DONALD GILLIES

The Duhem Thesis and the Quine Thesis

In current writings on the philosophy of science, reference is often made to what is called 'the Duhem-Quine thesis'. Really, however, this is something of a misnomer; for, as we shall see, the Duhem thesis differs in many important respects from the Quine thesis. In this chapter I will expound the two theses in turn and explain how they differ. I will conclude the chapter by suggesting that the phrase 'the Duhem-Quine thesis' could be used to refer to a thesis which combines elements from both the Duhem thesis and the Quine thesis. . . .

1 | Preliminary Exposition of the Thesis. The Impossibility of a Crucial Experiment

Of Duhem's many significant contributions to the philosophy of science, perhaps the most important was his formulation of what I will call the *Duhem thesis*. With his usual clarity and incisiveness, Duhem states this thesis as a section heading thus:

> An Experiment in Physics Can Never Condemn an Isolated Hypothesis but Only a Whole Theoretical Group (1904–5, p. 183 [260]).*

Later in this section he expounds the thesis as follows:

> In sum, the physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses; when the experiment is in dis-

FROM Donald Gillies, *Philosophy of Science in the Twentieth Century* (Oxford: Blackwell Publishers, 1993), 98–116.

* All page references in square brackets are to the excerpts from Duhem and Quine in this volume.

agreement 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. (p. 187 [263]).

In order to discuss the Duhem thesis, it will be useful to introduce the notion of an *observation statement*. . . . Let us take an observation statement to be a statement which can provisionally be agreed to be either true or false on the basis of observation and experiment.

According to the Duhem thesis, an isolated hypothesis in physics (h, say) can never be falsified by an observation statement, O. As a generalisation covering all the hypotheses of physics, this is somewhat doubtful. Physics does appear to contain some falsifiable hypotheses. Consider, for example, Kepler's first law that planets move in ellipses with the Sun at one focus. Suppose that we observe a large number of positions of a given planet and that these do not lie on an ellipse of the requisite kind. We have then surely falsified Kepler's first law. The schema of falsification can be written, where 'not-h' is short for 'It is not the case that h':

If h, then O, but not-O, therefore not-h. (1)

This uses a logical law called *modus tollens*.

However, the Duhem thesis does apply to some hypotheses. . . . Consider, for example, Newton's first law of motion (T_1, say). . . . We cannot find an O such that schema (1) above holds when we substitute T_1 for h.*

Newton's full theory (T, say) consisted of three laws of motion (T_1, T_2, and T_3) and the law of gravity, T_4. So T was a conjunction of these four laws ($T = T_1 \& T_2 \& T_3 \& T_4$). Even from T by itself, however, we cannot derive any observable consequences regarding the solar system. To do so, we need to add to T a number of auxiliary hypotheses: for example, that no other forces but gravitational ones act on the planets, that the interplanetary attractions are small compared with those between the Sun and the planets, that the mass of the Sun is very much greater than that

* In the book from which this reading is excerpted, Gillies refers to an argument of Poincaré to support the claim that Newton's first law of motion is not falsifiable. Newton's first law has the form of a conditional statement: if there is no external force acting on a body, then the velocity of the body will not change. Thus, to falsify the law requires a body that accelerates even though it is free from any net external force. Poincaré argues that if we find such an apparent counterexample, we can always deny that the body is genuinely free from a net external force by attributing the acceleration to forces exerted by as-yet-undetected invisible molecules. In this way, the law will be protected from refutation. See Henri Poincaré, *Science and Hypothesis*, trans. W. J. Greenstreet (New York: Dover, 1952), 95–96; originally published in French (1902).

of the planets, and so on. Let us call the conjunction of such auxiliary hypotheses which are appropriate in a given case A. We now have the schema:

$$\text{If } T_1 \ \&\ T_2 \ \&\ T_3 \ \&\ T_4 \ \&\ A, \text{ then O, but not-O,} \\ \text{therefore not-}(T_1 \ \&\ T_2 \ \&\ T_3 \ \&\ T_4 \ \&\ A). \qquad (2)$$

Moreover, from not-(T_1 & T_2 & T_3 & T_4 & A) it follows that at least one of the set (T_1, T_2, T_3, T_4, or A) is false, but we cannot say which one.

As the history of science shows, it is often a very real problem in scientific research to decide which one of a group of hypotheses should be changed. Consider, for example, Adams and Leverrier's discovery of Neptune in 1846. From Newton's theory T together with auxiliary hypotheses, astronomers were able to calculate the theoretical orbit of Uranus (the most distant planet then known). This theoretical orbit did not agree with the observed orbit. This meant that either T or one of the auxiliary hypotheses was false. Adams and Leverrier conjectured that the auxiliary hypothesis concerning the number of planets was in error. They postulated a new planet Neptune beyond Uranus, and calculated the mass and position it would have to have to cause the observed perturbations in Uranus's orbit. Neptune was duly observed on 23 September 1846 only 52' away from the predicted position.[3]

This part of the story is quite well known, but there were some subsequent events which are also relevant to the Duhem thesis. Another difficulty which occupied astronomers at the time concerned the anomalous motion of the perihelion of Mercury, which was found to advance slightly faster than it should do according to standard theory. Leverrier tried the same approach that had proved successful in the case of the Uranus anomaly. He postulated a planet Vulcan nearer to the Sun than Mercury, with a mass, orbit, and so forth which would explain the advance in Mercury's perihelion. However, no such planet could be found.

