possible the symbolic representation of empirical laws, even if these laws do not bear strictly speaking on the same empirical properties as the empirical laws did. For Duhem, abstraction is indispensable because the magnitudes to which the theoretical hypotheses of a physical theory apply must be mathematized, so that these hypotheses state mathematically precise relations (laws) among magnitudes.

Hence, abstraction makes the mathematisation of nature possible. As he put it in (1893b, 58),

> It is abstraction that furnishes the notions of number, line, surface, angle, mass, force, temperature and quantity of heat or electricity. It is abstraction, or philosophical analysis, that separates and makes precise the fundamental properties of these various notions and enunciates axioms and postulates.

But abstraction, when used within a physical theory, also implies that, as Duhem (1906, 128-131) explains, a physical magnitude need only possess the relevant mathematical properties (to be expressed by a number and to be additive) and to obey the relevant mathematical laws (commutativity and associativity). Hence, different symbols could be chosen for a given physical property, as long as the mathematical features of interest of the latter are captured by the symbol. Apart from this requirement, the definition of the physical magnitude is arbitrary, like a sign is arbitrarily chosen to represent the thing it signifies.

Hypotheses and Beyond

The second step in theory construction consists in relating these symbols to each other through hypotheses. Here again, there is absolutely no constraint whatsoever on the choice of hypotheses provided that, taken as a whole, they represent the totality of the experimental laws. From his early work *Some Reflections on the Subject of Physical Theories*, published in 1892, to *The Aim and the Structure of Physical Theory*, Duhem kept defining the 'ideal' method for choosing the hypotheses as "accepting no hypotheses except the symbolic translation, in mathematical language, of some of the experimental laws from the group (…) [the physicist] wished to represent" (1892a, 6, see also 1906, 190-191). Concerning the recourse to hypotheses, Duhem made clear that their formulation should be as little restricted from above by metaphysics as possible, the only logical constraint on their adoption being the principle of non-contradiction.

Enter the third step. From these hypotheses, the theoretical physicist proceeds via mathematical deduction to derive the (mathematical) consequences of the chosen hypotheses. This mathematical development of the theory is committed only to the rules of logic: the physical world is put in brackets at this stage of the process. Finally, however (and this is the fourth step), the consequences of the mathematical deduction will be translated back into claims about observational and testable predictions, allowing the physicist to submit his theory to the verdict of experimentation. As famously put by Duhem, the agreement with the experiment is the only criterion of truth for a theory. If the consequences of the deduction tally with the experimental laws, the theory will have fulfilled its aim: allowing the physicist to substitute the multiplicity of experimental laws with a few number of principles from which the laws can be reconstructed, i.e., to provide a compact representation of a vast set of experimental facts. Such a successful representation would offer a condensed symbolic representation of the laws, but also a classification of them:

> Between a set of experimental laws taken as experimentation has brought them to light and the same set of laws connected by a theory, there is the same difference as that between a mass of documents

6 Abstraction principles were characterized as such by Bertrand Russell in his (1903) and were used by him and earlier on by Frege (1884, § 63-67) to define the concept of cardinal number. It is significant that the main idea was employed by Duhem.

> heaped in confusion and the same documents carefully classified in a methodical collection. They are the same documents; they say exactly the same thing and in the same way. But in the first case, their disorder makes them useless, for one is never sure of recovering the document one needs at the moment one needs it; similarly, in the second case, the documents are made fruitful by a methodological grouping which places the desired document surely and without effort in the hands of the researcher (1893a, 36).

Indeed, *qua* representation, the goal of a theory is merely to recover the experimental laws, only *simplified* and *better ordered*. *Qua* representation, then, the physical theory is seen as an economy of thought.

Explanation

Famously, Duhem contrasts representation to explanation. Now, for Duhem explanation proceeds with positing unobservable entities and structures and consists in reducing the behaviour of observable entities (the empirical laws) to these invisible entities, their properties and their own laws of behaviour. Being not given in experience these entities (and the laws they are supposed to obey) are deemed, by default, metaphysical. Hence, explanation is taken to be characteristic of metaphysics, thereby falling outside the scope and bounds of science. As he put it, science "is not an explanation. It is a system of *mathematical propositions deduced from a small number of principles, which aim to represent as simply as completely and as exactly as possible a set of experimental laws*" (1906, 19, our emphasis). Given how Duhem defines an explanation as an attempt to "strip the reality of the appearances covering it like a veil, in order to see the bare reality itself" (1906, 7), the physicist aiming to explain "the appearances" has to accept that there is some distinct 'reality' behind them and that the task of science is to reveal it. Outside such a framework, as Duhem puts it, "the search for a physical explanation could not be conceived" (1906, 9). Thus, the success of an explanation can only be assessed on the prior adoption of a given metaphysics. To explain is therefore to step outside of physics, and to subordinate a physical theory to a metaphysics that alone can deliver the standards of evaluation of a successful explanation. For Duhem, not only does this way of proceeding misrepresent the aim and object of physical theories, but it also ruins the possibility for an autonomous physics.

Notably, he blames Descartes for having breached "the barrier between physics and metaphysics" (1893a, 44). The Cartesian project to reconstruct the whole edifice of knowledge on secure and indubitable principles implies that physics rests on hypotheses not obtained through scientific methods and as such not belonging to science, but to metaphysics. But his criticism of Descartes bears as much on his extreme hypothetico-deductivism as on his use of mechanical hypotheses. Instead of merely translating into mathematical symbols the physical concepts appearing in experimental laws, the Cartesian physicist adds constraints on the choice of properties and admits no simple property other than motion, size, shape. Duhem argues that the imposition of these conditions on the physical theory will result in an extraordinary complexity:

> The (…) inconvenience of such a method is that in restricting the number of elements that may be used in constructing the representation of a group of laws, physicists are left with no other resource than to complicate the combinations they make with these elements in order to respond to all of the demands of experimentation (1892a, 13).

Disentangling the two Parts of the Theory

Explanations should be banned from ideal physical theories, according to Duhem. In fact, he found energetics, *qua* a rival of atomism, to be as close to this ideal as possible. Far from looking for the "revelation of the true nature of matter", energetics was taken to operate by general principles (like the principle of conservation of energy) under which experimental laws are subsumed. These principles, as Duhem put it, were "pure postulates or arbitrary decrees of reason" (1913, 233).

