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The Duhem Thesis

by ROGER ARIEW

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There are numerous references in contemporary sociology of science to a thesis called the underdetermination of theories—that there are in principle an indefinite number of theories that fit the observed facts more or less adequately—attributed to Pierre Duhem and Willard Van Orman Quine (*cf.*, for example, David Bloor [1981], Karin Knorr-Cetina [1983], and Andrew Pickering [1980]). The proponents of underdetermination argue that any theory can be maintained in the face of any evidence, provided we make sufficiently radical adjustments elsewhere in our beliefs, and that from underdetermination it follows that social factors must be invoked to explain why a scientist adopts a particular theory. This general thesis (or group of interconnected theses) is complex, so that unpacking it would require some preliminaries; for now, let us just say that it is evident that these sociologists of science take what they call the underdetermination thesis (sometimes called the Duhem thesis, the D-thesis, the Duhem-Quine thesis, the Duhemian problem, and holism) as given; some even see their task at times as providing empirical evidence for it (Pickering [1980], p. 113). On the other hand, there are many references concerning the Duhem thesis by philosophers of science who are very often critical of it. For example, Noretta Koertge claims to have solved the Duhemian problem (Koertge [1978], p. 253); one can also find similar assertions by Karl Popper (Popper [1963]) and more recently by Clark Glymour and others (Glymour [1975] and [1980]).

What is curious is that Duhem himself would not have been able to recognize what is attributed to him in any of the above writings. Though curious, this is not a very unusual state of affairs, of course; often a thesis is reinterpreted according to contemporary sensibilities and attributed to its originator, even though the reinterpretation warps the original intention. But Duhem is too important a thinker to allow his thought to remain clouded by contemporary reinterpretations. Some commentators and critics have already given Duhem's thought careful exposition, and their work has demonstrated that Duhem's actual thesis is much superior to the thesis

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normally attributed to him (*cf.* Laudan [1965]). But there is more exegetical work to be done. There are structures of Duhem's thought to be exposed that require the reading of Duhem's original text and its variations: Duhem's text spans a number of years—from 1892 to 1916, with a revised edition in 1917 (and 1981) and an English translation in 1954. The text underwent continual modifications from 1892 to 1917. In fact, it even made some difference to Duhem that he had discussed these topics as early as 1892 (1894 for the core of the Duhem thesis) since there is a question of priority of views between some of Duhem's views and the views of others—Poincaré, Milhaud, Le Roy, Wilbois, Hadamard, *etc.* (*cf.* Duhem [1954], pp. 144n, 150n, and 216n; for the publication history of Duhem's doctrine of physical theory, *cf.* Duhem [1892] to Duhem [1954]). Careful reading of Duhem's text reveals the many limitations Duhem imposed on his own thesis. I wish to exhibit these limitations and to argue for their importance to Duhem's thought. I have divided my exposition into four main sections. In section 1, I argue that Duhem's thesis is not the same as the Duhem-Quine thesis, and that Duhem's thesis cannot be faulted by the criticism that would be proper against the Duhem-Quine thesis.¹ In section 2, I argue that Duhem's primary thesis is what has been called the separability thesis, and that he regards what has been called the falsifiability thesis (which is generally taken to be his primary thesis) to be a consequence of the separability thesis. In section 3, I examine some consequences of the failure to understand Duhem's argument for alleged criticisms of the Duhem thesis. Finally, in section 4, I comment on the historical and philosophical significance of the Duhem thesis as formulated by Duhem.

1 DUHEM'S THESIS IS NOT THE DUHEM-QUINE THESIS

It is peculiar that the Duhem thesis did not make its mark on Anglo-American philosophy for a period of more than thirty years, that is, until it was introduced into Anglo-American philosophy in the 1950s through the work of W. V. O. Quine, who was relying on little more English-language sources than Phillip Frank's references to Duhem (Frank [1941]) and Armand Lowinger's book on Duhem (Lowinger [1941]). In Quine's 'Two Dogmas of Empiricism', the second dogma is reductionism, the belief that 'each meaningful statement is equivalent to some construct upon terms which refer to immediate experience' ([1953], p. 20). Quine, of course, argues that reductionism is an ill-founded dogma. He asserts that although reductionism has ceased to figure in some empiricists' thoughts, there remains a more subtle form of reductionism that each statement taken in isolation can admit of confirmation (or disconfirmation). Against this dogma Quine suggests a doctrine which he says was well argued by Pierre Duhem,

¹ I am extremely indebted, in this section, to Larry Laudan's excellent article on the Duhem thesis [1965]; I am also indebted to a paper he presented in April 1982 to the Philosophy Colloquium at Virginia Polytechnic Institute and State University [1982].

