philosophers have to conceive of time, causality, perception if there were such a thing as precognition?

It is a logical possibility that a mind survives the death of its body (or, to allow for reincarnation, bodies), even when due account has been taken of current science. But is there any evidence that it does? If there is, it is likely to be found by objective sifting of the reports concerning paranormal phenomena. In *A Critical Examination of the Belief in a Life after Death* (Springfield, IL, 1961), Ducasse states that the conclusion about survival seemingly warranted at present is that "the balance of the evidence so far obtained is on the side of the reality of survival," but that the evidence is not conclusive.

See also Aesthetic Experience; Art, Expression in; Causation: Metaphysical Issues; Hume, David; Logic, History of: Modern Logic; Mill, John Stuart; Moore, George Edward; Parapsychology; Reincarnation; Royce, Josiah; Sensa.

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For George Santayana's response to Ducasse's views on causation, "ontological liberalism," art, and properties, see *The Letters of George Santayana*, edited by Daniel Cory, 213–215, 234–235, and 287–288 (New York: Scribners, 1955). For a careful review of *Nature, Mind, and Death* by H. H. Price, see the *Journal of Parapsychology* 16 (2) (June 1952).

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Vincent Tomas (1967)
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DUHEM, PIERRE MAURICE MARIE

(1861–1916)

Pierre Maurice Marie Duhem was noted for his original work in theoretical physics, especially thermodynamics, and in the history and philosophy of science. He was born and studied in Paris, and at the age of twenty-five published an important book on thermodynamics. In 1887 he went to the faculty of sciences at Lille University, where he taught hydrodynamics, elasticity, and acoustics. He married but his wife soon died, leaving him with a daughter. In 1893 he moved to Rennes and in 1895 to a chair at Bordeaux University, which he held until his death. Throughout his life he was a Catholic and a conservative.

His approach to physics was systematic and mathematical, and his interest in axiomatic methods undoubtedly determined to some extent the nature of his philosophical account of scientific theories, contained mainly in his book *La théorie physique: son objet, sa structure* (*The Aim and Structure of Physical Theory*), first published in 1906. He wrote a great deal on the history of science, especially in the fields of mechanics, astronomy, and physics, largely because he believed that a knowledge of the history of a concept and of the problems it was designed to meet was essential for a proper understanding of that concept. For the scientist, the history of his subject should be not a mere hobby but an essential part of his scientific work. Duhem's most important works in this field are *Les origines de la statique,* published in 1905–1906, and *Le système du monde,* an account of various systems of astronomy, in eight volumes, published between 1913 and 1958.

SCIENCE AND METAPHYSICS

Duhem's account of physical theory is positivistic and pragmatic, having clear connections with those of Ernst Mach and Henri Poincaré. It begins with, and takes its character largely from, his views on explanation. Indeed, one might say that it begins with a dogmatic and unsupported presupposition about the nature of explanation. He says that to explain is "to strip reality of the appearances covering it like a veil, in order to see the bare reality itself."

But the sciences depend upon observation, and observation shows us no more than the appearances: it cannot penetrate to the reality beneath. This reality is the province of metaphysics; only metaphysics can explain. Science merely deals with the relations between, primarily, our sensations (or the appearance of the world to us) and, ultimately, our abstract ideas of these appearances. A physical theory is somehow an abstract representation of the relations between appearances and not a picture of the reality lurking behind them.

Thus, as far as science alone is concerned, Duhem is as antimetaphysical as Mach and more so than Heinrich Hertz. But, in general, he is not antimetaphysical at all. In a sense, metaphysics is the most important of all studies because it penetrates to the reality of things and explains the appearances; but when we are doing science, we must never import into it metaphysical aims or ideas. Science and metaphysics are both highly respectable, but they are utterly distinct and must be kept so on pain of confusion.

We may, Duhem thinks, penetrate to reality, not by the methods of science, but by pure reason. He attaches great importance to the doctrine that man is free, a statement that cannot conflict with any of the conclusions of science. His metaphysical views, which he did not work out in detail, are Aristotelian; properly understood—that is, stripped of its outmoded science—the Aristotelian physics contains an accurate picture of the cosmological order, whose appearance to human beings is studied by the sciences.

Scientists, according to Duhem, have seldom made the distinction between science and metaphysics, with the result that many theories have been seen as attempted explanations and so have been garnished with strictly superfluous "pictorial" and explanatory elements. These theories can be divided into two parts, called by Duhem "representative" and "explanatory." What is valuable in such theories, and hence what survives and what may be common to apparently different theories, is the representative part.