The discrepancy here is very small. Newcomb in 1898 gave its value as 41.24" ± 2.09" per century; that is, less than an eightieth part of a degree per century. However, this tiny anomaly was explained with great success by the general theory of relativity (T'), which Einstein proposed in 1915 as a replacement for Newton's theory, T. The value of the anomalous advance of the perihelion of Mercury which followed from the general theory of relativity was 42.89" per century—a figure well within the bounds set by Newcomb. We see that, although the Uranus anomaly and the Mercury anomaly were *prima facie* very similar, success was obtained in one case by altering an auxiliary hypothesis, in the other by altering the main theory.

In the next section, Duhem goes on to draw an important consequence from his thesis. This section is in fact headed 'A "Crucial Experiment" Is Impossible in Physics' (1904–5, p. 188 [264]). Duhem uses the term *crucial experiment* in something like the sense given by Bacon in the *Novum Organum* to his 'fact of the cross'. He formulates this notion of crucial experiment as follows: 'Enumerate all the hypotheses that can be made to account for this group of phenomena; then, by experimental contradiction eliminate all except one: the latter will no longer be a hypothesis, but will become a certainty' (ibid.). However, there is an obvious objection to crucial experiments in this strong sense: namely, that we can never be sure that we have listed all the hypotheses capable of explaining a group of phenomena. Duhem makes this point as follows:

> Experimental contradiction does not have the power to transform a physical hypothesis into an indisputable truth; in order to confer this power on it, it would be necessary to enumerate completely the various hypotheses which may cover a determinate group of phenomena; but the physicist is never sure he has exhausted all the imaginable assumptions. (p. 190 [266])

In view of this difficulty, it seems desirable to adopt a rather weaker sense of crucial experiment, which may be defined as follows. Suppose we have two competing theories T_1 and T_2. An experiment (E, say) is crucial between T_1 and T_2, if T_1 predicts that E will give the result O and T_2 predicts that E will give the result not-O. If we perform E, and O occurs, then T_2 is eliminated. If we perform E, and not-O occurs, then T_1 is eliminated. In any event, one of the two theories will be eliminated by E, which is thus crucial for deciding between them. It does not of course follow that the successful theory is necessarily true, because there may be some, as yet unthought of, theory, T_3, which differs from T_1 and T_2 but explains the whole matter much more satisfactorily.

Duhem's point is that if T_1 and T_2 are such that his thesis applies to them, then we cannot derive O from T_1 but only from T_1 and A, where A is a conjunction of auxiliary assumptions. So, if not-O is the result of the experiment, this does not demonstrate beyond doubt that T_1 should be eliminated in favour of T_2. It could be that one of the auxiliary hypotheses in A is at fault.

Duhem illustrates this by what is perhaps the most famous example of an alleged crucial experiment in the history of science: Foucault's experiment, which was designed to decide between the wave theory and the particle theory of light. The wave theory of light predicted that the velocity of light in water should be less than its velocity in air, whereas the particle theory predicted that the velocity of light in water should be greater than its velocity in air. Foucault devised a method for measuring the velocity of light in water, and found that it was actually less than the velocity of light in air. Here, then, we seem to have a crucial experiment which decides definitely in favour of the wave theory of light. Indeed, some of Foucault's contemporaries, notably Arago, did maintain that Foucault's experiment was a crucial experiment in just this sense.

Duhem pointed out, however, that to derive from the particle theory that the velocity of light in water is greater than its velocity in air, we need, not just the assumption that light consists of particles (the fundamental hypothesis of the particle theory), but many auxiliary assumptions as well. The particle theory could always be saved by altering some of these auxiliary assumptions. As Duhem puts it: 'For it is not between two hypotheses, the emission and wave hypotheses, that Foucault's experiment judges trenchantly; it decides rather between two sets of theories each of which has to be taken as a whole, i.e. between two entire systems, Newton's optics and Huygens' optics' (p. 189 [265]). So, according to Duhem, Foucault's experiment is not a crucial experiment in a strictly logical sense. Yet, as we shall see in the next section, there is another, weaker sense in which the experiment is crucial, even for Duhem.

2 | Duhem's Criticisms of Conventionalism. His Theory of Good Sense *(le bon sens)*

Duhem is sometimes classified as a conventionalist as regards his philosophy of science, but he is certainly not a conventionalist in the sense of Le Roy and Poincaré. Indeed, he devotes two sections of his *Aim and Structure of Physical Theory* to criticising these thinkers very clearly and explicitly. He formulates their conventionalist position as follows: 'Certain fundamental hypotheses of physical theory cannot be contradicted by any experiment, because they constitute in reality *definitions*, and because certain expressions in the physicist's usage take their meaning only through them' (p. 209 [271]).