But even a cursory look at the history of science suggests that many, if not most, of actual theories were far from meeting the above ideal standard. Actual theories were such that explanation and representation were intermingled. How can they be disentangled? And if they can be disentangled, what is the argument against theories that have an explanatory part?

Let us address these questions by noting that to motivate the distinction between two types (or two parts) of theory, Duhem (1906, 52) took a cue from Rankine's (1855) distinction between "abstractive" and "hypothetical theories". According to Rankine (1855, 209), the difference between the two kinds of theory stems from the first operation we described in section "Abstraction Principles", i.e., the definition of properties. An abstractive theory will consider as its fundamental properties only those that can be "perceived by the senses": properties are introduced in the physical theory only by means of an abstraction. A hypothetical theory, on the contrary, will accept properties "not apparent to the senses" through conjectures about the underlying nature of the perceived objects or phenomena. Thus, explanatory hypotheses will be introduced as soon as the relevant properties have been chosen, bearing consequences on all of the edifice afterwards. However, unlike Duhem, Rankine did not devalue hypothetical theories. He took them as an indispensable "preliminary step" for the reduction of "the expression of the phenomena to simplicity and order, before it is possible to make any progress in framing an abstractive theory" (1855, 213). For him, the contrast between the two types of theory was a contrast between two different modes of unification, of the "tendency (…) to combine all branches of physics into one system". One way is to rely on the axioms of mechanics as "the first principles of the laws of all phenomena—an object for the attainment of which an earnest wish was expressed by Newton", the other being to rely on "propositions comprehending as particular cases the laws of the particular classes of phenomena comprehended under the more extensive classes".

Hence, from Rankine's distinction it does not follow that an explanatory theory should be devalued. Is there any other reason that Duhem summons? For Duhem, explanatory hypotheses are actually "the germs that kill all mechanical theories", inasmuch as these hypotheses are not derived from any experimentation but from arbitrary *restrictions* added by the physicist. It might seem that the explanatory part of the theory is simply an extension of the representative part beyond the realm of the senses. Not so, for Duhem: the explanatory part is a *restriction* on theories. For him, as noted already, explanatory hypotheses consist of restrictions imposed on the construction of the theory. Drawing on his criticism of Descartes, he stresses that mechanical hypotheses add extra constraints on the choice of properties, since every physical property admitted within the theory must eventually be reduced to motion, size or shape. Hence, they limit the admissible properties to those which can have a mechanical grounding.

To see more clearly what Duhem has in mind, as well as to highlight the problem with his view, let us take a brief look at the difference between Newton and Descartes concerning the causes of gravity. Unlike Descartes and later Huygens, Newton avoided the mechanist demand to explain any phenomena "in terms of the arrangement and motion of minute, insensible particles of matter, each of which is characterized exclusively by certain fundamental and irreducible properties—shape, size, and impenetrability" (Nadler, 2000, 520), and did not try to account for the phenomena of attraction on the basis of mechanical principles:

> I use the word "attraction" here in a general sense for any endeavor whatever of bodies to approach one other, whether that endeavor occurs as a result of the action of the bodies either drawn toward one another or acting on one another by means of spirits emitted or whether it arises from the action of ether or of air or of any medium whatsoever – whether corporeal or incorporea l- in any way impelling toward one another the bodies floating therein. (Newton, 1999, 548-549)

Newton deliberately avoided hypotheses about the reduction of the phenomena of attraction to mechanisms and relied only on these principles which allow for the phenomena of attraction to be treated as a mathematically expressed natural law. It is arguable that Newton did this not by a mere desire to stay neutral with respect to these hypotheses, but because he was actually trying to "identify and so to isolate, precisely those presuppositions that, apart from their bearing on metaphysical questions, are also necessary presuppositions of the physics that he and his contemporaries practiced" (DiSalle, 2013, 449-450), that is, the presuppositions of the study of any dynamical system. This quest soon made him realize that the "common methodological ground between himself and his philosophical opponents" was constituted of those mathematical principles that allow the mathematical treatment of empirical concepts, like that of attraction, thus strengthening the distinction between mechanical and mathematical principles. Pushing this line further, if those mathematical principles were to be understood as the necessary and unavoidable presuppositions of any dynamical theory, it would also make sense to consider them as fundamental principles, which, as such, need not (and should not) be reduced to any more fundamental mechanism. This kind of stance, viz., taking the law of gravity as a fundamental principle, would have certainly made it easier to identify the forces holding planets in their orbits with the force attracting bodies towards the Earth. On the contrary, such an extension was not easily accessible to those who thought that gravity should be offered a mechanical explanation, simply because, as George Smith (2002, 141) has noted, "no hypothetical contact mechanism seems even imaginable to effect 'attractive' forces among particles of matter generally". Far from extending the theory, the quest for a mechanical account of gravity would restrict it since it could not ground the universality of the law of gravity. Huygens (1690, 160) himself, after reading Newton's *Principia*, admitted that his defense of the Cartesian theory of vortices had prevented him from extending the action of gravity to large distances:

> I had already thought, a long time ago, that the spherical shape of the Sun could have been produced in a similar way to the one which, according to me, produced the spherical shape of the Earth; but I never thought of extending the action of gravity to such large distances, from the Sun to the Planets, or from the Earth to the Moon; as my thoughts were obstructed by the vortices of Mr. Des Cartes, which in the past seemed to me so plausible and which I still had in my mind (M. G. translation).

This kind of case might bring home Duhem's point that explanatory hypotheses are not an *extension* of the theory to a realm inaccessible to sense, but chains by which a metaphysical system forced upon a theory, preventing its full growth. By the same token, however, this kind of case shows that representation and explanation need not be as far apart as Duhem thinks, if explanation is taken to be unification of diverse empirical laws under a theoretical scheme. Newton's law of gravity, as Newton himself admitted, is explanatory of a vast array of phenomena, even if the cause of gravity is not explained; or even if the very demand for an explanation of the cause of gravity is deflated.

It is fair to say that Duhem never conceded that explanation should be the aim of science. No matter how intricate the relation between representation and explanation can be, as the Newton case shows, Duhem thought of them as corresponding to distinct and separable parts of the theory.[5] In support of this view, he offers two arguments.

