illustrates the point that in some cases it is difficult to distinguish between those who accept the religious tradition and those who reject it. Tillich considered himself a Christian theologian, but his interpretation of Christian doctrine is so unorthodox that many feel he reconstrued it out of recognition and therefore should be classed with those who substitute a symbolic reinterpretation for traditional beliefs.)

In the other major group we can distinguish between those who simply reject traditional religion (Baron d'Holbach and Bertrand Russell) and those who in addition try to put something in its place. In the latter group we can distinguish between those who try to retain the trappings, perhaps even the doctrinal trappings, of traditional religion but give it a nonsupernaturalistic reinterpretation, usually as symbolic of something or other in the natural world (George Santayana), and those who attempt to depict a quite different sort of religion constructed along nonsupernaturalistic lines (Comte, Dewey, and Wieman).

Outside this classification are those analytical philosophers who restrict themselves to the analysis of concepts and types of utterances. We may regard them as not having a major position in the philosophy of religion, but rather as making contributions that may be useful in the construction of such a position.

See also Religion.

Bibliography

Edwin A. Burtt, *Types of Religious Philosophy,* rev. ed. (New York: Harper, 1951), and Robert Leet Patterson, *An Introduction to the Philosophy of Religion* (New York: Holt, 1958), are useful introductory textbooks. A wide variety of readings in the field can be found in *Philosophy of Religion,* edited by George L. Abernethy and Thomas A. Langford (New York: Macmillan, 1962), and in *Religious Belief and Philosophical Reflection,* edited by William P. Alston (New York, 1963).

The following works are important treatments of a wide variety of topics in this area: John Baillie, *The Interpretation of Religion* (New York: Scribners, 1928); H. J. Paton, *The Modern Predicament* (London: Allen and Unwin, 1955); A. E. Taylor, *The Faith of a Moralist* (New York: Macmillan, 1930); and F. R. Tennant, *Philosophical Theology* (Cambridge, U.K.: Cambridge University Press, 1928). These works are written from a standpoint more or less sympathetic to traditional theism. For fairly comprehensive discussions from a more critical standpoint, see J. M. E. McTaggart, *Some Dogmas of Religion* (London: Arnold, 1906), and Bertrand Russell, *Religion and Science* (New York: Holt, 1935).

Works dealing with the nature and significance of religious experience include William James, *The Varieties of Religious Experience* (New York: Longman, 1902), and Rudolf Otto, *The Idea of the Holy,* translated by J. W. Harvey (New York: Oxford University Press, 1958). The nature of religion is discussed in Josiah Royce, *The Sources of Religious Insight* (New York: Scribners, 1912), and in Julian Huxley, *Religion without Revelation* (New York: Harper, 1957). For the relation of religion and science, see Bertrand Russell, op. cit., and Michael Pupin, ed., *Science and Religion; a Symposium* (New York, 1931). J. H. Newman, *A Grammar of Assent* (London: Burns, Oates, 1870), and Paul Tillich, *Dynamics of Faith* (New York: Harper, 1957), cover the nature of religious faith as a mode of belief and/or awareness.

In Emil Brunner, *The Philosophy of Religion from the Standpoint of Protestant Theology,* translated by A. J. D. Farrer and B. L. Woolf (New York: Scribners, 1937), the nature of revelation and its relation to the results of human experience and reflection are considered. The place of religion in human culture as a whole is dealt with in G. W. F. Hegel, *Lectures on the Philosophy of Religion,* translated by E. B. Speirs and J. B. Sanderson, 3 vols. (London: K. Paul, Trench, Trubner, 1895), and in George Santayana, *Reason in Religion* (New York: Scribners, 1905). For the logical analysis of religious language, see A. G. N. Flew and Alasdair MacIntyre, eds., *New Essays in Philosophical Theology* (London: SCM Press, 1955), and C. B. Martin, *Religious Belief* (Ithaca, NY: Cornell University Press, 1959). Edwyn Bevan, *Symbolism and Belief* (Boston: Beacon Press, 1957), and W. T. Stace, *Time and Eternity* (Princeton, NJ: Princeton University Press, 1952), discuss the nature and significance of religious symbolism. Possibilities for reconstructing religion along relatively nontraditional lines appear in Immanuel Kant, *Religion within the Limits of Reason Alone,* translated by T. M. Greene and H. H. Hudson (La Salle, IL: Open Court, 1934); John Dewey, *A Common Faith* (New Haven, CT: Yale University Press, 1934); and Julian Huxley, op. cit.