'that our statements about the external world face the tribunal of sense experience not individually, but only as a corporate body' ([1953], p. 41). Quine then proceeds to detail an 'empiricism without the dogmas' in which knowledge is to be likened to a field of force where 'a conflict with experience at the periphery occasions readjustments in the interior of the field' ([1953], p. 42), and 'any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system' ([1953], p. 43). Because of Quine's references to Duhem, the thesis formed from the two subtheses, that (i) since empirical statements are interconnected, they cannot be singly disconfirmed, and (ii) if we wish to hold a particular statement true we can always adjust another statement, has become known as the Duhem thesis. We should emphasise that the thesis is the conjunction of two subtheses [(i) and (ii)], and that Quine attributes only one of them to Duhem [(i)]. Duhem might have recognised the subthesis attributed to him by Quine as a step-child of his thesis, but he would *not* have approved of *either* subthesis as they are formulated above.

As previously asserted, the literature on the Duhem thesis has proliferated without regard to Duhem's formulation; some articles have attributed formulations to Duhem for which Quine might have been a more suitable attribution. For instance, Adolph Grünbaum argues that 'Duhem's thesis' is a *non-sequitur* and is actually false. The thesis that is given as Duhem's is that the '*falsifiability* of an isolated empirical hypothesis H as an *explanans* is *unavoidably inconclusive*' ([1960], p. 75; this is a paraphrase of (i)). But Grünbaum's argument against this thesis is that 'Duhem *cannot* guarantee on any logical grounds the deducibility of O [empirical findings] from an *explanans* constituted by the conjunction of H and some revised *non-trivial* version R of A [auxiliary assumptions]' ([1960], p. 75; Grünbaum also describes a case study purporting to show that Duhem's thesis is false). This argument may be an adequate argument against Quine (since it attacks subthesis (ii)), but it badly misfires against Duhem (who, it will be shown, does not hold subthesis (ii)).

It should be emphasised that Grünbaum's argument is an important argument, especially against the versions of the Duhem thesis accepted by modern relativists in sociology of science—David Bloor, Harry Collins *et al.* Lest it be thought that only sociologists of science subscribe to Quine's version of the thesis, it should be pointed out that recently Mary Hesse has defined the underdetermination thesis as the thesis that 'there are in principle always an indefinite number of theories that fit the observed facts more or less adequately' (Hesse [1980], p. viii) and has attributed the underdetermination thesis to Duhem and Quine ([1980], pp. viii and xxv). But Hesse's underdetermination thesis is either empirically false or trivially true—this is what Grünbaum said about the Duhem thesis formulated as Hesse formulates the underdetermination thesis. It is false historically that there have been an indefinite number (or a large number) of rival theories accounting for some given set of facts, and it is open empirically whether

there can be an indefinite number of non-trivially different theories that can account for any given set of facts; and it is logically true that there are an indefinite number of theories that can account for any given set of facts, as long as one does not insist on the theories being non-trivially different. However, underdetermination does not need to suffer this difficulty as long as one understands underdetermination as simply the inability of empirical evidence to confirm or disconfirm conclusively a given theory—subthesis (i)—without having to add that there are an indefinite number of similar theories we can call upon to account for a given set of facts—the corollary to subthesis (ii). Of course, it is possible that by inserting the phrase ‘in principle’ in her formulation of the underdetermination thesis Hesse does not commit herself to there being historically (or at any time) an actual indefinite number of theories that fit the facts more or less adequately, but indicates only that she is making the logical point; though this is a possible interpretation of Hesse’s thesis, one should note that Hesse also thinks that ‘if we take the thesis of the underdetermination of theories seriously, relativism is a consequence that is inescapable in some form’ ([1980], p. xiv). In order to arrive at such a result, the underdetermination thesis has to be more than a mere logical thesis; ‘in principle’ must have some empirical content. Hesse must be considering situations where in actuality there is a large number of theories that can account more or less adequately for a given set of facts; she must also be thinking that there are no criteria of rationality other than choosing the theory that accounts for the facts (or that all such criteria all together are inadequate). Given these two premises, one might be able to conclude that a relativism (most likely a sociological relativism) is needed to account for the scientist’s decision for a given theory.