The *first* is an historical argument, expressed in the famous *continuity thesis* stated by Duhem in (1906, 32-33):

> 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

[5] In the original French text of *The Aim and the Structure*, Duhem translated Newton's famous words from the Optics, Query 31: "To tell us that every species of things is endowed with an occult specific quality by which it acts and produces manifest effects, is to tell us nothing: but to derive two or three general principles of motion from phenomena, and afterwards to tell us how the properties and actions of all corporeal things follow from those manifest principles, would be a very great step in philosophy, though the causes of those principles were not yet discovered". Duhem's own translation into French is this: "*Expliquer* chaque propriété des choses en les douant d'une qualité spécifique occulte par laquelle seraient engendrés et produits les effets qui se manifestent à nous, c'est ne rien *expliquer* du tout. Mais tirer des phénomènes deux ou trois principes généraux de mouvement, *expliquer* ensuite toutes les propriétés et les actions des corps au moyen de ces principes clairs, c'est vraiment, en Philosophie, un grand progrès, lors même que les causes de ces principes ne seraient pas découvertes" (2007, 81—emphasis added). Note that Newton's "to tell us" is translated (thrice) by Duhem into "expliquer" [to explain]. In the first two occurrences, 'to explain' refers to explanation by means of occult qualities, which Duhem says 'explains nothing'. But in the third instance, 'to explain' refers to the explanation of all properties by clear principles. This double use of 'explanation' seems to suggest that Duhem may well allow an *explanation* that does not rely on any hidden causes, but aims at the kind of unification Newton offers by his laws. However, in the subsequent paragraph he seems to equate this sense of 'explanation' with geometric representation (*représentation géométrique*). Thanks to an anonymous reader for bringing this subtle point to our attention.

> the inheritance of all the valuable possessions of the old theory, whereas the explanatory part falls out in order to give way to another explanation. Thus, by virtue of a continuous tradition, each theory passes on to the one that follows it a share of the natural classification it was able to construct, as in certain ancient games each runner handed on the lighted torch to the courier ahead of him, and this continuous tradition assures a perpetuity of life and progress for science. This continuity of tradition is not visible to the superficial observer due to the constant breaking-out of explanations which arise only to be quelled.

The metaphor of a relay in which the lighted torch is passed on from one runner to the next is very vivid. It highlights Duhem's belief in continuity; in the existence of a pattern of retention in the history of science. But the retention is limited to the representative parts of theories; the explanatory parts are supposed to have been abandoned and replaced by new explanations. For Duhem, the lighted torch of science is representation and not explanation.

His favorite example was the theory of light-refraction. In Descartes's own work, the representative part was entirely subsumed under one law: the Snell-Descartes law, which asserts the constancy of the ratio between the sine of the angle of incidence and the sine of the angle of refraction of a light ray. This law is still accepted and can nowadays be found in any optics textbook. However, this representation of the phenomena of refraction was accompanied by an explanation of this phenomenon, where light was analyzed as being caused by a "pressure engendered by the rapid motion of incandescent bodies within a "subtle" matter penetrating all bodies" (1906, 33). This explanation has a history of its own: it was replaced by the emissionist theory under Newton's influence, and was resuscitated by Young and Fresnel one century later. But according to Duhem it has never been genuinely related to the representative part of the theory of refraction. Duhem provides two considerations to support this idea of a mere juxtaposition of the two parts rather than a genuine relation between them: first, when trying to explain why light travels faster in denser than in rarer medium, Descartes appealed to a mechanical analogy with balls which is more suitable to the emissionist hypothesis than to the wave theory of light. Second, Descartes was convinced that the infinite speed of light was a necessary consequence of his explanation of light. As a result, Römer's experiment showing the finite speed of light led to the demise of Descartes's explanatory theory. Nonetheless, the Snell-Descartes law was never jeopardized by this experiment and has been retained through all the successors of the Cartesian theory of light.

An obvious worry with this kind of historical argument is that it might well be the case that the alleged distinction between the two parts of the theory is *ex post facto*: the representative parts are those that have been retained in theory change and the explanatory parts are those that have been abandoned. Is there an independent reason to draw this distinction for an arbitrary theory? Is there an argument why those *and only those* theoretical parts that have been abandoned are explanatory?

It is in order to address this kind of worry, that Duhem aims to make a case for the *predictive dispensability* of the explanatory parts. On his view, the only part of the theory that possesses empirical content that can lead to predictions is the representative part. This is supposed to be illustrated by the wave theory of light. Huygens, Duhem argues, despite being one of the most ardent defenders of the mechanist philosophy and being the one who actually unraveled the consequences of a wave theory, did not use mechanical hypotheses to extend Descartes's laws of refraction to the phenomena of double refraction. He simply extended the representative theory already available to a new range of phenomena. The only hypotheses on which his reasoning was grounded were "a comparison between the propagation of sound and the propagation of light, the experimental fact that one of the two refracted rays followed Descartes' law while the other did not obey it, a felicitous and bold hypothesis about the form of the surface of the optical wave in media of crystals" (1906, 35). Hence, on Duhem's view, the credit for Huygens's account of double refraction goes only to the representative part, whereas the wave hypothesis did not contribute at all to this predictive success.

Things, however, are more complicated. Duhem himself showed this when he described how Huygens used the Iceland Spar to study double refraction. Huygens observed, as Bartholin did before him, that two images of one and the same line could be produced by placing a Spar on a piece of paper. Moreover, one of these images would rotate if the Spar itself was being rotated. One of these images, that Huygens called "ordinary", obeys Descartes's law and stays fixed when the Spar is rotated. The rotating image, called "extraordinary", does not satisfy the law of refraction. Upon having explained the phenomena of reflection and refraction based on a wave hypothesis and the propagation of light through spherical waves, Huygens postulated that the two images were the results of two different kinds of wave propagation, corresponding respectively to the light propagation in the aether contained in the Spar and to the light propagation in the particles constituting the Spar itself. While the former waves are spherical, the latter are ellipsoidal, thus explaining the phenomena of double refraction. Based on these explanatory hypotheses which yielded the ellipsoidal model, Huygens thus constructed a geometrical model that succeeded in representing the phenomena of double refraction and thus extended Descartes's theory of refraction.