William P. Alston (1967)

PHILOSOPHY OF SCIENCE, HISTORY OF

Philosophy of science emerged as a distinctive part of philosophy in the twentieth century. Its defining moment was the meeting (and clash) of two courses of events: the breakdown of the Kantian philosophical tradition and the crisis in the sciences and mathematics in the beginning of the century. But what we now call philosophy of science has a rich intellectual history that goes back to the ancient Greeks. It is intimately connected with the efforts made by many thinkers to come to terms with the distinctive kind of knowledge (*epistēmē*, *scientia*) that science offers. Though science proper was distinguished from natural philosophy only in the nineteenth century, the *philosophy* of natural philosophy had almost the very same agenda that current philosophy of science has.

ARISTOTLE

Aristotle (384–322 BCE) thought that there was a sharp distinction between our understanding of facts and our understanding of the reasons for those facts. Though both types of understanding proceed via deductive syllogism, only the latter is characteristic of science, because only the latter is tied to the knowledge of causes. In *Posterior Analytics*, Aristotle illustrates this difference by contrasting the following two instances of deductive syllogism:

> **Syllogism A**
> Planets do not twinkle.
> What does not twinkle is near.
> Therefore, planets are near.
>
> **Syllogism B**
> Planets are near.
> What is near does not twinkle.
> Therefore, planets do not twinkle.

Syllogism A, Aristotle said, demonstrates the fact that planets are near, but does *not* explain this fact, because the syllogism does not state its causes. However, syllogism B is explanatory because the syllogism gives the *reason why* planets do not twinkle: *because* they are near. Aristotle's point was that, besides being demonstrative, explanatory arguments should also be asymmetric: The asymmetric relation between causes and effects should be reflected in an asymmetric relation between the premises and the conclusion of the explanatory arguments: The premises should explain the conclusion, and not the other way around.

For Aristotle, scientific knowledge forms a tight deductive-axiomatic system whose axioms are *first principles*, which are "true and primary and immediate, and more known than and prior to and causes of the conclusion" (71b19–25). Being an empiricist, he thought that knowledge of causes has experience as its source. But experience on its own cannot lead, through induction, to universal and necessary first principles that state ultimate causes. Nor can first principles be demonstrated, on pain of either circularity or infinite regress. So something besides experience and demonstration is necessary for knowledge of first principles. This is the process of abstraction based on intuition, a process that reveals the essences of things, that is, the properties by virtue of which a thing is what it is. Though Aristotle called first principles "definitions," they are not verbal, but rather state the essences of things. In Aristotle's rich ontology, causes are essential properties of their effects and necessarily give rise to their effects. He thought that the logical

necessity by which the conclusion follows from the premises of an explanatory argument mirrors the physical necessity by which causes produce their effects.

ARISTOTELIANISM

By the 1250s, Aristotle's works had been translated into Latin, either from the original Greek or through Arabic translations, and a whole tradition of writing commentaries on these works flourished. Aristotle's *Organon* was the main source on issues related to logic and knowledge. At about the same time, the first universities were founded in Paris and Oxford, and natural philosophy found in them its chief institutional home. Aristotelianism was the dominant philosophy throughout the Middle Ages, though it was enriched by insights deriving from religious beliefs and many philosophical commentaries. The new Aristotelianism put secular learning on almost equal footing with revealed truth, especially at the University of Paris.