THE USES OF THEORIES

This conception of the representative nature of theories is linked with the various ways in which theories are useful to us. First, they promote economy by connecting large numbers of experimental laws deductively under a few hypotheses or principles; we need remember only these principles instead of a large number of laws. Second, by classifying laws systematically they enable us to select the laws we need on a particular occasion for a particular purpose. Third, they enable us to predict, that is, to anticipate the results of experiments. These are functions that can be performed by the representative parts of theories, which merely link general statements derived from observation and experiment in a practically convenient way, rather than in a way that corresponds to the underlying reality of things.

THE CONSTRUCTION OF THEORIES

Duhem's account of the way in which theories are constructed exhibits his conception of the nature of physical theories. There are four fundamental operations in their construction.

(1) Among the observable, measurable properties that we wish our theory to represent, we look for a few that can be regarded as simple and as combining to form the rest. Because they are measurable, we can represent them by mathematical symbols. These symbols have no intrinsic connection with the properties they represent: they are conventional signs for these properties. For example, temperature measured in degrees centigrade is a conventional and quantitative representation of the felt warmth and cold of sense experience.

(2) We construct a small number of principles, or "hypotheses," which are propositions arbitrarily connecting our symbols in a manner controlled only by the requirements of convenience and logical consistency. We may give as an example the definition of "momentum" as the product of mass and velocity.

(3) We combine these hypotheses according to the rules of mathematical analysis; again there is no question of representing the real relations between properties, and convenience and consistency are still our guides.

(4) Certain of the consequences drawn out by our third operation are "translated" back into physical terms. That is, we arrive at new statements about the measurable properties of bodies, our methods of defining and measuring these properties serving as a kind of "dictionary" to assist us in the translation. These new statements can now be compared with the results of experiments; the theory is a good one if they fit, a bad one if they do not.

THE NATURE OF LAWS AND THEORIES

Thus, a physical theory, for Duhem, is always mathematical and is a conventional system of linkages between propositions "representing" general statements or laws arrived at by experiment or observation. It is a device for calculating, and nothing matters except that the results of the calculations square with our observations. We might illustrate this in the following way. There are various routes by plane from city *A* (the known laws) to city *B* (the new laws), and it does not matter which route we take as long as we arrive at *B*: We are flying blind; the plane has no windows, and we cannot see the landscape, the sun, or even the clouds during the journey; we must not suppose that the interior of the plane resembles *A* or *B* or the country in between.

The idea that physical characteristics are analyzable into basic elements that are simple and ultimate has figured largely in empiricist and positivist accounts of the sciences. This idea involves numerous difficulties, not the least among them being that of giving any precise meaning to simplicity. Duhem avoids some of the difficulties. Because physical theories do not explain, his simple elements need not be ultimate in nature; they need not be *incapable* of further analysis. They may merely be properties that we *take to be* fundamental and that we have not succeeded in analyzing.

Duhem distinguishes between "practical facts" and "theoretical facts." A description of a phenomenon in ordinary ("observational") language states a practical fact, and its translation into the symbols of the theory states a theoretical fact. But the theoretical fact, as should now be obvious, is a "fact" only in a very odd sense; it has some kind of formal correspondence with the practical fact, but it is always an approximation or an idealization and always has many alternatives.

There is a similar relation between empirical or "commonsense" laws and scientific laws. Scientific laws state the relations between symbols that derive their meanings from the theories of which they are a part. These laws are approximations and idealizations and do not state the relations between actual physical properties. As an example, Duhem cites Boyle's law. This states the relations, not between pressures that may be felt and volumes that may be seen, but between their ideal representatives in a complex theory of gases. The same word, *pressure,* may stand for different concepts in different theories, and in its commonsense, everyday use it stands for a concept or concepts different again from all these.

A commonsense law, such as "Paper is inflammable," is correctly said to be either true or false. No scientific law, however, can be said to be true or false because every accepted scientific law has equally acceptable alternatives. None of these alternatives is any more correct than any of the others. There are two points here. To call the law we actually accept "true" is to suggest that the acceptable alternatives are false, which is misleading. Moreover, all the possible alternatives are idealizations: there is nothing of which they can be said to be strictly true. The symbols used in scientific laws are always too simple to represent completely the phenomena and their connections; hence, the laws must always be provisional.