The point here that Huygens did *use* the wave hypothesis to construct his theory of double refraction. It was the wave hypothesis that made possible the ellipsoidal model, since as he (1690b, 73) put it: "upon having explained the refraction of transparent ordinary bodies, by means of the spherical emanations of light, (…), I went back to examining the nature of this crystal, about which I could not discover anything before" (M. G. translation). An ellipsoidal propagation as the one described would not have made sense outside the wave theory. Hence, that we may, *ex post facto*, deem a hypothesis 'explanatory' does not imply that, in the reasoning that led to this model, it did not play a role.

Duhem, indeed, grants that Huygens's theory "*represents* at the same time the laws of simple refraction, the object of Descartes' works, and the laws of double refraction" (1906, 35, our emphasis). But what does 'represent' mean here? There are only two ways to determine whether something is a representation according to Duhem: a part of a theory is said to be representative a) if it follows the four-step method of construction of a theory and aim only at summarizing and classifying experimental facts; and b) if it is retained over time. We noted already that the second way needs independent support and that Duhem's argument we have been examining, viz., that the explanatory part does not contribute to the predictive power of the theory, was meant to offer this independent support. Huygens's case shows that Huygens's theory was not representative in the sense of being constructed in the way suggested by Duhem's four-step method. It was nonetheless involved in extending the law of light-refraction to cover the phenomena of double refraction.

To sum up. The continuity thesis (viz., the retention of representative parts in theory-change), if it's not merely an *ex post facto*, and hence *ad hoc*, way to identify the representative parts of theories, has to be supported by an independent criterion for taking a principle to be representative as opposed to explanatory. This criterion is meant to be offered by the predictive impotence argument, viz., that explanatory principles do not contribute to the predictive success of the theory. But this alleged impotence cannot be substantiated by the very cases that Duhem considers. It turns out that the supposed sharp distinction between two parts of the theory (the explanatory and the representative) is either *ad hoc* or unsupported by historical evidence. To say the least, there is no cogent argument to the effect that the explanatory part is attached to the representative part "like a parasite" to a "fully formed organism" (1906, 32).

Re-assessing Duhem's Holism

It might be thought that Duhem cannot have it both ways. He cannot be a holist and at the same time accept that only the representative part of the theory gets any credit from the empirical successes of the theory. Wouldn't commitment to holism imply that the explanatory part (assuming that we can draw such a distinction) also gets some credit by getting some of the empirical support of the theory? In this section, we shall argue that appearances to the contrary, Duhem was not committed to a radical version of holism and that he used his anti-atomism as a weapon against the empirical support of the explanatory hypotheses of the theory.[7]

Duhem wanted to make the strong point that it is a "chimera" to try to isolate a(n) explanatory hypothesis and subject it to empirical test on its own (cf. 1906, 200). We call this view anti-atomism, since the emphasis is meant to be on showing that isolated hypotheses—that is, hypotheses which are not part

[7] At least part of Duhem's motivation for holism is his anti-inductivism. In his (1906, 190-194) he argues extensively against the claim that Newton's laws are inductive generalizations from experience. For Duhem laws are not justified "one by one", by observation and made general by induction. Rather, testing them "is a matter of comparing the corollaries of a whole group of hypotheses to a whole group of facts" (1906, 194).

of a theoretical system—do not have their own empirical content; hence, they cannot be tested atomistically, viz., independently of a theoretical system in which they feature.[8]

Suppose there is a dispute about a theoretical hypothesis H. Ideally, there must be some prediction "of an experimental fact" drawn from H. The experiment is then performed and if the "fact is not produced", the hypothesis H "will be irrevocably condemned" (1894, 82). Duhem shows that this account of testing is an illusion, since no hypothesis taken in isolation from a theoretical system implies *any* predictions. As he put it: "The prediction of the phenomenon whose nonproduction will cut off the debate does not derive from the disputed proposition taken in isolation but from the disputed proposition joined to this whole group of theories". Hence, when the prediction is not brought out "it is the whole theoretical scaffolding used by the physicists" which is "shown to be wanting" (1894, 82). But where does the error lie? The experiment cannot pinpoint the culprit among the parts of the whole theoretical system. Duhem's conclusion is that how the blame is distributed among these parts is not a matter of "logical necessity" (1894, 83).

All this is rather well-known. What is not typically perceived is that Duhem uses this kind of argument to show that explanatory hypotheses do not have empirical content of their own. Immediately after the logical argument, he offers an illustration by means of Newton's emission theory of light, which he took it to be a typical case of an explanatory theory. On this theory, light is formed of very small particles emitted with great velocities by light sources. These particles (the projectiles, as Duhem calls them) are subjected to distance-dependent attractive and repulsive forces and permeate all bodies. This set of "essential hypotheses", linked (and only linked) with many others entail that light travels faster in water than in the air. This prediction, noted by François Arago, was tested by Léon Foucault in a famous experiment. Duhem was quick to point out that the negative result of the experiment (viz., that light travels faster in air than in water) does not tell where the error in Newton's emission theory lies. Hence, not only has Newton's explanatory hypothesis no empirical content in isolation of a theoretical system, but given a conflict between the theoretical system and experience, Newton's hypothesis can be saved from refutation. Hence, because of anti-atomism, an isolated explanatory hypothesis is not genuinely testable.

What is more, Foucault's experiment is far from crucial. It does not prove the opposite theoretical hypothesis, viz., that light consists of waves. On this hypothesis, defended by Huygens, Young and Fresnel, light consists of waves which are propagated through an elastic luminiferous medium. This alternative explanatory theory yields the prediction that light travels faster in air than in water. One might have expected that we are faced here with a crucial experiment. It might be thought that we have two competing hypotheses H and H', H being that light consists of particles and H' being that light consists of waves, and a decisive experiment among them since H entails e and H' entails not-e. But this is the wrong way to think of the matter and anti-atomism brings out what's wrong with it. It is not two competing explanatory hypotheses that are being tested but two "theoretical groups or systems, each taken as a whole": Newton's optics and Huygens's optics (1894, 86). Given this, no experiment can decide between two explanatory hypotheses, viz., that light is a body and light is a vibration in a medium. These two explanatory hypotheses do not have their own empirical content.

