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BOOK SYMPOSIUM

New perspectives on Pierre Duhem's *The aim and structure of physical theory*

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The relation between history and philosophy in Duhem

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In the opening pages of *The Aim and Structure of Physical Theory*, Pierre Duhem claimed to provide "a simple logical analysis of the method by which physical theory makes progress". Yet, his book contains a large amount of history. One finds numerous examples taken from the history of physics as well as a general interpretation of progress as a continuous process. History also enables us to go beyond the limits imposed on logical analysis. This association of history and logic is perhaps what is most astonishing for the modern reader. We are used to separate the historical study of science from the logical analysis of its language. These

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methods have given rise to two philosophical traditions: analytical philosophy of science and historical epistemology.

One may distinguish four major readings of *The Aim and Structure*. I thereby qualify not individual interpretations but how the text was perceived at certain times and in certain places. My aim here is to question these readings, keeping in mind the issue of history. One should first mention the reception of his work at the turn of the nineteenth century. The context was that of the institutionalisation of philosophy of science, and early readers, I believe, sought in Duhem elements for constructing the field. The second major reading was that of the logical empiricists. We know that Duhem's conceptions, along with those of Poincaré, attracted the attention of members of the Vienna Circle. They noted the proximity of Duhem's views to those of Mach. What interested them was Duhem's logical analysis of theory structure. They of course downplayed the importance of history. What is intriguing in Duhem's case is that after having inspired logical empiricists, his ideas were taken up by their critics. Quine's allusions to Duhem are brief but significant. And the Duhem–Quine thesis was largely adopted in the post-positivist movement.

This alternate reading of Duhem eventually stimulated an interest in his work for its own sake. Undeniably, in the 1980s, a careful examination of the different aspects of his research began: his physics, his philosophical theses and his historical studies. This is Duhem in context. Since then post-positivism has given way to new methods: Crombie has argued for styles of thinking, and Hacking has criticised the idealism of Kuhn and others. The issue today is how to assess the accumulated results of Duhem studies and to go beyond. Is a new reading of Duhem possible?

The explicit role of history in Duhem

A historical approach to philosophy of science can be pursued in many ways, and indeed has given rise to a variety of styles of research. One direction consists in grounding philosophy of science on the history of science. Duhem's work offers in this respect a noteworthy example. In *The Aim and Structure,* "logical analysis" was complemented by recourse to historical study, both entering into a detailed and complex view of the nature and goal of theoretical physics. Moreover, the numerous volumes Duhem devoted to the evolution of science from Antiquity to present bear witness to this concern. Duhem drew on the pioneering studies of Paul Tannery, who had broken away from Comte's superficial history. He brought out their philosophical implications. This endeavour in turn caught the eye of his contemporaries who were intent on establishing philosophy of science as an academic discipline. Such was the case of Rey (1904, 1907), Milhaud (1898) and Le Roy (1899–1900). Thus, emerged a historical philosophy of science strongly represented in France, Meyerson and Brunschvicg providing further examples. I hold that what will come to be called historical epistemology,[1] as represented variously by Bachelard, Canguilhem, Foucault and Dagognet, has its origins here.

[1] The expression "historical epistemology" was introduced by Dominique Lecourt in 1969 with reference to Bachelard, but he attributes the paternity to his supervisor Canguilhem; cf. Lecourt (2008, 51).

Post-positivists were later to call on this tradition in their effort to reassert the importance of history. For example, Kuhn drew on Duhem's history of cosmology in his study of the Copernican revolution, and he pointed to Quine's break with logical empiricism, which was inspired by Duhem. Kuhn (1977, 108) came to describe Duhem's role in these terms:

> Duhem's search for the sources of modern science disclosed a tradition of medieval thought which […] could not be denied an essential role in the transformation of physical theory that occurred in the 17th century […]. More than any other, that challenge has shaped the modern historiography of science.

Kuhn then took up this challenge after others, and one can follow the evolution of his thought from a moderate view of scientific change to a radical view, in reaction to Duhem's continuism. It is interesting to note that Kuhn also mentions Meyerson and Brunschvicg, precisely the type of historical philosophy of science Duhem inspired. Other post-positivists called on Duhem's arguments and historical studies, such as Feyerabend, Lakatos and Laudan. However, these authors were pursuing their agenda, and it is not obvious that they provide us with a complete picture.

Let us take a closer look at the function of history in *The Aim and Structure*. Duhem's reasoning is frequently illustrated by examples taken from past science; each major claim is supported by historical arguments: representation, natural classification, analogy, etc. Furthermore, two crucial sections of the book are explicitly devoted to history that on the representative theories and the final pages (cf. Duhem 1906, part 1, Chap. 3; part 2, Chap. 7, § 6). The following passage summarises nicely Duhem's attitude (1906, 408–409; trans. 268–269):

> The legitimate, sure, and fruitful method of preparing a student to receive a physical hypothesis is the historical method. To retrace the transformations through which the empirical matter accrued while the theoretical form was first sketched; to describe the long collaboration by means of which common sense and deductive logic analyzed this matter and modelled that form until one was exactly adapted to the other; that is the best way, surely even the only way, to give to those studying physics a correct and clear view of the very complex and living organization of this science.

One should not forget the lengthy presentation Duhem gives of the genesis of Newton's principle of universal gravitation. The purpose of this passage is clearly to show that this principle did not arise from a mere generalisation of Kepler's laws nor from an inductive procedure alone. The historical presentation concludes with the idea of a continuous development, which characterises Duhem's historical view (cf. 1906, 384; trans. 252). Historical continuism replaces inductivism.

Duhem was not a professional philosopher; his intention was not aimed at establishing philosophy of science within the university curriculum. He was developing a general thermodynamic or energetics. And history serves as a justification of his scientific research programme (cf. Duhem 1913, 116; trans. 339). His programme was superseded by Relativity theory and Quantum mechanics. What remains is to some extent retrieved by the logical empiricists, that is axiomatics and the unity of physics. However, Duhem's recourse to history goes beyond the explicit role he assigns to it.

Duhem's definition of theory and its historical presuppositions

It is time we turn to Duhem's (1906, 24; trans. 19) well-known definition of theory:

> A physical theory 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.

A scientific theory is no longer conceived as the explanation of deep causes, but as an abstract representation of laws. One may perceive here an intuition of the standard view of theories, which the logical empiricists will develop further: a theory is an axiomatic system; a set of deductively linked propositions, arranged into axioms and theorems, its empirical interpretation being provided by certain operations such as measurements. Duhem goes on to explain that a theory consists in a symbolic construction characterised by four operations: the definition of concepts, the choice of hypotheses, the mathematical development and the comparison with experiment. This series of operations is what Duhem designates by *structure*, and the second part of his book treats of each one in turn.

Let us now apply the results of history of philosophy of science. In developing his definition Duhem was drawing on Kirchhoff. The latter wrote a series of important volumes on mathematical physics. The first, in 1876, is devoted to mechanics and begins as follows:

> The point of departure of the presentation I have chosen differs from that generally adopted. It is usual to define mechanics as the science of forces, and forces as the causes that produce motion or tend to produce them […]. But this presentation is vitiated by the obscurity inherent in the concepts of cause and tendency" (1876, v).

