From Donald Gillies, *Philosophy of Science in the Twentieth Century*, 1993.

3.4 The Lives of Duhem and Poincare

Pierre Duhem was born in Paris on 10 June 1861, and died at his country home of Cabrespine (Aude) on 14 September 1916.[2] At the age of twenty, Duhem entered the École normale supérieure where he studied theoretical physics. He was a brilliant student, and received first place in 1885 in the competitive examination for teaching physics. Yet already Duhem had given offence to the French scientific establishment. The trouble began with a dissertation on thermodynamics composed when Duhem was twenty-three. This work contained very outspoken criticisms of Berthelot, then a powerful and influential figure, and it was, as a result, rejected. Moreover, no position in Paris became available for the young Duhem. After brief stays at the science faculties of Lille and Rennes, Duhem became professor of theoretical physics at Bordeaux at the age of thirty-two. He held this post till his death.

Though he later became reconciled with Berthelot, Duhem made further enemies, and was never on friendly terms with the Parisian scientific establishment. He was a man of strong personality, very honest, outstandingly brilliant intellectually, and with firmly held, but unusual, convictions. In short, he was just the sort of individual likely to come into conflict with an intellectual establishment. In addition to all this, Duhem was a devout Catholic, and held very conservative political views. He was therefore somewhat out of place in the liberal, anticlerical atmosphere of the Third Republic. Duhem is thus the mirror image of the members of the Vienna Circle, who were, for the most part, liberal and anticlerical, and consequently at odds with conservative and clerical circles in Austria.

Yet there is something to be said for Duhem’s opponents, for Duhem was one of those unfortunate scientists who, despite great intellectual brilliance, seem to have an unfailing instinct for adopting approaches which prove to be unsuccessful. Duhem had a love of abstract mathematical theories, and tried to develop general thermodynamics and an energeticist programme similar to those of Ostwald and Mach. He rejected the attempt of Boltzmann and Gibbs to reduce thermodynamics to statistical mechanics, and attacked the introduction of atoms into physics. Yet, of course, it was the atomic approach which proved successful. Similarly, in the field of electricity, Duhem attacked Maxwell’s electromagnetic theory, and supported the ideas of Helmholtz, which are now largely forgotten. He also failed to appreciate the importance of Lorentz’s theory of electrons, and, as late as 1915, wrote a polemic against Einstein’s theory of relativity. This is not to say that Duhem did not make some contributions to physics but, on the whole, he was unsuccessful in that field.

His studies of history and philosophy of science had a very different outcome. Duhem's first book on the history of science (*L'Evolution de la mécanique*) was published in 1903. This was followed by *Les Origines de la statique* (1905-6) and a massive, three-volume study of Leonardo da Vinci (1913). Also in 1913, there appeared the first volume of his monumental *Système du monde*. This was planned as twelve huge volumes covering the development of astronomy and physical theory from the pre-Socratics to Galileo. By the time he died in 1916, Duhem had completed ten of the volumes single-handed, and had published five of these.

Duhem's main achievement in the history of science was a revaluation of the medieval period. Prior to him, science had been thought to have ended with the Greeks and to have begun again in the sixteenth century. Duhem showed that the medieval scholastics had considerable scientific achievements to their credit, and that these achievements had a notable influence on the scientific revolution of Copernicus and Galileo.

It is clear that Duhem's religious position influenced his work on the history of science. Enlightenment thinkers had seen Catholicism as an enemy of science, and had thought that science could flourish and develop only if it managed to shake itself free of the obstructive influence of the Church and its superstitious dogmas. Duhem wanted to show that, on the contrary, science had flowered in the medieval period under the aegis of the Church. He also wanted to show that the Enlightenment's hero Galileo had taken many of his ideas from the medieval scholastics, and that the Church had not been altogether wrong in its criticisms of Galileo's theories.

During this period of research on the history of science, Duhem was also developing his ideas on the philosophy of science. As we have seen, *The Aim and Structure of Physical Theory* appeared first as a series of articles in the *Revue de philosophie* in 1904 and 1905, then in book form in 1906, with a second edition in 1914.

Despite this intensive activity in history and philosophy of science, Duhem never abandoned physics, and indeed he always, rather perversely, regarded his work in physics as of much greater importance than his contributions to history and philosophy of science. Thus when, in the 1890s, an enquiry was made as to whether he would be interested in a professorship of the history of science at the College de France in Paris, he replied with the remark: 'I am a physicist. Paris will obtain me only as such, if I ever should return there.' Then again, when in 1913 he prepared his *Notice* in support of his candidacy for membership of the Academy, his account of his work in philosophy was only one tenth as long as his account of his work in physics, and was even shorter than his account of his work as a historian. On this point, the judgement of history is definitely against Duhem's assessment of himself.

………

[Here's some background on the Foucault-Fizeau experiment, 1850, an important measurement of the speed of light, comparing the speed of light in air to its speed in water.]

… 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). 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.

5.2 Duhem's Criticisms of Conventionalism. His Theory of Good Sense *(le bon sons)*

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 Poincare. 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).

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; 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 Poincare 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)

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). 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)

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 Poincare 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, Poincare 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)

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)

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 stientific 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. Poincare devotes a chapter (the thirteenth) of his 1902 book to electrodynamics. He begins (pp. 225-38) by discussing the theories of Ampere 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 Poincare'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, some of which will be raised again later in the book. In the next section, however, I will turn to a consideration of the Quine thesis.