Still, could it be that under *favourable* circumstances a theoretical group or system ends up being well supported by the evidence given that its rival is taken to be disconfirmed by the evidence? Holism is compatible with this scenario: one theoretical system might be more supported by the evidence than another. Though Duhem does allow that a theoretical system might be condemned by empirical evidence and abandoned, he was firm in claiming that a theoretical system T is supported by the evidence e *only if* we are certain that there are no other theoretical systems (hitherto unconceived) such that were they available, they would entail e. Hence, he couples his anti-atomism with a radical view of empirical support. Hence, assuming the empirical failure of the emission theory of light is not proof of the *truth* of the alternative wave theory of light because it does not have to be the case that light is either a body or that light is a wave. We will never be able "to affirm that no other hypothesis is imaginable" (1894, 87). And because of this we can "never be certain that we have exhausted all the imaginable hypotheses concerning a group of phenomena" (1894, 87).

This line of thought has become standard in arguments against a realist understanding of science: no theory T is confirmed by the evidence unless it is shown that no other theory exists or could be conceived

[8] It should be noted that Duhem was an anti-atomist too in that he opposed the atomic theory of matter. The two senses of anti-atomism should not be confused.

such that it is empirically equivalent to T. But it should be noted that Duhem was rather careful. The point he wanted to make is that no theory can be proved to be *true*. He noted this explicitly in his (1908b, 110):

> Grant that the hypotheses of Copernicus manage to save all the known phenomena; that these hypotheses may be true is a warranted conclusion, not that they are assuredly true. Justification of this last proposition would require that one prove that no other set of hypotheses could possibly be conjured up that would do as well at saving the phenomena. The latter proof has never been given.

But doesn't his anti-atomism extend to the representative part of the theory. The though here might well be that unless representative principles are sharply distinguished from explanatory ones by independent means, it seems that Duhem's anti-atomism does contribute to the undermining of the very distinction between two parts of the theory. Don't representative principles face the 'tribunal of experience' no less holistically than explanatory ones? Is there a way out for Duhem?

Duhem's way out was the restriction of his anti-atomism to the explanatory part of the theory. As we stressed, he used anti-atomism as an argument against the testability of explanatory hypotheses. But we saw already in section "Representation vs Explanation", that representative propositions were taken to be close to the experimental facts because they, in effect, replace these facts "with abstract and symbolic representations". This replacement is what Duhem calls "interpretation" (1894, 88). But interpretation does involve theories, since the very idea of abstract and symbolic representation implies the "transportation" of a fact into a theory. Though the experimental facts are interpreted by the theory that "physicists regard as established" (1894, 95), Duhem is adamant that the representative part of the theory is independent from the explanatory part, the reason being that it can be interpreted within alternative theoretical systems. Hence, an explanatory theory T may 'interpret' a certain set of empirical laws and facts according to its own conceptual resources, but *the very same laws* — being abstract and symbolic representations — can be "translated into the language" of an alternative theory T'. Once abstraction principles have allowed scientists to treat empirical properties as physical magnitudes, the symbols, *qua* symbols, admit of different interpretations. When transported into an explanatory framework, the symbols and the relations they stand to each other, are interpreted in light of the relevant theory. But precisely because they can be interpreted by different theories, theories can share representative parts (1894, 96). Hence it is possible to make "elements of the new theory correspond to elements of the old theory at certain points": a representative proposition, though interpret*ed* within a physical theory, is interpret*able* within alternative physical theories too. All this requires that, though interpretable by a given theory T, the representative part has empirical content of its own, independently of T. Hence, it acquires its content, as it were, atomistically. It is this atomistic empirical significance of the representative propositions that makes them interpretable in alternative theories. It is this that makes possible the establishment of a "correspondence" between the symbols that represent the results of experiments in theory T and the symbols that represent the same results in theory T' (cf. 1894, 96).

It seems plausible that Duhem drew the distinction between the two parts of the theory in terms of their distinct modes of testability: the explanatory hypotheses are tested non-atomistically, whereas the representative propositions are tested atomistically. But there is a drawback. This way to draw the distinction between the two parts of the theory very much depends on whether or not an explanatory hypothesis contributes to the empirical content of the theory. If it does, then it should certainly get some credit from the predictive and empirical success of the theory, even if Duhem is right in claiming that it is tested as part of a whole (that is, anti-atomistically). When we discussed Huygens's account of double refraction in the previous section, we noted that explanatory hypotheses contributed to predicting the distinct geometrical forms of the two ways, viz., the spherical and the ellipsoidal. That Huygens's wave hypothesis gets no credit from this requires the aforementioned controversial assumption that empirical support accrues to the explanatory part of the theory *only if* it is shown that there can be no other explanation of the same phenomena available. And though Duhem rightly noted that unless such as assumption is granted, no theory can be *proved* to be true, it's important to distinguish between proving the truth of a theory and allowing the evidence to confirm it. The problem with Duhem's radical view of empirical support is that it makes confirmation simply impossible: no evidence can bear on a theory.

Duhem used the vivid metaphor of an organism to talk about physics. Physics, he says, is not "a machine that let's itself be taken apart"; it is "an organism that must be taken as a whole" (1894, 85). This might be taken to imply that explanatory hypotheses, being part of the body of physics, can be subjected to test by subjecting to the test the whole body of physics. We have argued that for Duhem the organism metaphor, his holism, is meant precisely to show that what "is commonly thought", viz. that each of the explanatory hypotheses "may be taken in isolation, submitted to the control of experiment, and then, when varied and numerous proofs have established its validity, put in place in an almost definitive manner in the totality of science", is wrong: explanatory hypotheses lose their significance and "no longer represent anything" (1894, 88) if they are cut off from a system. Still, it does not follow that theoretical systems within which explanatory hypotheses are embedded can never be supported by empirical evidence.[9]

Natural Classification

Duhem had always been adamant that the aim of physical theory is classification. As he explains in the Introduction to his essay *The Electric Theories of J. Clerk Maxwell: A Historical and Critical Study*, for him theoretical physics "is only a schematic representation of reality. Using mathematical symbols, it classifies and directs the laws that experience has revealed; it condenses these laws into a small number of hypotheses; but the knowledge it gives us from the outside world is neither more penetrating nor of a different nature than the knowledge provided by experience" (1902a, 8).