Duhem objects strongly to Poincaré's claim that the principles of Newtonian mechanics will never be given up, because they are the simplest conventions available and cannot be contradicted by experiment. According to Duhem, the study of the history of science makes any such claim highly dubious:

> The history of science should show that it would be very imprudent for us to say concerning a hypothesis commonly accepted today: 'We are certain that we shall never be led to abandon it because of a new experiment, no matter how precise it is.' Yet M. Poincaré does not hesitate to make this assertion concerning the principles of mechanics. (p. 212 [274]; I have here slightly altered the standard English translation in the interests of clarity.)

Poincaré's mistake, according to Duhem, was to take each principle of mechanics singly and in isolation. It is indeed true that when a principle of mechanics—for example, Newton's first law of motion—is taken in this fashion, it cannot be either confirmed or refuted by experience. However, by adding other hypotheses to any such principle, we get a group of hypotheses which can be compared with experience. Moreover, if the group in question is contradicted by the results of experiment and observation, it is possible to change any of the hypotheses of the group. We cannot say with Poincaré that certain fundamental hypotheses, because they are appropriately simple conventions, are above question and can never be altered. This is how Duhem puts the matter:

> It would be absurd to wish to subject certain principles of mechanics to *direct* experimental test; . . .
>
> Does it follow that these hypotheses placed beyond the reach of direct experimental refutation have nothing more to fear from experiment? That they are guaranteed to remain immutable no matter what discoveries observation has in store for us? To pretend so would be a serious error.
>
> Taken in isolation these different hypotheses have no experimental meaning; there can be no question of either confirming or contradicting them by experiment. But these hypotheses enter as essential foundations into the construction of certain theories of rational mechanics . . . these theories . . . are schematisms intended essentially to be compared with facts.
>
> Now this comparison might some day very well show us that one of our representations is ill-adjusted to the realities it should picture, that the corrections which come and complicate our schematism do not produce sufficient concordance between this schematism and the facts, that the theory accepted for a long time without dispute should be rejected, and that an entirely different theory should be constructed on entirely different or new hypotheses. On that day some one of our hypotheses, which taken in isolation defied direct experimental refutation, will crumble with the system it supported under the weight of the contradictions inflicted by reality on the consequences of this system taken as a whole. (pp. 215–16 [276–77])

Thus Duhem's position seems to me more accurately described as *modified falsification*, rather than *conventionalism*. Duhem claims that some hypotheses of physics, when taken in isolation, can defy direct experimental refutation. He is thus not a strict falsificationist. On the other hand, he denies that such a hypothesis is immune from revision in the light of experimental evidence. A hypothesis of this kind may be tested indirectly if it forms part of a system of hypotheses which can be compared with experiment and observation. Further, such a hypothesis may on some occasion 'crumble with the system it supported under the weight of contradictions inflicted by reality'. Duhem does not deny that 'among the theoretical elements . . . there is always a certain number which the physicists of a certain epoch agree in accepting without test and which they regard as beyond dispute' (p. 211 [273]). However, he is very concerned to warn scientists against adopting too dogmatic an attitude towards any of their assumptions. His point is that, in the face of recalcitrant experience, the best way forward may be to alter one of the most entrenched assumptions. As he says:

> Indeed, we must really guard ourselves against believing forever warranted those hypotheses which have become universally adopted conventions, and whose certainty seems to break through experimental contradiction by throwing the latter back on more doubtful assumptions. The history of physics shows us that very often the human mind has been led to overthrow such principles completely, though they have been regarded by common consent for centuries as inviolable axioms, and to rebuild its physical theories on new hypotheses. (p. 212 [273])

Duhem gives as an example the principle that light travels in a straight line. This was accepted as correct for hundreds—indeed, thousands—of years, but was eventually modified to explain certain diffraction effects.

Duhem even cites Newton's law of gravity as a law which is only provisional and may be changed in future. Unfortunately this passage has been accidentally omitted from the English edition of the *Aim and Structure of Physical Theory*. It is here translated from the French edition:

> Of all the laws of physics, the one best verified by its innumerable consequences is surely the law of universal gravity; the most precise observations on the movements of the stars have not been able up to now to show it to be faulty. Is it, for all that, a definitive law? It is not, but a provisional law which has to be modified and completed unceasingly to make it accord with experience. (p. 267)

The episode of the anomalous motion of the perihelion of Mercury fits Duhem's analysis perfectly. It would surely have seemed reasonable to explain such a small discrepancy between Newton's theory and observation by altering some auxiliary assumption. In fact, however, the anomaly was only explained satisfactorily when Newton's whole theory of gravity was replaced by Einstein's general theory of relativity. Indeed, from a logical point of view, Duhem's philosophy of science can be seen as offering support to the Einsteinian revolution in physics. It therefore comes as a surprise to discover that Duhem rejected Einstein's theory of relativity in the most violent terms. In his 1915 booklet *La Science allemande* ('German Science'), Duhem argues that Einstein's theory of relativity must be considered as an aberration due to the lack of sound judgement of the German mind and its disrespect for reality. Admittedly, this booklet was written at a time when bitter nationalistic feelings were being generated by the First World War. Indeed, it belongs to a genre known as 'war literature', and is actually a relatively mild example of this unfortunate species of writing. All the same, it is clear that Duhem did reject Einstein's theory of relativity in no uncertain terms.