Having indicated that Grünbaum’s argument may be an adequate argument against subthesis (ii), it should be shown that Duhem does not hold subthesis (ii). Duhem does not claim, as does the D-thesis, that when there is some conflict with experience, we can always make enough adjustments elsewhere in the system. He makes a weaker claim, also limited in other ways, but which would be equivalent to: when there is some conflict with experience what is disconfirmed is necessarily ambiguous; ‘when the experiment disagrees with the physicist’s prediction, he learns that at least one of the hypotheses that constitutes the set is erroneous, but the experiment does not indicate which’ (Duhem [1954], p. 187).

It is clear that Duhem holds at most only a portion of the D-thesis, namely that ‘if the predicted phenomenon is not produced, not only is the questioned proposition put into doubt, but also the whole theoretical scaffolding used by the physicist’ (*ibid.* [1954], p. 185). There is no question here of holding any statement true come what may, merely that ‘the only thing the experiment teaches us is that there is at least one error among the propositions used to predict the phenomenon and to verify that it has not been produced; but where the error lies is just what the experiment does not tell us’ (*ibid.* [1954], p. 185; *cf.* also pp. 211 and 216). Some of Duhem’s most

telling remarks indicating that he does not hold the ‘stronger’ D-thesis occur in a section of *La Théorie physique* entitled ‘Good Sense is the Judge of Hypotheses Which Must be Abandoned’. There Duhem refers to two possible ways of proceeding when an experiment contradicts the consequences of a theory: a timid scientist might wish to safeguard certain fundamental hypotheses and *attempt* to complicate matters by invoking various causes of error and multiplying the corrections, while a bolder scientist can *resolve* to change some of the essential suppositions supporting the entire system ([1954], pp. 216–17; emphasis mine). It is clear that for Duhem the scientist has no guarantee of success—again there is no question of upholding any hypothesis come what may: ‘If they both succeed in satisfying the requirements of the experiment, each is logically permitted to declare himself content with the work he has accomplished’ ([1954], p. 217; emphasis mine). Of course, we can always reject the work of one scientist or another; ‘pure logic’ is not the only rule for our judgments. If ‘pure logic’ were the only rule for our judgments, then ‘strictly speaking’ we might be able to hold any statement true come what may (*cf.* Duhem [1954], p. 218, with respect to Biot’s defence of the emission hypothesis against Foucault). We might think that the bold scientist has been too hasty or that the timid scientist has been unreasonably obstinate. Duhem even seems to indicate the grounds by which one can deem an alteration *ad hoc*.¹

It has been argued that Duhem does not hold the Duhem–Quine thesis—for which more evidence will be provided later; it has also been asserted that Quine was responsible for introducing the Duhem–Quine thesis into Anglo-American philosophy. These two items together do not entail that Quine holds the Duhem–Quine thesis. That is a question open to scholarly interpretation. It is evident, at least, that Quine does not now hold the Duhem–Quine thesis as attacked by Grünbaum (*cf.* the letter to Grünbaum in Quine [1976], where Quine asserts that Grünbaum’s claim ‘that the Duhem–Quine thesis . . . is untenable if taken nontrivially . . . is persuasive’ and that for his own part he ‘would say that the thesis as [he] has used it is probably trivial’). It is possible that the present Quine position evolved from his earlier ‘Two Dogmas of Empiricism’ position; after all, Quine has now specified the structure of the field of force analogy and asserts that ‘sentences higher up in theories have no empirical consequences they can call their own; they confront the tribunal of sensory evidence only in more or less