Thomas Aquinas (c. 1225–1274) argued that science and faith cannot have the same object, since the object of science is something seen, whereas the object of faith is the unseen. He found in Aristotle's views the mean between two extremes, one being Plato's view, which demeaned experience and saw in it just an occasion in the process of understanding the realm of pure and immutable forms, the other being the Democretian atomist view, which reduced all knowledge to experience. Aristotelianism, Aquinas thought, was the golden mean. Experience is necessary for knowledge, since nothing can be in the mind if it is not first in the senses. But thought is active in that it extends beyond the bounds of sense and states the necessary, universal, and certain principles on which knowledge is based.

Aquinas inherited (and suitably modified) much of Aristotle's rich metaphysics. Aristotle, drawing a distinction between matter and form, argued that when a change takes place, the matter perdures (persists), while the form changes. He conceived of change as the successive presence of different (even opposing) forms in the substratum. Scholastic philosophers differentiated this substratum from the ordinary matter of experience and called it "prime matter" (*materia prima*). The form that gives prime matter its particular identity (making it a substance of a particular kind) they called "substantial form." Substantial forms were individuating principles that accounted for the specific properties of bodies (which all shared the same prime matter). Aquinas added that prime matter is pure potentiality, incapable of existing by itself. He adopted the view that change (as well as

motion) was the passage from potentiality to actuality. Since a thing cannot be both actual and potential at the same time, he took it to be obvious that nothing can be the active source of its own motion, and hence that motion always requires a mover. Aquinas found solace in the Aristotelian doctrine of the first unmoved mover (the source of all motion), which immediately lent itself to being identified with God.

THE PROBLEM OF MOTION

The status of motion was heavily debated among the Scholastics. One central Aristotelian axiom was that everything that moves requires a mover. Another central axiom was that the mover is in contact with the thing moved. This might be borne out in ordinary experience, but some cases created problems. One of them was projectile motion, and another concerned natural motion, that is, motion toward the natural place of a thing. In both cases, it is not obvious that something does the moving, let alone by being in contact with the thing moved. There was no easy way out of these problems. Underlying them was the very issue of what motion is. Is motion merely the final form momentarily attained by the moving object at any instant? Or is it something in addition, a flux or transformation of forms (in medieval terminology, *forma fluens* or *fluxus formae*)?

The radical answer to this question was sharpened by William of Ockham (c. 1280–1349), who argued that motion is nothing over and above the moving body and its successive and continuous termini. He was a nominalist who thought that only particulars exist. He denied that universals exist and claimed that general terms, or predicates, refer to concepts that apply to many particulars. He argued that the key to the problem of motion was thus held by the abstract noun "motion." It is wrong, he claimed, to think that this and other abstract nouns refer to distinct and separately existing things. Only individual bodies, places, and forms are needed to explain what motion is. Another view came from Jean Buridan (c. 1295–1358). He argued that local motion involves *impetus*, a motive force transmitted from the mover to the moving body, which acts as an internal cause of its continued motion.

ARGUMENT ACCORDING TO IMAGINATION

On March 7, 1277, Etienne Tempier, Bishop of Paris, issued an act condemning 219 propositions drawn from the works of Aristotle and his commentators (including Aquinas). These propositions were supposed to be in conflict with Christian faith and in particular with the omnipotence of God. They included such claims as that the world is eternal, that God could not make several worlds, that God could not make an accident exist without a subject, that God could not move the entire cosmos in straight line. Ironically, this act opened up new conceptual possibilities that were hitherto regarded as closed. If Aristotle could err in matters theological, could he not err in matters philosophical too?

On the premise that only the law of noncontradiction constrains God's actions, it was argued that anything that can be conceived without contradiction is possible. This led to a new type of argumentation: arguing according to the imagination (*secundum imaginationem*). If something could be consistently imagined, then it was possible. New ideas were pursued on this basis, unconstrained by claims concerning the actual course of nature (*secundum cursus naturae*). Central elements of Aristotelian doctrine were given close logical scrutiny. For instance, in the Aristotelian scheme of things, where there is no void and the entire cosmos occupies no place, it made no sense to say that the entire cosmos could move. But what if, Buridan asked, God made the whole cosmos rotate as one solid body? Freed to inquire into the logical possibility of this rotation, Buridan argued that since we can imagine it, there must be something more to motion than the moving body, its forms, and the places it acquires. For if these were all there were to motion, then, contrary to our assumption, the entire cosmos could not move, simply because there would be no places successively acquired.