So, as already observed, we find in both Duhem and Poincaré a contradiction between their philosophical views and their scientific practice. Duhem was led by philosophical considerations to the conclusion that Newtonian mechanics is provisional and may be altered in future; yet he repudiated the new Einsteinian mechanics.[2] Conversely, Poincaré suggested in his philosophical writings of 1902 that the principles of Newtonian mechanics were conventions so simple that they would never be given up; yet, only two years later, in 1904, he decided that Newtonian mechanics needed to be changed, and started work on the development of a new mechanics. Some light is thrown on these strange contradictions by one further element in the Duhem thesis which we have still to discuss. This is Duhem's theory of good sense *(le bon sens)*.

Let us take the typical situation envisioned by the Duhem thesis. From a group of hypotheses, $\{h_1 \ldots h_n\}$, say, a scientist has deduced O. Experiment or observation then shows that O is false. It follows that at least one of $\{h_1 \ldots h_n\}$ is false. But which one or ones are false? Which hypothesis or hypotheses should the scientist try to change in order to re-establish the agreement between theory and experience? Duhem states quite categorically that logic by itself cannot help the scientist. As far as pure logic is concerned, the choice between the various hypotheses is entirely open. The scientist in reaching his decision must be guided by what Duhem calls 'good sense' *(le bon sens)*:

> Pure logic is not the only rule for our judgements; certain opinions which do not fall under the hammer of the principle of contradiction are in any case perfectly unreasonable. These motives which do not proceed from logic and yet direct our choices, these 'reasons which reason does not know' and which speak to the ample 'mind of finesse' but not to the 'geometric mind,' constitute what is appropriately called good sense. (1904–5, p. 217 [277–78])

Duhem imagines two scientists who, when faced with the experimental contradiction of a group of hypotheses, adopt different strategies. Scientist A alters a fundamental theory in the group, whereas scientist B alters some of the auxiliary assumptions. Both strategies are logically possible, and only good sense can enable us to decide between the two scientists. Thus, in the dispute between the particle theory of light and the wave theory of light, Biot, by a continual alteration and addition of auxiliary assumptions, tenaciously and ingeniously defended the particle theory, whereas Fresnel constantly devised new experiments favouring the wave theory. In the end, however, the dispute was resolved.

> After Foucault's experiment had shown that light travelled faster in air than in water, Biot gave up supporting the emission hypothesis; strictly, pure logic

> would not have compelled him to give it up, for Foucault's experiment was *not* the crucial experiment that Arago thought he saw in it. but by resisting wave optics for a longer time Biot would have been lacking in good sense. (p. 218, [278])

This passage in effect qualifies some of Duhem's earlier remarks about crucial experiments. Let us take two theories, T_1 and T_2, which are both subject to the Duhem thesis; that is, which cannot be tested in isolation but only by adjoining further assumptions. In a strictly logical sense, there cannot be a crucial experiment which decides between T_1 and T_2. The good sense of the scientific community can, however, lead it to judge that a particular experiment, such as Foucault's experiment, is in practice crucial in deciding the scientific controversy in favour of one of the two contending theories.

In his 1991 book (particularly chapters 4–6), Martin argues that 'lifelong meditation on certain texts of Pascal shaped many of the most important and difficult features of Duhem's thought' (p. 101). In particular, Duhem's theory of good sense *(le bon sens)* was derived in part from Pascal. Indeed, in the passage introducing *le bon sens*, Duhem quotes part of Pascal's famous saying that the heart has its reasons which reason knows nothing of.[3]

Although Duhem was undoubtedly influenced by Pascal, it is possible to suggest factors of a more personal and psychological nature which may have led him to his theory of scientific good sense. As his writings on philosophy of science show, Duhem was a man of outstanding logical ability; yet, as a physicist, he was a failure. In almost every scientific controversy in which he was involved, he chose the wrong side, rejecting those theories such as atomism, Maxwell's electrodynamics, and Einstein's theory of relativity which were to prove successful and lead to scientific progress. Although Duhem stubbornly defended his erroneous scientific opinions, he must have known in his heart of hearts that he was not proving to be a successful scientist. Yet he must also have been aware of his own exceptional logical powers. This situation could only be explained by supposing that something in addition to pure logic was needed in order to become a successful scientist. Here, then, we have a possible psychological origin of Duhem's theory of scientific good sense: namely, that Duhem saw that good sense is necessary for a scientist precisely because he himself was lacking in good sense. Duhem's rejection of a new theory which agreed so well with his own philosophy of science (that is, Einstein's theory of relativity) is just another instance of that lack of good sense which unfortunately characterized Duhem's scientific career.