¹ What Duhem indicates is that good sense is the judge of whether a hypothesis is *ad hoc*. Although a full discussion of ‘good sense’ would have one stray from the topic at hand, it should, at least, be indicated that by ‘good sense’ Duhem means an extra-logical, but not an extra-rational faculty, something like intuition, as opposed to deduction—*raison* as opposed to *raisonnement*—*cf.* Duhem [1915] where good sense is characterised as that which allows one to intuit the truth of axioms in the deductive sciences (*Les Sciences de Raisonement—Première leçon*, pp. 1–22) and that which allows one to intuit fundamental principles or assumptions in experimental science (*Les Sciences Expérimentales—Seconde Leçon*, pp. 23–50). For a thorough discussion of good sense and Duhem’s use of Pascalian ideas, *cf.* Martin [1981] (*cf.* also Martin [1982]).

inclusive aggregates. The observation sentence, situated at the sensory periphery of the body scientific, is the minimal verifiable aggregate; it has an empirical content all its own and wears it on its sleeve' (Quine [1969], p. 89); in this specification of the Quinean thesis, the sense of 'anything goes' looks like a mere logical sense. However, it is open whether this is a specification of the initial view or an evolution away from the initial view. It is entirely plausible that Quine simply overstated the initial view (*cf.* the letter to Grünbaum: 'Actually my holism is not as extreme as those two brief paragraphs at the end of "Two Dogmas of Empiricism" are bound to sound'—Quine [1976]) or that later interpreters have read Quine uncharitably.

2 DUHEM'S PRIMARY THESIS IS SEPARABILITY, FROM WHICH FALSIFIABILITY DERIVES

Duhem does not argue directly for what is called the Duhem thesis, or the thesis that has been taken to be his primary thesis, what Philip Quinn calls the falsifiability thesis ([1974], p. 37), even in its restricted formulation. He seems to regard it as an obvious corollary of another thesis, which, following Quinn, will be called the separability thesis (*ibid.*, [1974])¹ that the physicist can never submit an isolated hypothesis to experimental test: 'To seek to separate each of the hypotheses of theoretical physics from the other assumptions upon which this science rests, in order to subject it in isolation to observational tests, is to pursue a chimera' (Duhem [1954], pp. 199–200 and elsewhere).

Quinn has argued that the separability thesis is entailed by the falsifiability thesis, but not vice-versa (although he conjectured that Duhem might have thought the two theses entail each other) (Quinn [1974], p. 37). But he retracted this opinion following a discussion with Nancy Tuana (Tuana [1978], pp. 456–62 and Quinn [1978], pp. 463–5). Quinn argued that falsifiability does not follow from separability alone, but from separability together with the thesis that no theoretical hypothesis can be conclusively verified (if one can conclusively verify a theoretical hypothesis, then there is a falsifiable hypothesis—namely A, the hypothesis falsified by the negative experimental result affecting the conjunction of the verified hypothesis and A). Tuana responded by indicating that if any theoretical hypothesis can be conclusively verified, then the separability thesis itself is false—an excellent

¹ Although non-separability (and non-falsifiability) might have been better designations. To simplify the discussion, one can regard the separability thesis as 'no theoretical hypothesis of physics by itself has observational consequences' and the falsifiability thesis as 'no theoretical hypothesis of physics can be conclusively falsified by observations alone'. However, the text shows that Duhem does not need so strong a thesis—'few or no theoretical hypotheses' will do in both cases. It might have been thought that the Duhem–Quine thesis is a conjunction of the separability and falsifiability theses; it should now be clear that the Duhem–Quine subthesis (i) in itself contains both the separability and falsifiability theses and that the Duhem–Quine subthesis (ii) is entirely independent.

criticism since Quinn was attempting to give an example where separability is true and falsifiability is false by itself. Quinn now believes that the only conclusion one can draw from *La Théorie physique* is that the two theses hang together at the core of Duhem's system (Quinn, [1978]). This is also an inaccurate interpretation of Duhem's doctrine; Duhem's lack of direct support for the falsifiability thesis, a feature of Duhem's exposition acknowledged by Quinn, should warn us that the interpretation is dubious.