Kirchhoff obviously has in mind here the traditional definition given by Lagrange. He then goes on to expound his own presentation: "For this reason I assign to mechanics the task of describing motions that occur in nature and of describing them indeed completely [*vollständig*] and in the simplest manner possible [*einfachste*]" (ibid.).

The description provided by mechanics then must be complete and the simplest possible. Let us pay attention to the exact formulation of this passage, which Duhem read in the light of his own concerns: simplicity alone admits of degrees; completeness is taken absolutely. If simplicity admits of degrees, it is because it refers to the possibility of scientific progress. Later in his book, Kirchhoff gives a traditional example: the replacement of Kepler's laws by Newton's principle of gravitation. The progress accomplished in this case implies the use of more complex mathematics, but allows for a much more synthetic account. What we have here is a phenomenalist perspective: physics should no longer search for the causes nor be given over to the explanation of things. The concept of force can in a sense be eliminated. It only serves to simplify the mathematical expressions. The position just outlined has been described variously as descriptivism, positivism or nominalism.

One may make several remarks on this presentation. Does the requirement of completeness not also admit of any degrees? Duhem will sweep this restriction away. Does scientific description not imply other conditions? In truth, Kirchhoff is pretty much obliged to admit it: indeed, he mentions accuracy. But he does *not* include this concept among the explicit conditions of his definition of mechanics. This is another obvious omission of his presentation. What we have here is an attempt at a new definition of mechanics. But the enterprise is not brought to completion. Duhem will introduce significant changes. He delves deeper into the consequences of the descriptive task assigned to mechanics, and now extended to science in general. But let us notice that as axiomatisation no longer means explanation, the descriptive presentation of physical theories must receive justification from elsewhere. Such are the requirements of simplicity, completeness and accuracy, which Duhem emphasises.

We are also in a position to explain the origins of the other major elements of Duhem's conception. The idea of theory as representation, rather than explanation, goes back to Ampère (cf. Duhem 1906, 72; trans. 51). What is original in Duhem's definition is that we now have a two-tier model. The physicist does not represent the phenomena but the experimental laws. As for the idea of symbolic construction, Duhem draws on Helmholtz. The elaboration of Duhem's conception throws further light on his intention. His definition of theory as representation goes back to 1892, a time when he was seeking to develop an empiricist conception of thermodynamics. The difficulties he encountered led him to formulate his holist thesis in 1894. His works in history of science then brought him to reject traditional interpretations and to recognise the importance of mediaeval science. He came to develop historical continuism. In contrast with his earlier account of theory in terms of nature and goal (*but*), Duhem speaks now in terms of structure and aim (*objet*).[2] The formulation takes into account his rejection of empiricist interpretations of thermodynamics and inductivist methodology. It is more abstract and will exert considerable influence.

The criteria of choice in historical perspective

Let us focus on the three terms that occur in Duhem's definition of theory noted above: simplicity, completeness and exactness—for the last two terms an English-speaking physicist would rather use *comprehensiveness* and *accuracy*. One can understand why he brings them up: he has rejected the Newtonian method of induction as well as the idea of crucial experiments; in summary, he has rejected the traditional schemes of justification. Hypotheses are now freely chosen, which does not mean that they are arbitrary: the theorist must motivate his choice. Hence, he appeals to rational criteria. Let us first examine how they relate to his definition of

[2] The French term *objet* designates here that toward which the theory tends. It is less concrete in its connotations than the term *but*. One must take into account Duhem's distinction between the aim of individual theories and the *tendency* of physical theories.

theory. Duhem (1906, 25–26; trans. 20) makes the following comments with respect to the concept of exactness or accuracy:

> The various consequences […] drawn from the hypotheses may be translated into as many judgements bearing on the physical properties of the bodies […]. These judgements are compared with the experimental laws which the theory is intended to represent. If they agree with these laws to the degree of approximation corresponding to the measuring procedures employed, the theory has attained its goal.

Accuracy in physics then is in agreement within the accepted degree of approximation. Duhem is led to distinguish between truth in ordinary contexts and truth in physics. He goes as far as to advocate dispensing with truth altogether. In other words, the scientist must take into account the exact nature of the procedure of verification or confirmation. Accuracy, as indeed other criteria, must be related to the operations constituting scientific activity.

Likewise, simplicity is relativised in accordance with the definition of theory. In the second part of his book, Duhem (1906, 188–189; trans. 127) explains what he means by primary qualities and in so doing he calls on history:

> The physicist who seeks to make his theories autonomous and independent of any philosophical system attributes an entirely relative sense to the words 'simple quality' or 'primary property'; they designate for him simply a property that it has been impossible for him to resolve into other qualities.[3]

Simplicity, completeness and exactness explicitly enter into the definition of theory. Reading further, one encounters the requirements of consistency and fruitfulness. It is interesting to note that we already find in Duhem the standard list of criteria of theory choice that Kuhn (1977) will bring up in his later philosophy. Of course there are some differences, but the fact that the question continues to preoccupy philosophers at the end of the twentieth century is worth noting.

Conclusion

Calling on different readings of Duhem with respect to the role of history, we have been led to put into perspective several constitutive concepts of philosophy of science: structure, representation and criteria of choice. Duhem's definition of theory prefigures the standard view given by logical empiricists; it was a stage in a long and complex process, whose roots go well back into the nineteenth century. Traditional views of knowledge were difficult to overturn, and several advances had to be made: a precise understanding of the different components of scientific theories and the concrete operations underlying them. Science has moved further away from common knowledge; it no longer provides explanations in the usual sense. Mathematical organisation and experimental research have to be understood in their own terms. Definitions and hypotheses are interlinked. The consistency of

[3] It is interesting that Duhem calls on Ampère in this context and his recognition that intensity of electrical current is an irreducible property.

theoretical systems requires specific formal techniques of evaluation. And several epistemic values come into play.

The hypothetico-deductive presentation or axiomatics, on which Duhem laid such stress, appears today to be giving way to modelling and simulation. These techniques certainly express a prominent aspect of current scientific rationality, which is increasingly aided by computers. Axiomatics has come to be seen as one among different procedures, and this shift has helped to direct attention to the historical context of emergence and the trajectory of styles of reasoning. One may also mention another change of focus in philosophy of science from experimental testing of theories to the competition between paradigms or perhaps even negotiation among different styles of reasoning. Philosophy of science has moved away from Duhem in these respects. But if we are to make progress—and in the absence of a method analogous to experimental testing in the sense of physics—we should keep in mind—better still—we should reflect upon the elaboration of our evolving conception of philosophy of science. And Duhem is a key author in this history.

Duhem's moderate realism

Paul Needham

Charles Gillispie is quoted on the cover of the 1991 reprint of the English translation of Duhem (1906) saying: "The central proposition of this famous book is that physical theories are conventions serving to economize scientific thought rather than descriptions or explanations of the way the world is made". This short statement encapsulates a familiar interpretation of Duhem's philosophical position as anti-realist, which I want to challenge. My point is not that he was an extreme realist who advocated a correspondence theory of truth, advanced the miracle argument or maintained that there is some underlying reality with certain features beyond the scope of experimental assessment which ultimately explain scientific theories. Rather, I shall maintain that the attribution of distinctive anti-realist or instrumentalist theses to Duhem cannot be sustained, and that he is more plausibly interpreted as steering a course between these extremes of instrumentalism and raving realism.