Poincaré, by contrast, was one of the great physicists of his generation, and was amply endowed with the scientific good sense which Duhem lacked. The contrast between the two men is particularly evident in their respective discussions of electrodynamics. As we have already remarked, Duhem attacked Maxwell's theory harshly, and advocated the ideas of Helmholtz. Poincaré devotes a chapter (the thirteenth) of his 1902 book to electrodynamics. He begins (pp. 225–38) by discussing the theories of Ampère and Helmholtz and by mentioning the difficulties which he finds in these theories. Then, on p. 239, he introduces Maxwell's theory with the words: 'Such were the difficulties raised by the current theories, when Maxwell with a stroke of the pen caused them to vanish.' Subsequent developments completely endorsed Poincaré's support for Maxwell, while Helmholtz's ideas on electrodynamics, so strenuously advocated by Duhem, are now remembered only by a few erudite historians of science. It was Poincaré's scientific good sense which led him, contrary to the principles of his own conventionalist philosophy of science of 1902, to a modification of Newtonian mechanics.

Duhem's theory of good sense seems to me correct, but, at the same time, more in the nature of a problem, or a starting-point for further analysis, than of a final solution to the difficulty with which it deals. What factors contribute to forming scientific good sense? Why are some highly intelligent individuals like Duhem lacking in good sense? These are important questions. . . . In the next section, however, I will turn to a consideration of the Quine thesis.

3 | The Quine Thesis

In his famous 1951 article, 'Two Dogmas of Empiricism', Quine puts forward, with a reference to Duhem, a thesis which is related to Duhem's. Nonetheless, it seems to me that Quine's thesis is sufficiently different from Duhem's to make the conflation of the two intellectually unsatisfactory.[4] I will next briefly describe the Quine thesis,[5] and explain how it differs from the Duhem thesis.

The first obvious difference between Quine and Duhem is that Quine develops his views in the context of a discussion about whether a distinction can be drawn between analytic and synthetic statements, whereas Duhem does not even mention (let alone discuss) the analytic/synthetic problem.

[There are] two ways of defining an analytic statement. The first [is] due to Kant, who actually introduced the analytic/synthetic distinction. According to Kant, a statement is analytic if its predicate is contained in its subject. This formulation presupposes an Aristotelian analysis of statements into subject and predicate. It is not surprising that Frege, who rejected Aristotelian logic and introduced modern logic, should have proposed a new way of defining an analytic statement. Frege defines an analytic statement as one which is reducible to a truth of logic by means of explicit definitions. These two ways of defining an analytic statement are both illustrated by the standard example of an analytic statement, namely 'All bachelors are unmarried'. But Quine defines analytic statement in yet a third way. He writes critically of 'a belief in some fundamental cleavage between truths which are *analytic*, or grounded in meanings independently of matters of fact, and truths which are *synthetic*, or grounded in fact' (1951, p. 20 [280]). In effect, Quine is here taking a sentence to be analytic if it is true in virtue of the meanings of the words it contains. This is the definition of 'analytic' which is adopted by most modern philosophers interested in the question. Once again it is admirably illustrated by the standard example: S = 'All bachelors are unmarried'. Someone who knows the meanings of 'all', 'bachelors', 'are', and 'unmarried' will at once recognize that S is true, without having to make any empirical investigations into matters of fact. Thus S is analytic.

All this seems very convincing; yet Quine denies that the distinction between analytic and synthetic is a valid one. He writes:

> It is obvious that truth in general depends on both language and extralinguistic fact. The statement 'Brutus killed Caesar' would be false if the world had been different in certain ways, but it would also be false if the word 'killed' happened rather to have the sense of 'begat'. Thus one is tempted to suppose in general that the truth of a statement is somehow analyzable into a linguistic component and a factual component. Given this supposition, it next seems reasonable that in some statements the factual component should be null; and these are the analytic statements. But, for all its a priori reasonableness, a boundary between analytic and synthetic statements simply has not been drawn. That there is such a distinction to be drawn at all is an unempirical dogma of empiricists, a metaphysical article of faith. (1951, pp. 36–7 [292])

The empiricists to whom Quine refers are, of course, the empiricists of the Vienna Circle, especially Carnap. . . . Their particular brand of empiricism (logical empiricism) did indeed involve drawing a distinction between analytic and synthetic statements. However, support for the distinction is not confined to some members of the empiricist camp. Kantians too support the distinction, which was indeed introduced by Kant himself.

But what has all this to do with the issues involving Duhem and conventionalism, which we have been discussing? We can begin to build a bridge by observing that the meanings given to sounds and inscriptions are determined purely by social convention. Indeed, the social conventions differ from one language to another. So if a sentence is true in virtue of the meanings of the words it contains (that is, is analytic), it is *a fortiori* true by convention. Thus if a law is analytic, it is true by convention. The converse may not hold, since it is conceivable that a law might be rendered true by a set of conventions which include not just linguistic conventions concerning the meanings of words but also perhaps conventions connected with measuring procedures.