There is another text that can help decide this question. Shortly before his death, Duhem composed a detailed overview of all his scholarly works in a document supporting his candidacy for membership to the Académie des Sciences. The 'Notice sur les titres et travaux scientifiques de Pierre Duhem' was a work devoted to describing Duhem's master scientific idea which dominated his thought throughout his life, the creation of a science that would unite the principles of mechanics and those of thermodynamics: energetics. The salient feature of energetics was that 'it did not imitate the numerous mechanical theories proposed at the time by the physicists [*i.e.* Maxwell's theories]; it did not substitute the hidden movements of hypothetical bodies for the observable properties that instruments measured' (Duhem [1917], p. 74). What energetics offered was a formal theory, with the character of a logical system, which instead of reducing physical qualities in the manner of mechanistic theories, limited itself to marking by means of a numerical scale the various intensities of such qualities. Duhem's critique of mechanistic theories (or the method of Cartesians and atomists) was that they are not autonomous:

The physicist who wishes to follow them can no longer use the methods proper to physics exclusively [. . .]; he is carried into the domain of cosmology. He no longer has the right to shut his ears to what metaphysics wishes to teach him with respect to the real nature of matter. Hence physics becomes dependent on metaphysics and subject to the fluctuations of its doctrines. Thus the theories constructed according to the Cartesian or atomist method are condemned to infinite multiplicity as well as perpetual new beginnings; they appear far from being able to insure the general consent and continuous progress of science (*ibid.* [1917], p. 152; *cf.* also [1892] and [1954], pt. I, chap. i).

Duhem also contrasts the methodology of energetics with that of another great historical scientific methodology—the inductive method (or the Newtonian method)—which rejects any hypotheses about imperceptible bodies and hidden movements, and admits only general laws known by induction from observation. It is with respect to this contrast that the Duhemian thesis makes its first appearance. Duhem accuses the Newtonian method of being unmanageable ([1917], p. 152; *cf.* also [1894] and [1954], pt. II, chap. vi); he asserts that a science can only follow the Newtonian method when its means of knowing are those of common sense: 'Induction can no longer be practiced in this way when science no longer observes facts directly, but substitutes for them measurements given by instruments, of

magnitudes that only a mathematical theory has defined' ([1917], p. 153). Duhem clearly indicates that some sciences can use the Newtonian method, derive their principles by induction from observation and disconfirm them as tests warrant. What Duhem thinks is wrong with inductivism is that it cannot be applied to all sciences.

According to Duhem, physics as a science has reached a stage in which the observational consequences of its theoretical hypotheses need to be interpreted by another chunk of theory; as a result, no theoretical hypothesis by itself has any observational consequences: 'a physics experiment is not simply the observation of a phenomenon. . . . It is the precise observation of a group of phenomena, accompanied by the *interpretation* of these phenomena; this interpretation substitutes for the concrete given, actually gathered by observation, some abstract and symbolic representations which correspond to the given by virtue of the physical theories admitted by the observer' ([1917], p. 153; cf. also [1954], p. 147). This is the separability thesis. Duhem's reason for it rests on his empirical thesis about a peculiar feature of experiments in physics. A physics experiment might require the measurement of the electrical resistance of a coil; another might require the measurement of the volume and temperature of a gas. These measurements cannot be observed directly; they require sophisticated instruments that rely upon other theories for their construction and operation, and for the interpretation of their results ([1894] and [1954], pt. II, chap. iv).