Ockham pushed argument according to imagination to its limits by arguing that there is no a priori necessity in nature's workings. God could have made things other than they are. Hence, all existing things are contingent. There are no necessary connections between distinct existences, and there is justification for inferring one distinct existence from another, Ockham forcefully argued. Accordingly, all knowledge of things comes from experience. Ockham claimed that there could never be certain causal knowledge based on experience, since God might intervene to produce the effect directly, thereby dispensing with the secondary (material) cause. Ockham thus gave a radical twist to empiricism, putting it in direct conflict with the dominant Aristotelian view.

FIRST PRINCIPLES

The status of scientific knowledge was heavily debated in the thirteen and fourteenth centuries. John Duns Scotus (c. 1265–1308) defended the view that first principles are

knowable with certainty, as they are based only on the natural power of the understanding to see that they are self-evident, ultimately by virtue of the meanings of the terms involved in them. For him, the understanding is not caused by the senses, but only occasioned by them. Once it has received its material from the senses, the understanding exercises its own power in conceiving first principles. Interestingly enough, Scotus thought that there could be certain causal knowledge coming from experience. He asserted as self-evident a principle of induction. He held that this principle is known a priori by the intellect, since a free cause (that is, an act of a free agent) leads by its *form* to the effect that it is ordained to produce. It was then an easy step for him to extend this principle from free causes to natural causes: "Whatever happens frequently through something that is not free, has this something as its natural *per se* cause."

Ockham disagreed with Scotus's account of the first principles, but his central disagreement with his predecessors was about the *content* of first principles. Since he thought there was nothing in the world that corresponded to general concepts (such as universals), he claimed that first principles are, in the first instance, about mental contents. They are about concrete individuals only *indirectly* and insofar as the general terms and concepts can be predicated of concrete things. Ockham is famous for the principle known as Ockham's razor: Entities must not be multiplied without necessity. In fact, this principle of parsimony was well-known in his time. Robert Grosseteste (c. 1168–1253) had put it forward as the law of parsimony (*lex parsimoniae*).

Ockham's most radical follower, Nicolas of Autrecourt (c. 1300–after 1350), rejected the demand for certainty altogether and claimed that only probable knowledge is possible. He endorsed atomism, claiming that it is at least as probable as its rival, Aristotelianism. In reaction, the fourteenth-century Parisian masters—Buridan, Albert of Saxony (c. 1316–1390), and others—claimed that empirical knowledge can be practically certain and wholly adequate for natural science. For Buridan, if we fail to discover an instance of *A* that is not *B*, then it is warranted to claim that all *A*s are *B*. On the basis of this principle, he defended on empirical grounds the Aristotelian claim that there is no vacuum in nature, since, he said, we always experience material bodies.

THE PREROGATIVES OF EXPERIMENTAL SCIENCE

Despite their engagement with philosophical issues in natural science, thinkers such as Ockham and Scotus were little concerned with natural science itself. They saw little role for mathematics, the science of quantity, in physics. They neglected experiment altogether. This was a drawback of their thought in relation to some earlier medieval thinkers. Grosseteste was one of the first to emphasize the role of mathematics in natural science. Roger Bacon (1214–1292) went further by arguing that all sciences rest ultimately on mathematics, that facts should be subsumed under mathematical principles, and that empirical knowledge requires active experimentation. Bacon put forward three virtues of experimental science. First, it criticizes by experiment the conclusions of all the other sciences. Second, it can discover new truths (not of the same kind as already known truths) in the fields of science. Third, it investigates the secrets of nature and delivers knowledge of future and present events.