Duhem used his thesis against the claim that a particular scientific law was true by convention. It is now obvious that exactly the same argument could be used against the claim that the law is analytic. Indeed, Quine does argue against the analytic/synthetic distinction along just these lines.[6]

But to carry his argument through, Quine makes a claim (the Quine thesis) which is much stronger than the Duhem thesis. The key difference between the two theses is clearly expressed by Vuillemin as follows: 'Duhem's thesis ("D-thesis") has a limited and special scope not covering the field of physiology, for Claude Bernard's experiments are explicitly acknowledged as crucial. Quine's thesis ("Q-thesis") embraces the whole body of our knowledge' (1979, p. 599).

Duhem does indeed place explicit limitations on the scope of his thesis. He writes: 'The Experimental Testing of a Theory Does Not Have the Same Logical Simplicity in Physics as in Physiology' (1904–5, p. 180 [257]). He thinks that his thesis does not apply in physiology or in certain branches of chemistry, and defends it only for the hypotheses of physics. My own view is that Duhem is correct to limit the scope of his thesis, but wrong to identify its scope with that of a particular branch of science—namely, physics. There are in physics falsifiable laws—for example, Snell's law of refraction applied to glass—whereas physiology and chemistry no doubt contain hypotheses subject to the Duhem thesis. . . . For the moment, however, it is not of great importance where exact boundaries are drawn. The crucial point is that Duhem wanted to apply his thesis to some statements and not to others, whereas the Quine thesis is supposed to apply to any statement whatever.

This is closely connected with a second difference between the Duhem thesis and the Quine thesis. Duhem maintains that hypotheses in physics cannot be tested in isolation, but only as part of a group. However, his discussion makes clear that he places limits on the size of this 'group'. Quine, however, thinks that the group extends and ramifies until it includes the whole of human knowledge. Quine writes: 'The unit of empirical significance is the whole of science' (1951, p. 42 [296]); and again:

> The totality of our so-called knowledge or beliefs, from the most casual matters of geography and history to the profoundest laws of atomic physics or even of pure mathematics and logic, is a man-made fabric which impinges on experience only along the edges. Or, to change the figure, total science is like a field of force whose boundary conditions are experience. A conflict with experience at the periphery occasions readjustments in the interior of the field. . . . But the total field is so underdetermined by its boundary conditions, experience, that there is much latitude of choice as to what statements to reëvaluate in the light of any single contrary experience. No particular

experiences are linked with any particular statements in the interior of the field, except indirectly through considerations of equilibrium affecting the field as a whole. (pp. 42–3 [296])

The Quine thesis is stronger than the Duhem thesis, and, in my view, less plausible. Let us take, as a concrete example, one of the cases analysed earlier. Newton's first law cannot, taken in isolation, be compared with experience. Adams and Leverrier, however, used this law as one of a group of hypotheses from which they deduced conclusions about the orbit of Uranus. These conclusions disagreed with observation. Now the group of hypotheses used by Adams and Leverrier was, no doubt, fairly extensive, but it did not include the whole of science. Adams and Leverrier did not, for example, mention the assumption that bees collect nectar from flowers in order to make honey, although such an assumption might well have appeared in a contemporary scientific treatise dealing with a question in biology. We agree, then, with Quine that a single statement may not always be (to use his terminology) a 'unit of empirical significance'. But this does not mean that 'The unit of empirical significance is the whole of science' (1951, p. 42 [296]). A group of statements which falls considerably short of the whole of science may sometimes be a perfectly valid unit of empirical significance.

Another difference between Duhem and Quine is that Quine does not have a theory of scientific good sense. Let us take, for example, Quine's statement: 'Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system' (p. 43 [296–97]). It is easy to imagine how Duhem would have reacted to such an assertion when applied to a statement falling under his thesis. Duhem would have agreed that, from the point of view of *pure logic*, one can indeed hold a particular statement—for example, Newton's particle theory of light—to be true, come what may. However, someone who did so in certain evidential situations would be *lacking in good sense*, and indeed *perfectly unreasonable*.

Because Quine does not have a theory of good sense, he cannot give the Duhemian analysis which we have just sketched. Indeed, it is significant that his 1951 article, 'Two Dogmas of Empiricism', is reprinted in a collection entitled *From a Logical Point of View*. Where Quine does go beyond logic, it is towards pragmatism, though Quine's pragmatism is usually mentioned only in passing, rather than elaborated, as in the following passage: 'Each man is given a scientific heritage plus a continuing barrage of sensory stimulation, and the considerations which guide him in warping his scientific heritage to fit his continuing sensory promptings are, where rational, pragmatic' (p. 46 [299]).

Although the Duhem thesis is quite clearly distinct from the Quine thesis, it might still be possible—indeed, useful—to form a composite thesis containing some, but not all, elements from each of the two theses. The phrase *Duhem-Quine thesis* could then be validly used to denote this composite thesis. In the last section of this chapter, I will elaborate a suggestion along these lines.