A central pillar of my argument is that he took explanation very seriously. This is particularly clear in Duhem's argument against atomism. Philosophers have given little attention to the detailed arguments of the anti-atomists and been too quick to ascribe the resistance to atomism in the nineteenth century as due to positivist scruples about what cannot be directly observed. There is certainly no trace of any such appeal to observation in Duhem's case against atomism. Atomism does not figure prominently in *The Aim and Structure*. But much the same point can be made about Gillispie's thesis.

Gillispie stresses the importance Duhem attached to the formal development of physical theory as an efficient calculus. This is not sufficient to justify the anti-realist interpretation, however, since logical virtues of whatever sort might well be valued by realist and anti-realist alike. In Duhem's case, his pursuit of rigour is sufficiently explained by his concern to clearly discern what a theory does and does not say. “Mathematicians regret”, he says in an early paper (Duhem 1887, 123), “that the principles of thermodynamics should have been developed in general with so little precision that the same proposition can be regarded by some as a consequence, and by others as a negation, of these principles”. Attaining such clarity is promoted by logical order and not by reliance on intuitive inferences based on models: “a gallery of paintings is not a chain of syllogisms” (1906, 86). The central theme of his 1887 paper, the rigorous formulation and development of the principles of thermodynamics, is one which has been pursued by other authors throughout the 20th century. Duhem elaborated his approach in a longer three-part study (1892–1894), which he later improved (Duhem 1911). Here, we can see how he used the term “convention”, namely an assumption presupposed in the application of thermodynamics that he made explicit. It is not a term which can be freely interpreted in accordance with anti-realist views without duly considering whether Duhem’s texts bear out such a reading.

If the calculus charge is to be pressed home in support of the anti-realist interpretation, it must be specifically supplemented with the idea of a theory as a mere instrument for relating observations, as distinct from theoretical claims, to observations. The gigantic problem with any such interpretational claim is not merely that there is no trace of such reliance on observation in Duhem. “This famous book”, to use Gillispie’s words, is famous for presenting the strongest argument known to philosophy *against* the viability of the observation-theory distinction. Although subsequently repeated in essence by Quine, Lakatos and many others, the holistic argument against the conclusive determination of a theory’s fate by observation is most convincingly argued by Duhem because of his detailed concern with the problems of experimental error and improving precision. I will return to elaborate these points below.

Gillispie elaborates Duhem’s alleged instrumentalist concerns with an alleged disdain for “descriptions or explanations of the way the world is made”. I hope to show that Duhem’s account of precision demonstrates his concern with proper scientific description. The claim that he rejected the quest for explanation in science is misleading because, again, it imports a reading of a term which may be common in twentieth century literature but does not respect Duhem’s use of the term. What he rejects is *metaphysical* explanations, as the title of the first chapter has it, which “strip reality of the appearances covering it like a veil, in order to see the bare reality itself” (1906, 7). As I have argued (Needham 1998), it is quite clear that what he means by “explanation” here is appeal to a priori theorising about the ultimate reality underlying the deliverances of scientific theory. He traces examples of this in the history of science and regarded the quest to reduce everything to mechanics in his own time as the latest expression of this metaphysical predilection. Later in the same book, he occasionally uses “explanation” in the more familiar sense, and elsewhere, when criticising atomists, he says “[i]t is, in fact, easier to describe how the atomic school interposes atomicity in the phenomena of substitution than to ascertain how it *explains* this peculiar property of the atom” (Duhem 1892, 445; my emphasis). But even in Chap. 1, he slips into this usage when criticising Descartes, who “proved, it is true, that these effects do not contradict his principles of philosophy, but he did not give an explanation of the law by means of these principles" (1906, 18). I will develop this latter point in the next section and elaborate the points about holism and precise description in subsequent sections.

Duhem's concern with explanation

Duhem's first book (1886) was concerned with the development of the theory of thermodynamic potentials first broached by Mathieu. He makes it quite clear in the introduction that his central topic is the problem of chemical combination, where he describes the failings of Thomsen's principle of maximum work, advocated at the time by Berthelot. According to this principle, heat is always evolved when chemical reaction occurs. Endothermic reactions would not be possible by themselves, but only in conjunction with an exothermic reaction at the same temperature releasing more heat than the first absorbs. Although this accommodated most of the chemical reactions then known, it was not a quantitative theory giving a measure of chemical affinity. Moreover, there were definite exceptions which were only accommodated by what Duhem insisted were inadequate explanations based on an ad hoc distinction between physical and chemical processes (Needham 2008a). He goes on to show how all cases are accommodated by appropriately complementing the energy factor with an entropy factor, tempered by the absolute temperature, under the prevailing conditions as formulated in the thermodynamic potentials. As his historical interests made him well aware, this was the first fully workable theory of chemical affinity which explained why the chemical reactions that do occur do so, and those that do not do not.

While pursuing the development of this theory in later years, he articulated a critique of chemical atomism. I am reluctant to speak of an atomic theory since a main line of Duhem's criticism was that there simply was no theory of the nature atoms explaining chemical phenomena beyond the macroscopically discerned features that were just read into atoms. The prime example of this is the concept of valency, which he defined in such a way as to bring out its sensitivity to chemical context on the basis of his understanding of chemical formulas. Duhem showed that all the chemical theory embodied in the use of chemical formulas could be described in terms of a notion of a structural formula developed from a simple notion of a compositional formula expressing the proportions of elements in a compound (Needham 1996, 2008b). He would not allow that Daltonian atomism, contrary to popular claims, explained even the basic law of constant proportions which compositional formulas build on, although he granted that "it is easy to deduce the fundamental laws of chemistry [from the atomic theory], as Dalton showed" (1892, 441). His point, now a commonplace after Hempel's Deductive-Nomological model, was that deduction is not sufficient for explanation (Needham 2004).

Church (1958) clearly illustrates Quine's thesis that the theorist who would dispense with an ontological commitment must either give up the theory in question or show how it can be formulated without that commitment. Duhem had no idea of eliminating the theory (or even the terminology of atoms and molecules), but understood that removing the ontological commitment did not establish that atoms do not exist. He took the force of his account to show that chemical formulas were *neutral* on the question of atomism, maintaining that "a foundation [had] yet to be discovered" (Duhem 2002, 147). He was well aware that much remained to be explained, as his continued efforts testify, but could see no advance in this direction on the part of the atomic theory.

Although lack of space prevents me from going into details, I have sought to illustrate Duhem's concern for what we would call good scientific explanation, which guided his own theorising and served as a criterion by which he criticised other theories.

Approximation

The fact that arguments in science often appeal to approximation is a central theme in *The Aim and Structure* figuring in several of Duhem's arguments, one of which is misunderstood by philosophers who mistakenly contrast approximation with precision. But before coming to that I want to mention the argument which counters the meaning variance thesis advocated by Feyerabend and (perhaps) Kuhn in the early 1960s, which is often considered to be an anti-realist thesis.