The emphasis on the mathematical representation of nature exerted important influence on the work of the masters of Merton College in Oxford, who, in the fourteenth century, by and large put aside the philosophical issues of the nature of motion and focused instead on its mathematical representation. Walter Burley (c. 1275–c. 1345), Thomas Bradwardine (c. 1295–1349), William of Heytesbury (before 1313–1372/1373), Richard Swineshead (d. c. 1355), known as the Mertonians, most of whom where nominalists, engaged in a project to investigate motion and its relation to velocity and resistance in an abstract mathematical way. Similar research, though more concerned with the physical nature of motion, was undertaken in Paris by Buridan, Albert of Saxony, and Nicole Oresme (c. 1320–1382), known as the Paris terminists. The mathematical ingenuity of the Mertonians and the Parisians led to many important mathematical results that spread throughout Western Europe and germinated in the thought of many modern thinkers, including Galileo Galilei (1564–1642). By the end of the fourteenth century, a protopositivist movement, concerned not with the ontology of motion, but with its measurement, started to spread.

THE COPERNICAN TURN

In *De revolutionibus orbium coelestium* (On the revolutions of the celestial spheres), Nicolaus Copernicus (1473–1543) developed his famous heliocentric model of the universe. The unsigned preface of the book, which was published posthumously in 1543, firmly placed it within the saving-of-appearances astronomical tradition favored by Plato and endorsed by many medieval thinkers. As it turned out, the preface was written not by Copernicus himself but by Andreas Osiander, a Lutheran

theologian. Copernicus emphatically refused to subscribe to this tradition. He had a *realist* conception of his theory, according to which, as Pierre Duhem put it, "a fully satisfactory astronomy can only be constructed on the basis of hypotheses that are true, that conform to the nature of things" (1908, p. 62).

Before Copernicus, the dominant astronomical theory was that of Claudius Ptolemy (c. 85–c. 165). Pretty much like Aristotle and Plato, Ptolemy had assumed a geocentric model of the universe. To save the appearances of planetary motion, he devised a system of deferents (large circles centered on the earth) and epicycles. There were alternative mathematical models of the motion of the planets (e.g., one based on a moving eccentric circle), but Ptolemy thought that since all these models saved the appearances, they were good enough. The issue of their physical reality was not raised (though at least some medieval philosophers understood these models realistically). Geometry was then the key to studying the celestial motions, but there was no pretense that the world itself was geometrical (though Plato, in the *Timaeus*, did advocate a kind of geometrical atomism). The Copernican heliocentric model, though it made the earth move around the sun, continued to use epicycles. But Copernicus argued that his theory was true. He based this thought mostly on considerations of harmony and simplicity: His own theory placed astronomical facts into a simpler and more harmonious mathematical system.

THE BOOK OF NATURE

Galileo Galilei (1564–1642) famously argued that the book of nature is written in the language of mathematics. He distinguished between logic and mathematics. Logic teaches us how to derive conclusions from premises, but does not tell us whether the premises are true. Mathematics is in the business of demonstrating truth. Though Galileo emphasized the role of experiment in science, he also drew a distinction between appearances and reality, which set the stage for his own, and subsequent, explanatory theories of phenomena, which posited unobservable entities. He accepted and defended the Copernican system and further supported it with his own telescopic observations, which spoke against the dominant Aristotelian view that the heavens are immutable. But the possible truth of Copernicus's theory suggested that the world might not be as it is revealed to us by the senses. Indeed, Galileo understood that the senses can be deceptive, and hence that proper science must go beyond merely relying on the senses. The mathematical theories of motion that he advanced were based on idealizations

and abstractions. Experience provides the raw material for these idealizations (frictionless inclined planes, ideal pendula), but the key method of science was extracting, via abstraction and idealization, the basic structure of a phenomenon so that it could be translated into mathematical form. Then mathematical demonstration takes over and further consequences are deduced, which are tested empirically. So Galileo saw that understanding nature requires the use of creative imagination.