But classification is always relative to a scheme of classification and there can be different and competing schemes. These schemes are "the free decree of our understanding" (1906, 286) and the only constraint in using these schemes is that they should not be mixed up. Using an example from biology, Duhem notes that a naturalist can classify some animals according to the structure of their nervous systems and some other group of animals according to the circulatory system. Similarly, the physicist can use the hypothesis that matter is continuous to classify some laws and the hypothesis that matter is atomic in another classification (1893b, 66). This no-mixing up condition, Duhem attributes to Poincaré. In fact, he finds it in Poincaré's *Électricité et Optique*, where Poincaré notes that "Two conflicting theories can, indeed—provided they do not mix and that are not seeking the bottom of things—be both useful instruments of research, and perhaps reading Maxwell would be less suggestive if it had not opened both new and divergent pathways" (1890, v; cited by Duhem 1902a, 8). Duhem himself is happy with this condition, since when it comes to the logical examination of theories, the only constraint he has put forward is logical consistency. Otherwise, a theorist is free to represent "different sets of laws, or even a single group of laws, by several irreconcilable theories" (1893b, 66). Logic imposes only one "obligation on physicists, and that is not to mix theory different procedures of classification" (1893b, 66.). To put the point bluntly, a theorist can use scheme A on Mondays, Wednesdays and Fridays and scheme B of Tuesdays, Thursdays and Saturdays. This way she avoids incoherence by avoiding to combine "a major premise" obtained by theory A with a "minor premise" obtained by theory B (1906, 294). The key rationale for this attitude is that the genuine content of a physical theory is taken to be the set of the empirical laws classified. As he says: "The systematic classification that theory gives [the empirical laws] does not add or take away anything concerning theory truth, their certainty or their objective scope" (1906, 285).

But this is half of the story. Duhem attributed to Poincaré and Édouard Le Roy the view that the no-mixing up principle is the only condition on scientific rationality and let himself occupy a different, more nuanced, position. He takes it that theoretical physics "deserves the name of science on the condition of being *rational*" (1902a, 8). But being rational, that is responding to reasons, is not confined to following strictly and exclusively the principle of non-contradiction and the rules of logic. A scientist may be free to choose any hypothesis she pleases "provided that these hypotheses are not redundant or contradictory",

[9] It is noteworthy that if Duhem was an anti-atomist, he was an anti-conventionalist too. He resisted the thought that just because hypotheses are not atomistically tested, they can be held on come what may. Taking distances from Poincaré and Le Roy, he argued against the view that some principles are elevated to conventions thereby acquiring a status of being "universally adopted" (1906, 212). For him, no principle (or hypotheses) is immune to revision; hence no principle can be held on come what may. As he put it: "The history of science shows us that very often the human mind has been led to overthrow such principles completely, though they have been regarded by common consent for centuries as inviolable axioms" (1906, 212). Duhem's opposition to conventionalism has been thoroughly discussed by Maiocchi (1990).

but a unified theory is preferable to "a junk heap of irreconcilable theories" (1906, 295). This "single physical theory which, from the smallest possible number of compatible hypotheses between them, would derive, by impeccable reasoning, all known experimental laws is obviously an ideal perfection which the human mind will never reach; but if it cannot reach this limit, it must constantly be directed" (1902a, 8). In fact, Duhem notes that representing by theories "unconnected with each other, or even by theories that contradict each other when they meet in a common domain" is a "transitory evil". Unity should be what physics should aim for.

It might seem that in Duhem's writings, two tendencies are always fighting against each other: on one hand, his own project of proceeding to the strict logical examination of physical theories—"we shall in this book offer a simple logical analysis of the method by which physical science makes progress" (1906, 3); and, on the other hand, his desire to support some theories over others. In the first case, Duhem insists on the value of the physical theory as an economy of thought, as condensing a multitude of facts and laws. In the second case, the emphasis is put on the theory as a *natural* classification, reflecting the true order of the world.

We shall argue that Duhem went beyond the positivist trend in French epistemology of science by bringing into it the thought that some kind of contact between theory and an underlying reality is necessary for taking a theoretical classification to be "a satisfactory representation" of experimental laws (1906, 298). His chief point, addressed to his fellow epistemologists, is that the ideal form of a scientific theory is achieved when a theory is a "natural classification" of experimental laws and that looking for this ideal form, even though it is a limiting condition, is reasonable and warranted (by the history of science). Its justification, however, exceeds the confines of the positivist method they (and he himself) were prone to follow. All this was thrown into sharp relief after Abel Rey published a sympathetic but critical essay of Duhem's views in 1904 and Duhem replied in 1905. But it was already there in Duhem's very early writings on the epistemology of science. Let us go into this matter in some more detail.

Already in 1893, Duhem assumed the idea of "the best classification" of experimental laws as this classification which would follow from a "detailed metaphysical knowledge of the essence of material things" (1893a, 37). This is because this kind of classification would map the order (viz., relations) there is (are) among things in the world, where this order would "result from their nature itself". He's careful to add that even if this knowledge were available, the physicist would still have the right to adopt another theory, "to connect physical laws in a different order, to accept another mode of representation of physical phenomena" (1893a, 37). But he adds that this attitude, though fully consistent with logic, would be "unreasonable" since "in every order of things we should choose what excels". The very assumption he started with is "purely ideal". But its conceivability is used by Duhem to show that the sceptics—those who deny the very "principles on which experimental science logically depends" (1893a, 38)—can be blocked only if we go beyond the method of physics and look for its justification. He meaningfully dissociated himself from positivism insofar as the latter asserts that "there is no logical method other than the method of positive sciences". Hence, there is more to justification than the method of science (understood as being constrained only by the principle of non-contradiction).

In another piece he published in 1893, he introduced the idea of 'natural classification' in connection with a perfect theory. Here he noted that "considerations of pure logic are not the only ones that reasonably direct out judgements (1893b, 67). Take the following rule:

> In physical theory, we must avoid logical incoherence BECAUSE IT INJURES THE PERFECTION OF SCIENCE (1893b, 67).

This is not a principle of logic. Yet Duhem thinks it's reasonable (legitimate). Perfection is a matter of degree, but ideally a perfect theory (or the "true theory", as he put it) would be "the complete and adequate metaphysical explanation of material things" (1893b, 68). The perfect theory would classify experimental laws in a natural way:

> an order which would be the very expression of the metaphysical relations that the essences that cause the laws have among themselves. [A perfect theory] would give us, in the true sense of the world, a natural classification of laws (ibid.)