Duhem realised that the textbook account of how Newton's laws deductively subsume Kepler's laws misrepresents the situation because "universal gravity … very far from being derivable by generalisation and induction from the observable laws of Kepler, formally contradicts these laws. If Newton's theory is correct, Kepler's laws are necessarily false" (1906, 193). The influence of all the remaining mass in the universe, especially the other planets, distort what would otherwise be a regular elliptic path arising solely from the mutual attraction of the sun and a single planet. Feyerabend (1962) saw in essentially this same point the basis of his "meaning variance" thesis, according to which the meaning of terms in a theory is determined by the general principles of that theory, and to change the principles is to change the meaning of the terms. This "incommensurability thesis" brought into question Nagel's conception of homogeneous reduction. It was taken to challenge the cumulative conception of scientific progress and to motivate an alternative "reject and replace" view of scientific progress. But apart from being a *non sequitur* (that theory change entails meaning change does not follow from the explicitly formulated principle of the determination of meaning by the entire theory since different conditions might well determine the same thing), Duhem provides a much more sensible interpretation.

A central tenet embodied in Duhem's holism is that a mathematical statement is endowed with "physical meaning only if it retains a meaning when we introduce the word 'nearly' or 'approximately'" (1906, 215). In particular, a hypothesis cannot be considered independently of those factors delimiting the degree of approximation with which it is upheld at a given point in history. Duhem argued against Poincaré that in interpreting formulas as statements of physical theory, we should not draw consequences by importing to them a more definite determination than experiment can support (Needham 1998, 48–50). Further, in confining interpretation within these limits, we must take into account all the considerations leading to the interpretation which observation does sustain. Such considerations are drawn from the whole body of knowledge at the scientist's command; they are not easily delimited and enumerated, but cannot reasonably be ignored. Accordingly, the incorporation of Kepler's laws within Newtonian theory should be formulated so as to accommodate facts such as that although Kepler's laws are based on the hypothesis of elliptic orbits, it was the increased precision of Tycho Brahe's observations that allowed Kepler to distinguish an elliptical form of Mars' orbit from that ascribed by his predecessors. Kepler constructed a curve which fitted within a narrower margin of error, but well understood that his own elliptical hypothesis could be justifiably upheld only within certain limits of experimental error, albeit narrower than those within which Ptolemy and Copernicus believed their claims justified. By parity of reasoning, observations conforming to yet narrower limits of error might well exclude an elliptic orbit, and this is just what Duhem points out.

Accommodating improved precision is important for the progress of science, on Duhem's view. But this has been completely misunderstood as the claim that "reality must be taken to be as it is revealed to us through observation: fuzzy and imprecise" (Worrall 1982, 213). Duhem clearly distinguishes between the vague judgement that the sun rises in the east and the scientific claim about the sun's path made precise by all the detailed considerations of experimental error underlying the careful specification of the approximation with which the scientific claim can be justifiably made (for example 1906, 169–170; see Needham 2000, 123–126). Omission of qualifications about limits of error is not a mark of precision but rather a sign that the speaker does not have the faintest idea of how to go about specifying limits of approximation within which claims can be upheld in the face of evidence. Claims so vaguely delimiting what they assert and what they deny that they can be confidently upheld without fear of contradiction by evidence are fuzzy and imprecise. A precise claim is one sufficiently well specified that it is clear what a more and a less precise claim would be, whereas a vague claim gives no indication of the degree of approximation with which it can be upheld, which would place it on a scale of more and less precise claims. This is where the holistic considerations are brought to bear, Duhem's point being not merely that theoretical considerations enter even simple experiments; it is that they arise particularly in connection with precautions taken to produce stable, reproducible results and reduce error. His comparatively detailed examples illustrate the accommodation of systematic error, bringing to bear laws and theories which seem otherwise quite distant both from the simple considerations in terms of which the experiment was initially conceived and from one another.

The instrumentalist interpretation capitalises on the fact that Duhem sometimes expresses the approximate character of precise scientific claims by saying that they are neither true nor false without considering the difficulties at issue. A more careful statement has it that "it would be absurd to say that the same point describes two of these curves [falling within the margin of error] at the same time … [t]he physicist does not have the right to say that any of these laws is true to the exclusion of the others" (1906, 171). There is no suggestion here that there is no true curve nor that "the physicist does not have the right to say that" curves falling outside the margins of error are false, but only that the physicist does not have the right to say which is the true curve beyond the claim that it must fall within the margin of error.

Progress and unity

Scientific theories are provisional because "the symbols they relate are too simple to represent reality completely", being subject to restrictions of which we can "never … possess a complete enumeration" (1906, 176). Science progresses by continually adjusting its provisional claims by modification in the light of improved observational precision, articulating assumptions and sometimes making more radical changes to accommodate surprising experimental results. Although it would be "awkward and ill inspired" (1906, 211) for the scientist to immediately question firmly entrenched views when faced with recalcitrant data, more radical changes may be needed. "Perhaps someday … by refusing to invoke causes of error and take recourse to corrections … and by resolutely carrying out a reform among the propositions declared untouchable by common consent, [the scientist] will accomplish the work of a genius who opens up a career for a theory" (1906, 211). The principle that light travels in straight lines in homogeneous media was maintained for thousands of years. It is the principle we rely upon when checking for a straight line by sight. But the day came when diffraction effects ceased to be explained by the intervention of some cause of error and optics was given a new foundation.

This gives some flavour of what is involved in Duhem's continuity thesis. It accommodates revolution, excluding only revolution by incommensurability. It is not the simple accumulation of observations that Popper found it convenient to criticise. Duhem contrasted scientific progress with the straightforward cumulative progress he thought characteristic of mathematics. The thesis is also opposed to the idea of reduction to preconceived conceptions of what physical theory should be as exemplified by the quest for reduction to mechanics widely accepted in his time as the mark of progress. New phenomena call for new ideas. Thermodynamics explains phenomena beyond the scope of mechanics, and in Duhem's (1892–1894) vision incorporates the old mechanics in a broader theory. But he had no thought that physics had thereby been completed nor that any present researcher could meaningfully delimit final complete physics in any non-trivial way. Nevertheless, unification, not by reduction to preconceived ideas but by expansion and integration into a general theory without internal contradictions, is the goal of science, which he called a natural classification. It mattered to Duhem because "the more complete it becomes, the more … the logical order in which theory orders experimental laws is the reflection of an ontological order … correspond[ing] to real relations among things" (1906, 26–27). Why would unity matter to an instrumentalist?