Galileo also distinguished between primary qualities and secondary qualities. Primary qualities—such as shape, size, and motion—are possessed by objects in themselves and are immutable, objective, and amenable to mathematical exploration. Secondary qualities, such as color and taste, are relative, subjective, and fleeting. They are caused on the senses by the primary qualities of objects. The world that science studies is the world of primary qualities. Subjective qualities can be left out of science without any loss. Galileo set for modern science the task of discovering the objective and real mathematical structure of the world. This structure, though mathematical, was also mechanical: All there is in the world is matter in motion.

THE INTERPRETATION OF NATURE

The emerging new science was leaving Aristotelianism behind. But it needed a new method. Better, it needed to have its method spelled out so that the break with Aristotelianism, as a philosophical theory of science, could be complete. Aristotelianism offered two criteria of adequacy for scientific method: epistemological adequacy and metaphysical adequacy. For epistemological adequacy, the scientific method had to meet some philosophical requirements as to what counts as knowledge. For metaphysical adequacy, the metaphysical presuppositions of scientific theories should coincide with the metaphysical presuppositions of philosophical theories. To different extents, the theories of scientific method developed in the seventeenth century were attempts to challenge these criteria, for they were considered more as fetters to science than enablers of its development.

In *Novum organum* (*The New Organon*; 1620/1960), Francis Bacon (1561–1626) placed method at center stage and argued that the world is knowable but only after a long process of trying to understand it—a process that begins with experience and is guided by a new method of induction by elimination. This new method differed from Aristotle's on two counts: on the nature of first principles and on the process of attaining them. According to Bacon, the Aristotelian method (which Bacon called

"anticipation of nature") starts with the senses and particular objects but then flies to first principles and derives from them further consequences. He contrasted this method to his own, which aims at an interpretation of nature, and which gradually and carefully ascends from the senses and particular objects to the most general principles. He rejected induction by enumeration as childish (since it takes account only of positive instances).

Bacon's alternative proceeds in three stages. Stage 1 involves compiling a natural and experimental history to derive a complete inventory of all instances of natural phenomena and their effects. Here observation rules. Then at stage 2, one constructs tables of presences, absences, and degrees of variation. Take, for example, the case of heat, which Bacon discussed in some detail. The table of presences records all phenomena with which the nature under examination (heat) is correlated (e.g., heat is present in light, etc.). The table of absences is a more detailed examination of the list of correlations of the table of presences that seeks to find absences (e.g., heat is not present in the light of the moon). The table of degrees of variation consists of recordings of what happens to correlated phenomena if the nature under investigation (heat) is decreased or increased in its qualities. Stage 3 is induction. Whatever is present when the nature under investigation is present or increases, and whatever is absent when this nature is absent or decreases, is the *form* of this nature. The crucial element in this three-stage process is the elimination or exclusion of all accidental characteristics of the nature under investigation. On the basis of this method, Bacon claimed that heat is motion and nothing else.

Bacon's forms are reminiscent of Aristotelian substantial forms. Yet he also claimed that the form of a nature is the law(s) it obeys. Indeed, Bacon's view was transitional between the Aristotelian view and a more modern conception of laws of nature. Bacon, in his view of science, found almost no place for mathematics, however, though he did favor active experimentation and showed great respect for alchemists because they had laboratories. In an instance of a fingerpost, he claimed that an essential part of interpreting nature by the new method of induction consists in devising a crucial experiment that judges between two competing hypotheses for the causes of an effect. Accordingly, Bacon distinguished between two types of experiments: those that gather data for a natural and experimental history and those that test hypotheses.