Although Duhem talks of metaphysical relations between essences, what he really refers to are relations among unobservable entities—the minute constituents of material objects. Recall that for him the atomic hypothesis (as well as any other hypothesis which refers to unobservable entities) was a "metaphysical" hypothesis. A perfect theory would pertain to the true theory, and a "natural classification" is the one issued by a *true* theory. Avoiding contradictions and unifying the empirical laws into a single system of hypotheses is for Duhem the road to perfection. In effect, Duhem argues that if science aims at a natural classification, then unification is the most natural thing to look for. A natural classification cannot possibly be "an incoherent collection of incompatible theories" (cf. 1893b, 67)—even though each and every theory may save some phenomena. Unification is then seen as a way to remove inconsistencies and to approach what Duhem calls the "perfect theory".

Note that a natural classification is still a *classification* and not an explanation. What makes it natural is that the scheme for the classification, far from being arbitrary, is the one that nature itself uses, so to speak, to classify "the relations" that the causes of the empirical laws have among themselves. This, again, is an ideal form. Achieving it "infinitely surpasses the scope of human mind". But this does not mean that it does not exist; that is, there are true relations among the "essences" whose manifestation are the relations among the phenomena (the empirical laws). Given this ideal theory, it makes sense to aim to remove the contradictions among existing physical theories since the relations that there are among the causes of the phenomena are "neither indeterminate nor contradictory" (1893b, 68).

This is the justification for the *Unification Principle* (*UP*):

> Physical theory has to try to represent the whole group of natural laws by a single system all of whose parts are logically compatible with one another (1906, 293).

This principle was for him perfectly reasonable, though it could be denied without contradiction. So the key point we want to make is that the very idea of natural classification is part and parcel of a broader conception of reasonableness that Duhem endorsed in order to distinguish his view from the positivist ones in vogue in France in his time.

As noted already, this reaction to positivism was thrown into sharp relief in his exchange with Rey. He actually compared Duhem to most of his contemporaries (notably, Rankine, Helmholtz, Dubois-Reymond, Ostwald, Poincaré and Milhaud). They take it to be the case that science explains nothing and that looking for causes is a venture into metaphysics. For them, Rey (1904, 703, M. G. translation) says, "Sciences merely record relations among phenomena, connections that are convenient to achieve an exact description of these relations, a description that allows to some extent to predict". For Rey, Duhem stresses the indispensability of theoretical physics and claims that "purely experimental physics is a chimera" (1904, 704). Still, theories are arbitrary; they are "formal"; "They are a set of relations between numerical values, between quantities; They do not at all worry about the real content which enters relations, the objective properties evaluated by these quantities" (1904, 718). The comparison with reality is done at the end, when the theory is tested empirically. "But at the end, this game gains meaning thanks to a set of measures, that allow to detect reality; our formula must then give us results that coincide as fully as possible with this real detection" (1904, 722).

The issue Rey concentrated on was the value and objectivity of theory, if all there is to it is a scheme of classification. He credited Duhem with showing that empiricism (which was based on the claim that science is "a simple summary of experimental observations") is a fiction. But he took him too to distance himself from the claim that science is an arbitrary conventional classification. The theory "has a relation that is certain with reality, i.e., with the experimental records – the fact that the experiment must eventually intervene to confirm it or refute it proves it. What is arbitrary is everything that at first sight will allow us to make the order of our thoughts correspond to the real order. What is not arbitrary anymore is the correspondence itself" (1904, 728). In fact, according to Rey, Duhem argued against the "neo-sceptics" that it's not the case that every theoretical path possibly taken is fruitful. Instead, "there will be a theoretical development, which, better than any other, will correspond to the order of the phenomena which we wish to describe. There will thus be a set of theories which will impose themselves at least in general lines, to the exclusion of any other. It will constitute theoretical physics; This will be determined and one, not arbitrary and multiple" (1907, 133).

In the end, Rey called Duhem's view the "physics of a believer" (1904, 744). This was a charge that Duhem tried to dispel in his reply. But the key feature of his reply was his insistence on the claim that natural classification is the aim of science. He noted that the natural classification of experimental laws is a "limiting form" that the theory tends to achieve, through "its successive advancements" (1906, 297). He insisted that if a scientist is not an "intransigent positivist", he or she will come to see that "physical theory advances gradually toward its limiting form" (ibid.). Now, one point that Rey insisted on was that Duhem's resistance to neo-scepticism will come to nothing if the history of science showed that attempts to unify the theoretical image of the world had been a failure. Duhem (1906, 295) responded to this criticism by acknowledging it and by saying that, ultimately, the issue at stake is empirical:

> it is up to the history of science […] to tell us whether men, ever since physics took on a scientific form, have exhausted themselves in vain efforts to unite into a coordinated system the innumerable laws discovered by experimenters; or else, on the other hand, whether these efforts through slow and continuous progress have contributed to fusing together pieces of theory, which were isolated at first, in order to produce an increasingly unified and ampler theory.

Duhem's verdict was that the history of science has tilted the balance towards unity: "diversity fusing into a constantly more comprehensive and more perfect unity, that is the great fact summarizing the whole history of physical doctrines" (1906, 296).

What's also important to stress is that there is a contingent mark for a classification being natural, viz., the ability of theory to yield novel predictions; that is, the ability of theories to anticipate experiment, establishing novel predictions like prophets would reveal the future:

> The highest test, therefore, of our holding a classification as a natural one is to ask it to indicate in advance things which the future alone will reveal. And when the experiment is made and confirms the predictions obtained from our theory, we feel strengthened in our conviction that the relations established by our reason among abstract notions truly correspond to relations among things (1906, 28).

Still, the very idea that a theory is (or tends to be) a natural classification cannot be justified by the narrow positivist method that Duhem himself canvassed. All the more so for his anti-scepticism. Far from yielding to scepticism Duhem relied on a broader conception of justification which, we might say, relies on explanatory considerations: it is truth that explains perfection and it is perfection that explains why unification should be aimed at, or at least why it is reasonable to strive for it. The fact that *natural* classification will always remain an unjustifiable claim on the positivist method does not make it unjustifiable:

> Physical theory confers on us a certain knowledge of the external world which is irreducible to merely empirical knowledge; this knowledge comes neither from experiment nor from the mathematical procedures employed by the theory, so that the merely logical dissection of theory cannot discover the fissure through which this knowledge is introduced into the structure of physics; through an avenue whose reality the physicist cannot deny, any more that he can describe its course, this knowledge derives from a truth other than the truth apt to be possessed by our instruments; the order in which theory arranges the results of observation does not find its adequate and complete justification in its practical or aesthetic characteristics; we surmise, in addition, that it is or tends to be a natural classification; through an analogy whose nature escapes the confines of physics but whose existence is imposed as certain on the mind of the physicist, we surmise that it corresponds to a certain supremely eminent order (1906, 334-335).