The scientist as impartial judge: moral values in Duhem's philosophy of science

David J. Stump

Rereading Duhem's classic work in the philosophy of science is a true pleasure that I highly recommend. There is of course more in Duhem's work than can possibly be covered in a short essay, so I will focus on his famous epistemological claims and the solution that Duhem proposes to them. My central thesis is that moral virtues are at the centre of Duhem's philosophy of science, a point that is often overlooked or slighted as unimportant by those who follow his epistemology. After laying out Duhem's position, I will consider four possible objections. The following are some of the famous claims from the book, in Duhem's own words, expressing his empiricism (scientific anti-realism), the theory ladenness of observation, and his holism (the Duhem-Quine thesis):

> A physical theory 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 (Duhem 1906, 19).
>
> *Agreement with experiment is the sole criterion of truth for a physical theory* (1906, 21).
>
> An experiment in physics is not simply the observation of a phenomenon; it is, besides, the theoretical interpretation of this phenomenon (1906, 144).
>
> An experiment in physics can never condemn an isolated hypothesis but only a whole theoretical group (1906, 183).
>
> A 'crucial experiment' is impossible in physics (1906, 188).

In order to test a hypothesis, we devise an experiment that may falsify it. However, as Duhem (1906, 187) pointed out, we never test hypotheses in isolation:

> … the physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses; when the experiment is in disagreement with his predictions, what he learns is that at least one of the hypotheses constituting this group is unacceptable and ought to be modified; but the experiment does not designate which one should be changed.

Rather than derive a simple prediction from a single hypothesis, we always make multiple assumptions and draw on other theories involved in setting up the test. We do not directly falsify a hypotheses, but rather we only know that we made a mistake somewhere in our system of beliefs. It is worth noting that Duhem thinks that this applies especially to physics because when setting up an experiment, physicists use parts of their own theory as auxiliary hypotheses to make sure that the experiment is working properly. In other sciences (Duhem uses physiology as his example), experimenters may rely on physics, but not on any aspect of the theory that is being tested. So physics is caught in a circle that other sciences may be able to avoid. Kosso (1988, 1989) revived this line of thinking to argue for experimental realism, pointing out that even in cases of very indirect, heavily theory-laden observations, it is quite possible for the theory under test to be epistemologically independent of the auxiliary theories that are used to construct the observing instrument.

Holism threatens to make testing impossible, yet Duhem believes that scientific consensus will emerge. While the pure logic of the testing situation leaves theory choice open, good sense does not. Duhem claims that the history of science shows that while there is controversy in science, there is also closure of scientific debates.

He does not just leave the scientist unable to decide between two empirically adequate theories, rather, he introduces the notion of 'good sense' by which the scientist properly choices between theories:

> In any event this state of indecision does not last forever. The day arrives when good sense comes out so clearly in favour of one of the two sides that the other side gives up the struggle even though pure logic would not forbid its continuation (1906, 218).

Duhem emphasises the choice of the scientist as a judge of theory as a moral agent who must decide where the problem lies when a scientific experiment comes out negatively (cf. 1906, 216). According to Duhem, we always have two choices when faced with negative evidence: timidity, holding on to our existing theories and changing auxiliary assumptions to accommodate the new facts, or boldness, replacing the old theory with a fundamentally different one that accounts for the new facts and the old ones too (cf. 1906, 217). Thus, both choices are rational necessitating good sense to make a judgement about which path to take, since there is no formal method by which to make a decision. In Duhem's account of scientific theory choice, there is openness, since strict rules do not apply, but also objectivity. The source of this objectivity is the epistemic agent—the scientist who acts as an impartial judge and makes a final decision.

The source of Duhem's view is Pascal, in particular Pascal's discussion of types of minds. The geometric mind, often rendered as the mathematical mind in the English translation, is narrower, abstract and logically rigorous. The *esprit de finesse*, the supple, penetrating, or intuitive mind, by contrast, "consists essentially in the aptitude to see clearly a very large number of concrete notions, and to grasp simultaneously the whole and the details" (1906, 61). Good sense belongs to the *esprit de finesse*.

Indeed, this kind of good sense is used throughout science, not just in the special case of underdetermination. Duhem also introduces the notion of an "experimental sense":

> The estimation of the degree of approximation of an experiment is, therefore, an extremely complex task. It is often difficult to hold to any logical order in this task; reasoning should then make way for that rare and subtle quality, that sort of instinct or flair called the experimental sense, a pennant worn by the penetrating mind (*esprit de finesse*) rather than by the geometrical mind (1906, 163).

So not only in comparing empirically adequate theories do we need good sense, but in all experimental science. The point that I want to emphasise here is not only the pervasiveness of good sense but also its ethical component. Duhem could hardly be clearer on the role of ethical values in theory choice, a point he attributes to Claude Bernard, but with which he completely agrees:

> The sound experimental criticism of a hypothesis is subordinated to certain moral conditions; in order to estimate correctly the agreement of a physical theory with the facts, it is not enough to be a good mathematician and skilful experimenter; one must also be an impartial and faithful judge (1906, 218).

It is very important to note that scientific judgement is *subordinated* to moral conditions, that is, Duhem makes the moral status of the agent primary. To be a competent judge in science, you must not only learn the theory in question, but you must also have the right kind of mind (*esprit de finesse*) and the right intellectual and moral virtues.

In his book, *German Science*, Duhem elaborates on the ethical dimension of good sense. Here, as before, Duhem is explicit in linking epistemic virtues to moral qualities, referring specifically to the writing of history, but generalising the point to all sciences.

> In the realm of every science, but more particularly in the realm of history, the pursuit of the truth not only requires intellectual abilities, but also calls for moral qualities: rectitude, probity, detachment from all interest and all passions (Duhem 1991, 43).

In another passage in *The Aim and Structure of Physical Theory*, Duhem also specifically links the moral conditions and the demands put on the scientific experimenter:

> he must, if he does not wish to be accused of scientific bad faith, establish an absolute separation or watertight compartment between the consequences of his theoretical deductions and the establishing of the facts shown by his experiments. Such a rule is not by any means easily followed; it requires of the scientist an absolute detachment from his own thought and a complete absence of animosity when confronted with the opinion of another person; neither vanity nor envy ought to be countenanced by him. As Bacon put it, he should never show eyes lustrous with human passions. Freedom of mind, which constitutes the sole principle of experimental method, according to Claude Bernard, does not depend merely on intellectual conditions, but also on moral conditions, making its practice rarer and more meritorious (1906, 182).

Along with the ethical component, there is a further aspect of good sense which deserves attention. Duhem introduces his central notion of "good sense" precisely because he considers logic to be insufficient for theory choice in experimental science:

> If the mathematical mind owes to the rigor of its approach all the force of its deductions, the penetration of the intuitive mind belongs entirely to the spontaneous suppleness with which it moves. No unchangeable principle determines the path which its free endeavours will follow. At one moment we see it, with an audacious leap, clear the abyss which separates two propositions (1991, 83 also see 126).

Good sense is attractive not only because it helps explicate the normative aspects of epistemology, but also because it permits the flexibility of non-rule governed explanations of epistemic choice.

In a previous article (Stump, 2007), I linked Duhem's emphasis on moral values with contemporary virtue epistemology, which can be introduced as follows:

> Just as virtue theories in ethics try to understand the normative properties of actions in terms of the normative properties of moral agents, virtue epistemology tries to understand the normative properties of beliefs in terms of the normative properties of cognitive agents (Greco 2004).

The key point about virtue theories is that there is a change in what is taken to be primary in analysis of ethical terms. Virtue theorists argue that moral or epistemic virtues are basic. Rather than define a virtuous person as one who conforms to principles of morality, the virtue theorist defines a right action as that which would be done by a virtuous person. Likewise in epistemology, the strong form of virtue epistemology would define justified true belief in terms of what an intellectually virtuous person would believe (cf. Blackburn 2001, 16).