Having asserted the separability thesis, Duhem then states that there are consequences to be derived from this thesis, and proceeds to assert the falsifiability thesis: 'The physicist can never submit an isolated [theoretical] hypothesis to the controls of experiment, but only a whole set of hypotheses; when the experiment disagrees with his prediction, he learns that at least one of the hypotheses that constitutes the set is erroneous and must be modified' ([1917], p. 153 and [1954], p. 187). It is clear that Duhem is claiming that the falsifiability thesis is a result of the separability thesis, and that the separability thesis is an empirical thesis depending upon factors that do not govern all sciences. This reading is also supported by the fact that Duhem claims, in *La Théorie physique*, that he is imposing narrow limits on his inquiry, and is restricting his generalizations to physics ([1954], p. 3); moreover, when he first announces the separability thesis, he announces it as a principle about what physicists do, whose consequences will be developed in the remainder of the book ([1954], p. 147). One should note that this interpretation of Duhem's thesis, that it is an empirical thesis whose scope is restricted to physical science, a thesis aimed against inductivism (Newtonianism), does not fall prey to a paradox that lurks about in these matters. It would be paradoxical if Duhem were to hold that he has empirically refuted inductivism using a thesis that there can be no empirical refutations; Duhem can think that he has refuted inductivism empirically because he thinks that inductivism does not account for what physicists do (and perhaps it fails to account only for what physicists do).

3 SOME CONSEQUENCES OF FAILING TO UNDERSTAND DUHEM'S THESIS

Failure to understand that Duhem considers the falsifiability thesis as a consequence of the separability thesis can lead to confusions about what would count as a refutation of Duhem's position. For example, Popper argues against the Duhem thesis that (1) there is no reason why a counter-example to an axiomatised theoretical system may not be found to satisfy all of one's axioms except for one whose independence would thereby be established ([1963], pp. 238–9); (2) we can make use of our background knowledge in the search for a counter-example, and refute one or another theory rather than our background knowledge ([1963], pp. 239–40);¹ and (3) some scientific behaviour cannot be explained by one who adopts Duhem's view of scientific theorising ([1963], pp. 242–4; this is Koertge's main criticism—*cf.* [1978], p. 259).

It is clear that Popper's first argument fails against Duhem, since the Duhem thesis does not concern axiomatised theoretical systems, but whole theories, including the chunk of theory connecting the theoretical hypothesis to observation. It should be recalled that Duhem has formulated his thesis as a response to inductivism, and does not think it applicable to such methodologies as Cartesianism, in which the aim is to produce internally consistent axiomatised theoretical systems, which are externally consistent with a specific metaphysics. As long as Popper's argument attempts to establish the independence of an axiom, using a model to be constructed as a counter-example, the argument is not relevant to Duhem's point, given that Duhem's point is restricted to experimental physics. So Duhem may very well agree with Popper's statement, but disagree with it as a criticism.

If one continues to keep Duhem's objectives clear, he would see that Popper's second and third arguments must also fail. Again, Duhem announces his separability thesis against the view that inductivism is the only scientific methodology. He first establishes that it is false that all scientific statements confront observation directly, by arguing for the separability thesis that some scientific hypotheses—those of theoretical physics—are such that, by themselves, they cannot confront observation directly. Duhem does not claim that no statement of science *qua* science ever confronts observation directly. He merely claims that if the theoretical hypothesis requires other interpretive statements in order to confront experience (as hypotheses of physics apparently do), then observation is not decisive by itself (that is, the falsifiability thesis results). Duhem does not claim that scientists cannot decide such cases. He merely claims that such

¹ In some respects, Clark Glymour's criticism of Duhem is just a reworking of this part of Popper's criticism; *cf.* Clark Glymour, [1975] or chap. 5 of [1980]. It should be noted that Quinn's objection (see above) is also an extension of this criticism—if we can verify a hypothesis, we can use it to refute some portion of theory.

disconfirmations (or confirmations) are not conclusive. For some statements of science, background knowledge can be used to refute one or another theory; for the hypotheses of theoretical physics, if the background knowledge must be used in order to interpret the statement so that it may confront observation, then the background knowledge cannot escape the negative results of the observation. In that case, a scientist may still hold on to the background knowledge, and disconfirm the theory, but the disconfirmation would not be conclusive.