4 | The Duhem-Quine Thesis

Let us say that the *holistic thesis* applies to a particular hypothesis if that hypothesis cannot be refuted by observation and experiment when taken in isolation, but only when it forms part of a theoretical group. The differences between the Duhem and Quine theses concern the range of hypotheses to which the holistic thesis is applied and the extent of the 'theoretical group' for a hypothesis to which the holistic thesis does apply. In discussing these differences, I have so far sided with Duhem against Quine. There is one point, however, on which I would like to defend Quine against Duhem. Quine, as we have seen, extends the holistic thesis to mathematics and logic. Duhem, however, thought that mathematics and logic had a character quite different from that of physics. Crowe (1990) gives an excellent general account and critique of Duhem's views on the history and philosophy of mathematics. I will here confine myself to a brief account of some views concerning geometry and logic which Duhem expounded in his late work *La Science allemande* ('German Science').

Duhem begins his treatment of geometry with the following remarks:

> Among the sciences of reasoning, arithmetic and geometry are the most simple and, consequently, the most completely finished; . . .
>
> What is the source of their axioms? They are taken, it is usually said, from common sense knowledge *(connaissance commune)*: that is to say that any man sane of mind is sure of their truth before having studied the science of which they will be the foundations. (1915, pp. 4–5)

Duhem agrees with this point of view. In fact, he holds what in 1915 was a very old-fashioned opinion, that the axioms of Euclid are established as true by common-sense knowledge *(connaissance commune)* or common sense *(le sens commun)* or intuitive knowledge *(connaissance intuitive)*. A proposition from which Euclid's fifth postulate can be deduced is that, given a geometrical figure (say a triangle), there exists another geometrical figure similar to it but of a different size. Duhem argues that the intuitions of palaeolithic hunters of reindeer were sufficient to establish the truth of this proposition. As he says:

> One can represent a plane figure by drawing, or a solid figure by sculpture, and the image can resemble the model perfectly, even though they have different sizes. This is a truth which was in no way doubted, in palaeolithic

times, by the hunters of reindeer on the banks of the Vézère. Now that figures can be similar without being equal, implies, as the geometric spirit demonstrates, the exact truth of Euclid's postulate. (pp. 115–16)

Naturally enough, this attitude to the foundations of geometry leads Duhem to criticize non-Euclidean geometry, and, in particular, Riemannian geometry. This is what he says:

Riemann's doctrine is a *rigorous algebra*, for all the theorems which it formulates are very precisely deduced from its basic postulates; so it satisfies the geometric spirit. It is not a *true geometry*, for, in putting forward its postulates, it is not concerned that their corollaries should agree at every point with the judgements, drawn from experience, which constitute our intuitive knowledge of space; it is therefore repugnant to common sense. (p. 118)

It is perhaps no accident that the non-Euclidean geometer cited by Duhem (namely, Riemann) was a German; for, as already remarked, *La Science allemande*, written in 1915, was an example of the war literature of the time, designed to denigrate the enemy nationality. Duhem attacks German scientists by claiming that, while they possess the geometric spirit *(l'esprit géométrique)*, their theories contradict common sense *(le sens commun)* or *l'esprit de finesse*, which is Duhem's new term for something like his old notion of good sense.

Given this general point of view, it is not surprising that we find Duhem condemning the theory of relativity. He speaks of 'the principle of relativity such as has been conceived by an Einstein, a Max Abraham, a Minkowski, a Laue' (p. 135). Forgetting the contributions of his own compatriot Poincaré, he denounces relativity as a typical aberration of the German mind. As he says:

The fact that the principle of relativity confounds all the intuitions of common sense, does not arouse against it the mistrust of the German physicists —quite the contrary! To accept it is, by that very fact, to overturn all the doctrines where space, time, movement were treated, all the theories of mechanics and physics; such a devastation has nothing about it which can displease German thought; on the ground which it will have cleared of the ancient doctrines, the geometric spirit of the Germans will devote itself with a happy heart to rebuilding a whole new physics of which the principle of relativity will be the foundation. If this new physics, disdainful of common sense, runs counter to all that observation and experience have allowed to be constructed in the domain of celestial and terrestrial mechanics, the purely deductive method will only be more proud of the inflexible rigour with which it will have followed to the end the ruinous consequences of its postulate. (p. 136)

The development and acceptance of non-Euclidean geometry and relativity have rendered Duhem's attempt to found geometry on common sense untenable. It is surely now more reasonable to extend the holistic thesis from physics to geometry and to say that, in the face of recalcitrant observations, we have the option of altering postulates of geometry as well as postulates of physics. This is, after all, precisely what Einstein did when he devised his general theory of relativity.