Like contemporary virtue epistemologists, Duhem also takes knowledge to be dependent on the virtues of the knower. Scientists must have intellectual and, indeed, moral virtues in order to reach scientific knowledge, especially when choosing between empirically adequate theories. There is no doubt that the bottom line for Duhem is the ethical condition of the scientist. Scientific judgement frequently comes down to the use of good sense and its use requires both epistemic and moral virtues, as well as the right kind of mind.

Now let us consider some possible objections, both to Duhem's position and to my interpretation of his position. First, there are two ways of undercutting Duhem's solution to the problem of underdetermination. One is simply to deny that there really is a problem of empirically equivalent theories in the first place. One could claim that it will always be possible to find some empirical difference between theories or that one can block the revisionary moves necessary to maintain a theory in the face of negative evidence by testing auxiliary assumptions and by blocking the introduction of *ad hoc* hypotheses. This first response denies that the Duhem–Quine problem is real. It is beyond the scope of this essay to reply; for my purposes, it is enough to say that Duhem clearly thinks that the problem is real and demands a solution. A second kind of response is to accept the Duhem–Quine thesis while denying that there are any further virtues that allow us to decide between theories. Duhem himself sometimes seems close to this conventionalist or relativist alternative, as he argues in "Physics of a Believer", printed as an appendix in *The Aim and Structure of Physical Theory*:

> No doubt the physicist will choose between these logically equivalent theories, but the motives which will dictate his choice will be considerations of elegance, simplicity, and convenience, and grounds of suitability which are essentially subjective, contingent, and variable with time, with schools, and with persons; as serious as these motives may be in certain cases, they will never be of a nature that necessitates adhering to one of the two theories and rejecting the other, for only the discovery of a fact that would be represented by one of the theories, and not by the other, would result in a forced opinion (1906, 288).

According to conventionalists, there is simply no cognitive way to decide between empirically equivalent theories. Despite the fact that Duhem is sometimes read this way, I maintain my reading on the basis of quotes given earlier that show that a scientist with good sense will choose between competing theories, despite the fact that it is possible to continue to defend a theory with sufficient adjustments to auxiliary assumptions. Duhem seems confident that scientific controversies will always be settled eventually, because good sense will weigh so heavily on one side that no one will be able to maintain the other. In the passage quoted above, Duhem maintains that the objectivity of the theory selection rests with the scientist as an epistemic agent, not with nature. Such a choice by a scientist with good sense will result in genuine knowledge of physical theory, according to Duhem.

Second, one could claim that it is common sense, not values, that determines the choice made by good sense. Indeed, Duhem does praise common sense highly as a source of truth (1906, 104), and there are times when he seems to imply that it is all that we need, neglecting to mention good sense:

> It is quite correct, then, to declare that physical science flows from two sources: one the certainty of common sense, and the other the clarity of mathematical deduction; and physical science is both certain and clear because the streams which spring from these two sources run together and mingle their waters intimately (1906, 267).

However, he says elsewhere that while common sense plays a role in mathematics, where axioms are founded on intuition or common sense and one must have good sense to know when deduction leads one astray, in experimental science there are no taken for granted axioms, only approximate truths founded on experimental results (cf. 1991, 81–82). While it is true that the *esprit of finesse* is connected more closely to ethics in *German Science* than it is in *The Aim and Structure of Physical Theory*, the quotes given above and direct and clear. In fact, Duhem thinks that we need both common sense and intellectual and moral virtues in order to be good scientific judges.

Third, one might say that Duhem had different aims from those of the virtue epistemologists and that it is not helpful to associate him with them (Ivanova 2010). It is certainly true that Duhem does not share all the views of contemporary virtue epistemologists. Indeed, contemporary virtue epistemologists are not a homogeneous group and Duhem came at the issues from a very different context. My point, however, is that on two key issues, there is an overlap between Duhem and the virtue epistemologists—making values primary and rejecting rule-based decision procedures. The fact that Duhem may have been a structural realist, for example, is completely compatible with his being a virtue epistemologist. While Duhem may turn out to have developed a unique form of virtue epistemology, he still shares these core features with others in that group. If nothing else, thinking about Duhem in this way reminds us that he is far from the Logical Empiricists who adopted him in the twentieth century.

Finally, one might say that Duhem's notion of "good sense" just cannot be taken seriously given that it is far too subjective and loose. I would respond that Duhem is not Quine, so while he introduces the problem of holism, he does not leave us there. He offers a solution to the problem of underdetermination and argues from the history of science that science does in fact reach consensus. There is no doubt he thinks these scientific decisions are rational, but of course to accept this, one must agree to a theory of rationality that does not require that all judgements can be reduced to rules and algorithms. I do not think that one has to accept virtue epistemology to make a claim that scientific choices are rational, but it is interesting to read Duhem as taking such a stand.

Homme de science, homme de foi: Pierre Duhem on science and religion

Robert Deltete

Pierre Duhem is a complex and fascinating figure for people who work in the history and philosophy of science—especially so for those of us, like me, who are also Catholic. He made substantial contributions to physics, the philosophy of science and the history of science—an almost unique achievement. But while certainly no theologian, Duhem *was* a devotional Catholic—he attended mass daily, where he said the rosary, took communion and regularly professed his adoration for Mary, the mother of Jesus. How could that be? As a widely read critic of religion wrote in his *History of the Conflict Between Religion and Science*:

> Roman Christianity and Science are…absolutely incompatible; they cannot exist together; one must yield to the other; mankind must make a choice—it cannot have both (Draper 1874, 363).

Duhem thought one could have both, that one did not have to make a choice. In section *Carving out a Position*, I explain how he thought that was possible. The key is what, following more recent contributors to the science and religion dialogue (see Barbour 2000, 17–22), I call the "Independence Thesis" (IT). In later sections, I seek to show that Duhem nevertheless sought to go beyond IT in a way that should be as problematic to Catholics as it will be to non-Catholics.

Carving out a position

Duhem is difficult to approach. In part, this is because he wrote so much, in so many fields, in his short life. But he is also difficult to approach because the environment in which he lived and wrote was so complex and unsettled—intellectually, politically and socially, and religiously. Duhem lived and worked in the French Third Republic, which he did not like and did not support, and that made him an outcast, but he was also a Catholic at odds with many of the intellectually oriented Catholics of the time who were deeply suspicious of him. In consequence, Duhem walked a tightrope. He spent much of his mature life trying to deal with conflicting claims to his allegiance by carving out an intellectual position that respected both the science he practiced and the faith he lived. In spite of his best intentions, however, Duhem rarely satisfied anyone.