Extensions of Popper's criticisms are numerous; it is often difficult to know against what the criticism is directed. For instance, Noretta Koertge begins her article, 'Towards a New Theory of Scientific Inquiry', in which the Duhemian problem is discussed and 'solved', with some remarks about common sense (as it is displayed in *Zen and the Art of Motorcycle Maintenance*, i.e., in case of ignition failure, test the spark plugs before dismantling the carburettor—[1978], p. 253; cf. pp. 255–67 for the solution to the Duhemian problem). Koertge intends 'to begin to construct a theory of scientific inquiry that is at least as sophisticated and right-minded as that found in *Zen and the Art of Motorcycle Maintenance*' (*ibid.*, [1978]). But Koertge does not really discuss and solve the Duhemian thesis; at one level she accepts it, and at another she merely denies it. Koertge accepts that it is sometimes possible that a scientist confronted with a negative experimental result would reject the auxiliary assumptions of the test instead of the theory itself (that would be to accept Duhem's thesis). However, she thinks that there are decision-theoretical reasons for proceeding one way or another. Koertge acts as if one can always separate one's options neatly. But this is what Duhem denies. The foundation of the Duhem thesis is the impossibility to separate hypotheses for testing. Duhem does not deny that one can, at times, have good reasons for proceeding one way rather than another; he denies that these reasons can be conclusive. Duhem has even taken the trouble to reject the metaphors that would liken the physicist with the motorcycle repairman and his machine:

Physics is not a machine that can be taken apart; one cannot try each piece in isolation and wait, in order to adjust it, until its solidity has been checked carefully. Physical science is a system that must be taken as a whole; it is an organism in which one part cannot be made to function without the more remote parts coming into play, some more than others, but all to some degree. If some discomfort, some illness is felt in the functioning, the physicist will have to guess, through the effect produced on the whole system, which organ needs to be remedied or modified, without being able to isolate this organ and examine it apart. The watchmaker to whom one gives a watch that does not function separates all the wheels and examines them one by one until he finds the one which is defective or broken. The doctor to whom a patient appears cannot dissect him in order to establish his diagnosis; he has to guess at the seat and cause of the ailment solely by inspecting disorders affecting the whole body. The physicist concerned with remedying a defective theory resembles the doctor and not the watchmaker ([1954], pp. 187–8).

4 FINAL COMMENTS ON THE DUHEM THESIS

It would seem that Duhem's thesis is so formulated that it escapes refutation by contemporary criticism (at least the criticism examined above). Duhem's thesis is sufficiently different from the Duhem–Quine thesis that it is not open to the charge that it is a *non-sequitur* to think that from separability and falsifiability we can conclude that scientists can hold any hypothesis come what may. Moreover, Duhem's primary thesis is separability, which he thinks is established empirically, and from which the falsifiability thesis derives. Duhem does not establish the separability thesis for all sciences, but merely for those sciences (theoretical physics, being the only example given) that require an interpretive chunk of theory in order for their theoretical hypotheses to confront observation. This engenders another distinction between Duhem's thesis and the Duhem–Quine thesis. Duhem thinks that theoretical physics has reached a stage such that its hypotheses do not have observational consequences; since this is an empirical thesis, Duhem would be willing to go back down from the universal thesis to a more modest thesis, if evidence can be presented against the universal thesis. The same cannot be said about the Duhem–Quine thesis. Given that the Duhem–Quine thesis concerns all statements and all evidence, it is not clear what can count as evidence against it such that its proponents would be willing to modify their views. In any case, Duhem's thesis is restricted both in the strength and in the scope of its conclusion. The separability thesis is directed solely against Newtonian inductivism; Duhem does not think that the argument can succeed against what he calls mechanistic theories (Cartesianism or atomism), although he thinks he has another argument against these theories—the great consensus in science, which allegedly could not be achieved if scientific theories were not independent from particular metaphysics (for which there is obviously no consensus). If one pays attention to these features of Duhem's argument one need not worry about the alleged contemporary refutations of Duhem's thesis. Of course, that is not to say that none of the attempted refutations have any merit—it is clear, for instance, that Grünbaum's argument succeeds against the broader interpretations of the Duhem thesis. Philosophers of science may legitimately wish to refute claims made for the Duhem thesis, but they should be careful to deny only that which is within the scope of their arguments.

Duhem is to be praised for setting forth an interesting thesis which attempts to navigate between the Scylla of 'reductionism' and the Charybdis of relativism.¹

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¹ I have profited from comments and suggestions by members of Virginia Polytechnic Institute and State University's Department of Philosophy and Center for the Study of Science in Society.

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