Are Relations the Epistemic Limit?

A natural question at this point is this: why does Duhem insist that knowledge can extent only up to relations among "hidden realities whose essence cannot be grasped" (1906, 297). The answer to this lies, by and large, with Duhem's account of representation and the role of hypotheses in science.

At one point Duhem notes that "an intelligence that see essences" would classify the laws according to the "natural order" (1893b, 68). But we humans do not see "essences". And that's the problem for Duhem. If we (have to) rely only on representations of "essences", then we can never have knowledge of them. Despite his criticism of empiricism, Duhem was wedded to the view that knowledge of entities requires that they are given to us in experience. Not so for knowledge of relations, though. Let us see why.

We noted already that Duhem's talk of essences is meant to capture the unobservable causes or constituents of the phenomena. In his (1902b, 117), he put the point thus: "Contemporary physics is not metaphysics. It does not propose to penetrate behind our perceptions and come to know the essence and intimate nature of the objects of these perceptions". The "essence and intimate nature" of perceived object were the particles posited by theories as their micro-structure, e.g., "viewing the rapid movement of particles as constituting the essence of heat (1902b, 39). The only possible access to them is via hypotheses, but hypotheses are beyond the limits of experience. Hypotheses might well be indispensable in doing science but they are never a means to empirical knowledge. As he put it in an early piece: "let us never trust hypotheses for an instant, and in particular let us never attribute a body and a reality to the abstractions that the weakness of our nature imposes on us" (1892b, 177).

How then can *relations* be knowable? For a start relations are captured by mathematical equations, which are constructed in such a way as to represent the formal properties of the empirical entities under investigation. For another, because of this formal character, resemblance is not required. It is worth repeating that for Duhem, theoretical physics starts with empirical objects and aims to "represent" their properties. But "in order to represent these properties, theoretical physics defines certain algebraic and geometric magnitudes and then establishes relations between these magnitudes which symbolize physical laws to which the system is subjected" (1892c, 39).These relations are among magnitudes which bear no resemblance to the actual physical properties; they are symbolic and abstract representations of these properties and as Duhem notes, stand in "no relation to [the] nature" of the properties they represent: "But we can put this non-quantitative property into correspondence with an algebraic magnitude which, without standing in any relation to its nature, will be a representation of it" (1892c, 47).

Representation, then, cannot cut through relations. But of this representation something more can be said, if it meets the requirements noted above (viz., unity, simplicity and novel predictions): that it is (tends to be) natural. That the mathematical relations among the physical magnitudes express real relations among "hidden realities", of which "the essence"—what they are intrinsically so to speak—cannot be known. This kind of 'relationist' approach to knowledge is not far from the one that Poincaré developed at roughly the same time in an attempt to defend the objectivity and value of science. In fact, Duhem himself spoke approvingly of Poincaré's attitude when he wrote:

> The logical analysis that he had made with a pitiless rigor ineluctably led M. Henri Poincaré to the following fully pragmatic conclusion: theoretical physics is a mere collection of recipes. Against this proposition, he felt a sort of revolt, and he loudly proclaimed that a physical theory gives us something else than the mere knowledge of the facts, that it makes us discover the real relations among things (2007, 446, M. G. translation).

One of us has discussed Poincaré's relationism in detail elsewhere (Psillos, 2014). The relevant point here is that for Duhem too relationism is the limit of objective knowledge and at the same time his resting point against scepticism. As noted already, a key argument for this relationist approach to theoretical knowledge comes from the pattern of retention in theory-change in science. But it should be added that in making a case for theoretical knowledge of relations Duhem had to rely on explanatory considerations of the very sort that he thought were illegitimate as part of science.

Far from being an instrumentalist, Duhem took it as fully legitimate for a scientist to accept that science does offer some substantial theoretical knowledge of the world. A scientist who would stick to a strict positivist account of rational judgement in science would

> at once recognize that all his most powerful and deepest aspirations have been disappointed by the despairing results of his analysis. [For he] cannot make up his mind to see in physical theory merely a set of practical procedures and a rack filled with tools…. [H]e cannot believe that it merely classifies information accumulated by empirical science without transforming in any way the nature of these facts or without impressing on them a character which experiment alone would not have engraved on it. If there were in physical theory only what his own criticism made him discover in it, he would stop devoting his time and efforts to a work of such a meagre importance (1906, 334).

And he immediately added: "The study of the method of physics is powerless to disclose to the physicist the reason leading him to construct a physical theory".

Conclusions

Duhem's philosophy of science could be described as (increasingly) anti-instrumentalist. Yet, his anti-instrumentalism did not amount to endorsement of realism. If we were to identify the realist view of science with the atomic theory of matter and with the mechanistic view of the world, then Duhem was clearly not a realist, since he resisted both of them till the very end. But, to his credit, Duhem distinguished emphatically between two questions: "Does physical theory have the value of knowledge or not?" and "Should physical theory be mechanistic or not?" (1906, 320). He answered negatively the second but positively the first. And it is the first question that is deeply philosophical. Duhem's positive answer was meant to distinguish his views from what he took them to be purely positivist accounts of scientific method and of the rationality of science. The proper appraisal of the epistemic credentials of scientific theories requires adopting substantive principles such as the Principle of Simplicity, the Unification Principle and the Principle of Novel Predictions, which, though not forced on scientists by the scientific method, strictly understood, are reasonable and are required for taking science to offer some knowledge of the world. But this knowledge has a limit: it can only reach up to the relations there are behind the 'hidden essences' of the observable entities and the laws they obey. This limit (which captures what we have called 'epistemic humility') is licensed by the pattern of retention in theory change, as exemplified in the history of science.

Duhem's view, then, is not anti-realist either. He readily admitted that there is a natural order in the world which can be fathomed by theories which possess the marks of natural classification, viz., simplicity, unity and novelty. It's just that justifiably endorsing this kind of anti-sceptical view requires a broader conception of justification, which takes it to be the case that there is more to rational judgement than experiment and logic.

Accordingly, Duhem occupied a rather unique philosophical position which can be characterized by a combination of anti-scepticism about scientific knowledge with epistemic humility concerning its extent.

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