The picture is the same when we turn from geometry to logic. . . . Duhem [claimed] that 'There is a general method of deduction; Aristotle has formulated its laws for all time *(pour toujours)*' (p. 58). Yet by 1915 the new logic of Frege, Peano, and Russell had clearly superseded Aristotelian logic. Moreover, Brouwer had criticized some of the standard logical laws, and suggested his alternative intuitionistic approach. Quine writes: 'Revision even of the logical law of the excluded middle has been proposed as a means of simplifying quantum mechanics' (1951, p. 43 [297]). Admittedly the new 'quantum logic' has not proved very successful in resolving the paradoxes of microphysics; but there is no reason in principle why a change of this kind should not prove efficacious in some scientific context. In artificial intelligence, non-standard logics (for example, non-monotonic logics) are being devised in order to model particular forms of intelligent reasoning, and this programme has met with some success. Thus it seems reasonable to extend the holistic thesis to include logic as well and to allow the possibility of altering logical laws as well as scientific laws to explain recalcitrant observations.

I am now in a position to formulate what I will call the *Duhem-Quine thesis*, which combines what seem to me the best aspects of the Duhem thesis and the Quine thesis. It will be convenient to divide the statement in two parts.

- A The holistic thesis applies to any high-level (level 2) theoretical hypotheses, whether of physics or of other sciences, or even of mathematics and logic. (A incorporates ideas from the Quine thesis.)
- B The group of hypotheses under test in any given situation is in practice limited, and does not extend to the whole of human knowledge. Quine's claim that 'Any statement can be held to be true come what may, if we make drastic enough adjustments elsewhere in the system' (1951, p. 43 [296–97]) is true from a purely logical point of view; but scientific good sense concludes in many situations that it would be perfectly unreasonable to hold on to particular statements. (B obviously follows the Duhem thesis rather than the Quine thesis.)

. . . The thesis seems to me to be both true and important. . . .

Notes

1. 1 degree = 60', and 1' = 60". So 52' is slightly less than a degree.

2. Einstein may have been influenced by Duhem, however, as is suggested by Howard in his interesting 1990 article. Howard shows that Einstein was on very friendly terms with Friedrich Adler, who prepared the first German translation of the *Aim and Structure of Physical Theory*, which appeared in 1908. From the autumn of 1909, Einstein and his wife rented an apartment in Zurich just immediately upstairs from the Adlers, and Einstein and Adler would meet frequently to discuss philosophy and physics. So probably Einstein had read *Aim and Structure* by the end of 1909 at the latest.

3. Or rather, misquotes. Duhem writes: 'raisons que la raison ne connaît pas' (1904–5, French edn, p. 330), whereas Pascal's original *pensée* was 'Le coeur a ses raisons que la raison ne connaît point.' Giving quotations which are slightly wrong is often a sign of great familiarity with a particular author.

4. Vuillemin (1979) and Ariew (1984) give valuable discussions of the differences between the Duhem thesis and the Quine thesis. I found these articles very helpful when forming my own views on the subject.

5. Quine's views have altered over the years, but here we will discuss only the position found in his 1951 article.

6. It is possible, however, to use arguments not involving the Quine thesis against the analytic/synthetic distinction. I give two such arguments against the distinction, the argument from justification and the argument from truth, in my 1985 article.

References

Ariew, R. 1984. "The Duhem Thesis," *British Journal for the Philosophy of Science* 35: 313–25.

Crowe, M. J. 1990. "Duhem and the History and Philosophy of Mathematics," *Synthese* 83: 431–47.

Duhem, P. 1904–5. *The Aim and Structure of Physical Theory*. English translation by Philip P. Wiener of the 2nd French edn. of 1914, Atheneum, 1962. French edn., Vrin, 1989.

Duhem, P. 1915. *La Science allemande*. A. Hermann et Fils.

Gillies, D. A. 1985. "The Analytic/Synthetic Problem," *Ratio* 27: 149–59.

Howard, D. 1990. "Einstein and Duhem," *Synthese* 83:363–84.

Martin, R. N. D. 1991. *Pierre Duhem. Philosophy and History in the Work of a Believing Physicist*. Open Court.

Poincaré, H. 1902. *Science and Hypothesis*. English translation, Dover, 1952. French edn., Flammarion, 1968.

Poincaré, H. 1904. "L État actuel et l'avenir de la physique mathématique." Lecture delivered 24 Sept. 1904 to the International Congress of Arts and Science, St. Louis, Missouri, and published in *Bulletin des sciences mathématiques* 28: 302–24. Reprinted in Poincaré, 1905, 91–111.

Poincaré, H. 1905. *The Value of Science.* English translation, Dover, 1958.

Quine, W. V. O. 1951. "Two Dogmas of Empiricism." Reprinted in *From a Logical Point of View,* 2nd rev. edn., Harper Torchbooks, 1961, 20–46.

Vuillemin, J. 1968. Préface to H. Poincaré, *La Science et l'hypothèse*, Flammarion, 1968, 7–19.

Vuillemin, J. 1979. "On Duhem's and Quine's Theses," *Grazer Philosophische Studien* 9: 69–96. Quotations are from reprint in *The Philosophy of W. V. Quine*, ed. L. E. Hahn and P. A. Schilpp, Library of Living Philosophers (La Salle, Ill.: Open Court, 1986), 595–618.