THE METAPHYSICAL FOUNDATIONS OF SCIENCE

René Descartes (1596–1650) too sought to provide an adequate philosophical foundation of science. But unlike Bacon, he felt more strongly the force of the skeptical challenge to the very possibility of knowledge of the world. So he took it upon himself to show how there could be certain (indubitable) knowledge and, in particular, how science can be based on certain first principles. Knowledge, he thought, must have the certainty of mathematics. Though Bacon was fine with some notion of virtual certainty, Descartes was after metaphysical certainty, that is, knowledge beyond any doubt. But in the end, Descartes accepted that in science a lot of things (other than the basic laws of nature) can be known only with virtual certainty. He distinguished all substances into two sorts: thinking things (*res cogitans*) and extended things (*res extensa*). He took the essence of mind to be thought and of matter extension. The vehicles of knowledge he took to be intuition and demonstration. We can be certain only of things that we can form clear and distinct ideas of or truths that we can demonstrate. Descartes tried to base his whole foundation for knowledge on a single indubitable truth, namely, "Cogito, ergo sum" ("I think; therefore I exist"). But having demonstrated the existence of God, he took God as guaranteeing the existence of the external world and, ultimately, of our knowledge of it.

Descartes was not a pure rationalist who thought that *all* science could be done a priori. Nor was he an empiricist either, obviously. He did not think that all knowledge stemmed from experience. In *Principia philosophiae* (*Principles of Philosophy*; 1644/1985), he argued that the human mind, by the light of reason alone, can arrive at substantive truths concerning the fundamental laws of nature. These laws (for instance, that the total quantity of motion in the world is conserved) are discovered and justified a priori, as they supposedly stem directly from God's immutability. Accordingly, the basic structure of the world is discovered independently of experience, is metaphysically necessary, and is known with metaphysical certainty. But once this basic structure has been laid down, science can use hypotheses and experiments to fill in the details. This is partly because the basic principles of nature place constraints on whatever else there is and happens in the world, without determining it uniquely. The less fundamental laws of physics are grounded in the fundamental principles, but are not directly deducible from them. Hypotheses are needed to flesh out these principles. Hypotheses are also needed to

determine particular causes and matters of fact in the world, such as the shape, size, and speeds of corpuscles. It is only through experience that the values of such magnitudes can be determined. Accordingly, Descartes thought that the less fundamental laws could be known only with virtual certainty. Descartes's view of nature was mechanical: Everything can be explained in terms of matter in motion.

NEWTON

The real break with the Aristotelian philosophical and scientific outlook occurred with the consolidation of empiricism in the seventeenth century. Empiricists repudiated the metaphysics of essences and the epistemology of rational intuition, innate ideas, and infallible knowledge. Modern philosophical empiricism was shaped by the work of three important figures: Pierre Gassendi (1592–1655), Robert Boyle (1627–1691), and Isaac Newton (1642–1727). Gassendi revived Epicurean atomism and stressed that all knowledge stems from experience. Boyle articulated the mechanical philosophy and engaged in active experimentation to show that the mechanical conception of nature is true.

Newton's scientific achievements, presented in his monumental *Philosophiae naturalis principia mathematica* (*Mathematical Principles of Natural Philosophy*) of 1687, created a new scientific paradigm. The previous paradigm, Cartesianism, was overcome. Newton's methodological reflections became the point of reference for all subsequent discussion concerning the nature and method of science. Newton demanded certain knowledge but rejected the Cartesian route to it. By placing restrictions on what can be known and on what method should be followed, he thought he secured certainty in knowledge. His famous dictum "Hypotheses non fingo" ("I do not feign hypotheses") was supposed to act as a constraint on what can be known. It rules out metaphysical, speculative, and nonmathematical hypotheses that aim to provide the ultimate ground of phenomena. Newton took Descartes to be the chief advocate of hypotheses of the sort he was keen to deny.

His official conception of the method of science was deduction from the phenomena. He contrasted his method with the broad hypothetico-deductive method endorsed by Descartes. Newton's approach was fundamentally mathematical and quantitative. He did not subscribe to the idea that knowledge begins with a painstaking natural and experimental history of the sort suggested by Francis Bacon. The basic laws of motion, in a sense, stem from experience. They are neither true a priori nor metaphysically necessary. Newton strongly disagreed with Gottfried Leibniz (1646–1716), who thought that laws of nature are contingent but knowable a priori through considerations of fitness and perfection. The empirically given phenomena that Newton started with are laws (e.g., Kepler's laws). Then, by means of mathematical reasoning and the basic axioms or laws of motion, he drew further conclusions, for example, that the inverse-square law of gravity applies to all the planets. This kind of deduction from the phenomena has been described as demonstrative induction. It is induction, since it ultimately rests on experience and cannot deliver absolutely certain knowledge. But it is demonstrative, since it proceeds in a mathematically rigorous fashion.