So what was the position Duhem sought to defend? It is best set out, in summary form, in a 1905 essay "Physique de croyant" and is presented more fully in his *La Théorie physique—son objet, sa structure* of 1906. But he had expressed the main ideas more than a decade earlier in essays that have only recently gotten much attention. The essential feature was a radical separation between physics (more generally, natural science) and metaphysics, including theology (the IT). A passage from an 1893 essay, "Physique et métaphysique" (1893, 57–58) makes the point clearly:

> Physics is the study of *phenomena* having their basis in brute matter and of the laws relating them. Cosmology ['the part of metaphysics which treats of non-living matter and that, in consequence, corresponds to physics by nature of the things studied'] seeks to know the nature of brute matter, considered as the *cause* of phenomena, and as the *raison d'être* of *physical laws.* Between metaphysics and physics there is thus a distinction in kind (my emphasis).

Physics and metaphysics are thus by nature distinct, each having its own goals and methods. A main reason, indicated in the last passage and made more explicit later, is that while metaphysics seeks to be causally explanatory, physics does not. It seeks only to describe and relate the phenomena—to represent them, but not to explain them. Duhem had several reasons for sharply separating physics from metaphysics. One was to defend the autonomy of physics, its independence from metaphysics. A second was, at the same time, to *protect* metaphysics and theology from all of the anti-metaphysical and anti-theological diatribes of the Republican positivists. And a third was—this will no doubt seem odd—to discourage fellow Catholics from using the results of science to promote Christian apologetics.

Let me amplify, if only briefly, each of these reasons. Duhem sought to defend his idea of physics as an autonomous discipline from the encroachment of metaphysics, which he thought had historically inhibited its fruitful development. He did this most famously in Part I of *La théorie physique*. There (1914, 24) he wrote:

> A physical theory is not an explanation. It is a system of mathematical propositions [he is clearly thinking only of physics], deduced from a small number of principles, whose aim is to represent as simply, completely, and exactly as possible a set of experimental laws.

This approach was, he thought, *positive*, but not *positivist*.[4] Indeed, Duhem denied that he was a positivist—someone for whom there is no rationally defensible method apart from that employed in the exact sciences, the positive ones, so that what is inaccessible to this method is *ipso facto* unknowable. On the contrary, he thought that, as he (1893, 58) put it

> The knowledge which metaphysics gives us of things is far more intimate, more profound, than that which is furnished by physics; it thus surpasses the latter in excellence.

In separating science from metaphysics, therefore, Duhem was not trying to demarcate sense from nonsense in the manner of earlier and later positivists. He thought that metaphysics, including theology, *was* a genuine form of knowledge with its own object and method. So he sought to protect it from the positivists. At

[4] The standard English translation misrepresents Duhem in rendering the French *positif* as *positivist* or positivistic. Cf. Duhem (1914, 416, 422, 423 and 428) with (1906, 275, 279 and 282).

the same time, though, he did not embrace the approach of many fellow Catholic intellectuals who were responding to the encyclical *Aterni Patris* of Pope Leo XIII in 1879, which urged the works of St. Thomas Aquinas as a cure to contemporary scientism and atheism. Duhem did not respect most of the proponents of neo-Thomism and so tried to distance himself from them. Some only tried to show that scholastic philosophy could be reconciled with contemporary science; others, however, argued that it had given rise to modern science, and some even asserted that science should be constrained by scholastic wisdom. Always fiercely independent, Duhem bristled.

Physique de croyant

This position is presented concisely in mature form in Duhem's essay "Physique de croyant" (1905). The essay was occasioned by a review of Duhem by the French doctoral student Abel Rey, which concluded that Duhem's "scientific philosophy is the philosophy of a believer" (*la philosophie scientifique d'un croyant*) (Rey 1904, 444; quoted in Duhem 1914, 414). Duhem was appalled. He (1914, 414) began by declaring that he was a believer in the teachings of the Catholic Church, but he interpreted Rey to mean that one had to be a Catholic in order to accept his physics.[5] Duhem denied that. He repeated that his discipline was autonomous, that it had its own goals and methods (cf. 1914, 414–415). Indeed, he reiterated his dual claim that his system of physics was "positive" in both its origins and its conclusions (cf. 1914, 416–422, 422–428), so it can be "as much the physics of an unbeliever as a believer" (1914, 427). But he then went to work on the positivists, arguing that his view of physics "sweeps aside the supposed objections of physical science to spiritualistic metaphysics and to the Catholic faith" (cf. 1914, 428). And he also went to work on fellow Catholics, arguing that his system of physics carried "no metaphysical or apologetic import" (1914, 435–440):

> It is absurd to claim that a principle of theoretical physics contradicts a proposition formulated by spiritualistic philosophy or by Catholic doctrine; [but] it is no less absurd to claim that it confirms such a proposition (1914, 435).
>
> Thus, the conclusion to the first five parts of his essay:
>
> We therefore propose a theoretical physics that is neither the theory of a believer nor that of an unbeliever, but merely and simply the position of a physicist; [while it is] admirably suited to classify the laws studied by the experimenter, it cannot oppose any assertion of metaphysics or of religious dogma, and is equally incapable of lending any support to any such assertion (1914, 441).

Note the two-sided balancing act. This looks like a position that has Duhem arguing that science and metaphysics do not conflict, but only because they do not

[5] Duhem misinterpreted Rey, since Rey did not discuss Duhem's physics in his review. Instead, he thought that Duhem's philosophy of science was the philosophy of a *croyant*, in an odd use of the word.

interact. They approach the same reality but, having different objectives, they deal with it in different ways: they ask different questions, have different domains of discourse and apply different methods. He can therefore defend *both* his physics *and* his religion by keeping them is separate compartments. This is what IT should tell him to do. His detailed analysis of the task of physics and its methodology purports to cut off *both* any science-based attacks on religion *and* all possibility of a science-based natural theology. So he should just let things rest. But this is *not* what Duhem does. In the last four parts of his essay, he instead argues that "physical theory has as its limiting form a natural classification" (NC) (1914, 445–453), that "between cosmology [the part of metaphysics that concerns inanimate nature] and physical theory there is an analogy" (1914, 452–453) and that the most propitious analogy, for which history had prepared the way, is an analogy between a generalised thermodynamics and Aristotelian metaphysics, "shorn of its fossilised elements" (1914, 462–472).

So, having worked hard to separate physics from metaphysics via IT, Duhem then worked just as hard to bring them back together via the idea of NC. I will not try to explain here what an NC would consist in except to note two things: *first*, it would be a physical theory that "reflects" or "mirrors" the underlying ontological order and *second*, the relation would be one of analogy, not identity. Nor will I try to explain why Duhem thought that a generalised thermodynamics, or energetics, is the most propitious analogy to a generally Aristotelian metaphysics, since that would take me too far afield. Instead, I want to ask why Duhem thought physical theory was approaching an NC at all. When he first introduced the idea of NC in the early 1890s, it was little more than a vague hope based on an appeal to 18th and 19th debates on proper biological classification. By the time he composed *La théorie* a decade later, however, he had formulated several arguments in its favour.

To set the stage for them, recall his definition of theory as "a system of… propositions deduced from a small number of principles, whose aim is to represent as simply, completely, and exactly as possible a set of experimental laws". This is in the interest of intellectual economy. Also in that interest is his claim that "Theory is not only an economical representation of experimental laws; it is also a *classification* of them" (1914, 30), although Duhem seems to have thought that this goes beyond what positive method can justify (cf. 1914, 287). And he then goes on to argue that such classification is not artificial, but, rightly pursued, approaches NC:

> [It is] the aim of physical theory to become a *natural classification*, [i.e.] to establish among the diverse experimental laws a logical co-ordination that is an image and a reflection of the true order according to which the realities that escape us are organised (1914, 41).