Duhem distinguishes between observation and interpretation in a way that would now be questioned by certain philosophers. An observer looking at a spot of light on a scale *may* be merely observing this spot, or he may be doing this *and* interpreting it as the final step in measuring the resistance of a coil. Here, observing needs only attentiveness and reliable eyesight, but interpreting requires a knowledge of electrical theory as well. A boy who knew nothing whatever about electrical theory could be given the task of recording the movements of the spot on the scale; a physicist who had not seen these movements but who knew the theory and was prepared to rely on the boy could interpret the records appropriately.

It follows from Duhem's account that scientific laws and theories are not arrived at by induction. No experiment in physics involves mere generalizing from observations because the description of the experiment and its result, in the appropriate terms, involves the use of our physical symbols and, therefore, an interpretation of the phenomena depending upon the acceptance of a particular theory.

Duhem has important things to say about the testing of scientific hypotheses and theories. An empirical generalization of the form "All *A*'s are *B*" can never be conclusively established, because we can never be sure that we have examined all the *A*'s, but it may be conclusively falsified by finding one *A* that is not *B.* Thus, if we take such a generalization to be the pattern of scientific hypotheses, we must say that these hypotheses are open to conclusive refutation. But this is too simple, for a scientific hypothesis can never be tested independently of other hypotheses. This is a point that probably has to be made for any adequate account of scientific theorizing, but it is clearly an essential part of Duhem's account. For him, a hypothesis is always part of a theory, and it is used to make predictions only along with other parts of the theory and perhaps other theories. The failure of a prediction, then, indicates some inadequacy in the hypothesis in question *or* in some other hypothesis of the theory *or* in another theory that has been assumed in making the prediction, but it does no more than this to locate the inadequacy. It shows conclusively that something is wrong, but it tells us neither where to look for that something nor what we must reject or modify.

Thus, there can be no crucial experiments in physics. The pattern of a crucial experiment is this: we have two conflicting hypotheses about a given phenomenon and we design an experiment that will give one specifiable result if one hypothesis is acceptable and the other not, and another specifiable result if the other is acceptable and the first not. But hypotheses are not, as this suggests, independent and isolable. In fact, we must always confront a whole theory, of which one hypothesis is a part, with another whole theory, of which the other hypothesis is a part. It is much more difficult to devise an experiment to choose between theories, and even if we could, it might be that a theory that conflicts with the experiment could be squared with it by making minor modifications whereby it would become as acceptable as the other theory under test.

This view may be criticized on the grounds that it is logically possible to find a crucial experiment that would enable us to choose between two *theories.* Of course, a theory that conflicts with experimental results may be capable of modification so that it does not conflict, but if it then gives exactly the same deductions as its rival, it is doubtful that they can be regarded as different theories, in Duhem's view. On the other hand, if they give different deductions covering the same field, it remains logically possible to devise a conclusive experiment to choose between *these two theories.* Karl Popper objects to Duhem's view on the grounds that the only reason Duhem thought crucial experiments impossible was because he stressed verification rather than falsification. It is not clear that Popper's objection is valid, for Duhem seems to have noticed the obvious fact that the aim of a crucial experiment is to eliminate one of the theories.

Although there is much in common between Duhem's and Poincaré's accounts of scientific theories, Duhem uses this last point about theory modification in criticism of part of Poincaré's view. According to Poincaré and others, certain important hypotheses of physical theory cannot be refuted by experiment because they are *definitions.* For example, the statement that the acceleration of a freely falling body is constant really defines "freely falling"; if an experiment appears to conflict with this, the most we can say is that the body was not falling freely. Nothing we observe can compel us to reject the original statement because it is not an empirical statement. Duhem, in reply, gives a different reason why we sometimes treat scientific statements in this way. It is not that the hypotheses we treat in this way are definitions but that they cannot be tested in isolation; thus, we are usually free, in the face of an unfulfilled prediction, to keep any given hypothesis and reject some other. This does not mean that we shall never be forced to reject that given hypothesis in consequence of some other modification we make to the theory, but only that the odds are against this happening on any given occasion.

See also Continental Philosophy; Conventionalism; Explanation; Hertz, Heinrich Rudolf; Laws, Scientific; Mach, Ernst; Philosophy of Science, History of; Poincaré, Jules Henri; Scientific Method.

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