But that claim, which Duhem thinks the physicist is right to endorse, goes far beyond what he is entitled to claim *qua* physicist.

How to proceed? Duhem's reply is to say that physicists operate (and should operate) from principles that cannot be justified in terms of their own proper method. One of these is the following postulate: "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" (1914, 445). Why does physical theory *have to* do that? Duhem's answer is that otherwise the representations of theory would be *only* "convenient summaries", *only* "artificial devices destined to facilitate the work of discovery" (Duhem 1914, 446). So what? If theory conveniently summarises current knowledge (intellectual economy) and effectively facilitates the discovery of new knowledge, what more is needed?

Here, Duhem argues that artificial classifications seldom promote (nor do we expect them to) "the work of discovery"; that the classifications actually provided by physical theories have, historically, promoted this work; and that theory tending to natural classification is the most plausible explanation for this. So the postulate is justified, since its end product, its limiting term, would be NC. This summary indicates two arguments (and suggests a third) for NC that Duhem offered in defense.

The *first* is what I will call the "argument from the history of science", or, more briefly, the "historical argument": The historical development of science testifies to its approach towards NC. As Duhem (1914, 447–448) wrote: "Diversity fusing into a constantly more comprehensive and more perfect unity, that is the great fact summarising the whole history of physical doctrines". But he added that progressive unification is only on the side of representation, not explanation; metaphysical fashions motivating such explanation come and go, like the ebb and flow of the tide. But on the side of *representation*, he thought, "each theory passes on to the one following it a share of the natural classification that it was able to construct" (1914, 48; also 53 and 410–411).

A *second* argument for natural classification is what might be called the "successful prediction" or "miracle" argument. It is simply stated. Physical theory can predict both particular outcomes of experiment and new experimental laws. But what reason would there be for thinking that such predictions will be confirmed *if* physical theory is *just* an artificial classification, an amalgam of useful maxims, without any ontological import. Duhem's answer is: none at all (cf. 1914, 36–40, 242, 450–452). Predictive success would then be an "incredible accident"—a "miracle".

> But *if*… we recognize in the theory a *structure* tending toward a natural classification, *if* we feel that its principles express profound and real *relations* among things, *then* we will not be surprised to see its consequences successfully telling about new phenomena and stimulating the discovery of new laws (1914, 37–38, my emphasis).

A *third* argument for natural classification has its basis in the other two, but is sufficiently distinct that I consider it on its own. Let me call this the "aspiration" or "conviction" argument. The basic idea here is that the mind naturally aspires to coherent unity: Natural scientists seek a coherent, unified understanding of the world because they think that the world really *is* a coherent, unified whole—a "universe". This argument for NC is simple: If the world is a coherent, unified whole, as scientists (rightly) believe it to be, then any adequate physical theory must also be coherent and unified, and so yield, in its limit, an NC.

Duhem's arguments

How should we view these arguments? Note that they do not—even taken together—constitute a proof, a conclusive demonstration, of Duhem's main assertions: that physical theory should try to represent all natural laws by means of a single, coherent, logical system; that such a system would be an NC; and that this ideal system would reflect the coherent, unified order of the real world. But Duhem *never* thought that they did; in fact, he is quite explicit in arguing (not just conceding) that his arguments do not yield any proof (e.g., 1914, 156, 234). I think Duhem's arguments are best construed as what we would now call "inferences to the best explanation"—the most plausible, reasonable explanations, given the circumstances. He argued, as we have seen, that it is such an inference that best explains the progressive unification of the natural sciences, the predictive success of physical theories (as well as the failures that motivate improvement) and also the aspiration that theorists have (and have always had) towards coherence and unity. Duhem was fully aware that such things cannot be proved, but he nevertheless thought they were defensible. I do too, though I realise that scientific realism is a hugely contentious issue in the philosophy of science.

Still, Duhem was evidently not satisfied with his "naturalistic" arguments for NC, and so often backs them up with an appeal to a quasi-Hegelian "directing idea" (e.g., 1903, 345) or—sometimes quite explicitly—to Providential direction. I wish he had not done that, but he did. Here is a passage that forms the last paragraph of *Les origines de la statique*. In it, Duhem (1905–1906, vol. 2, 447–448) brings together, triumphantly, fifteen centuries of work in statics.

> How could all these efforts combine with such precision and bring to completion a plan which was not known to the individual laborer, unless this plan existed previously in the mind of an architect, and if this architect did not have the power to direct and co-ordinate the labour of the masons? Even more than the growth of a living being, the evolution of statics is the manifestation of the influence of a guiding idea. Within the complex data of this evolution, we can see the continuous action of a divine wisdom which foresees the ideal form towards which science must tend and we can sense the presence of a Power which causes the efforts to converge towards this goal. In a word, we recognize here the work of Providence.

What is one to make of such a passage? As also an inference to the best explanation? Or as the imposition of a religious reading on the historical development? I am inclined to think that it is the latter, so that if Rey did not actually accuse Duhem of importing religious beliefs into the development of physical theory, he rightly could have, since Duhem's view of that development *is* that of a religious believer.[6]

[6] Helge Kragh has suggested in a recent essay (2008) that although Duhem denied that science could be used for apologetic purposes, he apparently thought that the history of science could. Kragh find this "rather surprising, if not inconsistent" (2008, 390). I agree. If the second law of thermodynamics, as applied to the universe as a whole, cannot be used to infer the existence of God, as Duhem thought (1914, 435–440), then neither can the history of statics.

Conclusion

I have argued that Duhem sought to carve out an intellectual position between two opposing camps, both of which sought his allegiance. On the one hand, he sought to distance himself from positivist Republicans, even while accepting (and promoting) many of the basic tenets of positive science. On the other hand, he sought to distance himself from Catholic intellectuals who wanted to deploy natural science in the cause of Christian apologetics. Against the former, he insisted on the value and legitimate rights of physical theory by arguing that its classification schemes were tending more and more towards an NC as their goal, a terminus that would reflect the underlying natural order. Against the latter, he argued that his physics was autonomous, that it did not depend on metaphysics—religiously oriented or otherwise. Central to both responses, I have argued, was Duhem's sharp distinction between physics and cosmology, the latter of which for him included theology. This allowed Duhem to protect metaphysics and religion from the positivist scalpel and, at the same time, to protect physics from misuse by his fellow Catholics.

Duhem sought to separate physics from metaphysics, but, as I have argued, he also tried to bring them into contact. The key to this *rapprochement* was the concept of NC, the idea that physical theory tends to a classification of physical and chemical phenomena which mirrors the ontological order of nature. This is one of a pair of tensions in Duhem's work. A second is that while his "scientific philosophy" offered arguments for natural classification that did not depend on religion, Duhem could not resist bringing Providence back in as the ultimate explanation for the approach of physical theory to NC. And this should trouble Catholics as much as it certainly will non-Catholics.

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