THE REVIVAL OF EMPIRICISM: LOCKE AND HUME

In his preface to *An Essay concerning Human Understanding* (1689), John Locke (1632–1704) praised "the incomparable Mr. Newton" and took his own aim to be "an Under-Labourer in clearing some Ground a little, and removing some of the Rubbish, that lies in the way of Knowledge." Locke was an empiricist and a nominalist. He thought that all ideas come from impressions and claimed that whatever exists is particular. He adopted as fundamental the distinction between primary and secondary qualities. He also drew a distinction between real essences and nominal essences. The real essence of a thing is its underlying internal constitution, based on its primary qualities. The nominal essence concerns the observable characteristics of a thing and amounts to the construction of a genus or a species. The nominal essence of gold, for instance, is a body yellow, malleable, soft, and fusible. Its real essence is its microstructure. Being a nominalist, he thought that real essences are individuals, whereas nominal essences are mere concepts or ideas that define a species or a kind. Though Locke argued that proper knowledge amounts to knowing the real essences of things, he was pessimistic about the prospects of knowing real essences. As he said, he suspected "that natural philosophy is not capable of being made a Science" (1689/1975, IV.12.10). To be sure, knowledge of nominal essences can be had, but Locke thought that this knowledge is trivial and uninteresting, since it is ultimately analytic. Even though Locke's famous book appeared after Newton's *Principia*, it is a pre-Newtonian work. It does not share Newton's optimism that the secrets of nature can be unlocked.

All empiricists of the seventeenth century accepted nominalism and denied the existence of universals. This

led them to face squarely the problem of induction. Realists about universals, including Aristotle, who thought that universals can exist only *in* things, could accommodate induction. They claimed that after a survey of a relatively limited number of instances, thought ascended to the universals shared by these instances and thus arrived at truths that are certain and unrevisable. This route was closed for nominalists. They had to rely on experience through and through, and inductive generalizations based on experience could not yield certain knowledge. This problem came in sharp focus in the work of David Hume (1711–1776).

The subtitle of Hume's *A Treatise of Human Nature* (1739/1978) was *Being an Attempt to Introduce the Experimental Mode of Reasoning into Moral Subjects*. This was an allusion to Newton's achievement and method. Hume thought that the moral sciences had yet to undergo their own Newtonian revolution. He took it upon himself to show how Newton's rules for philosophizing were applicable to the moral sciences. All ideas should come from impressions. Experience must be the arbiter of everything. Hypotheses should be looked upon with contempt. His own principles of association by which the mind works (resemblance, contiguity, and causation) were the psychological analogue of Newton's laws.

Being an empiricist, Hume argued that all factual (and causal) knowledge stems from experience. He revolted against the traditional view that the necessity that links cause and effect is the same as the logical necessity of a demonstrative argument. He argued that there can be *no* a priori demonstration of any causal connection, since the cause can be conceived without its effect and visa versa. Taking a cue from Nicolas Malebranche (1638–1715), he argued that there is no perception of a supposed necessary connection between cause and effect. Hume also went one step further. He found worthless his predecessors' appeals to the power of God to cause things to happen. Hume completely secularized the notion of causation. He also found inadequate, because circular, his predecessors' attempts to explain the link between causes and effects in terms of powers, active forces, and the like.

But his far-reaching point was that the alleged necessity of the causal connection cannot be empirically proved either. As he famously argued, any attempt to show, on the basis of experience, that a regularity that has held in the past *will* or *must* continue to hold in the future is circular and begs the question. It presupposes a principle of uniformity of nature. But this principle is *not* a priori true. Nor can it be proved empirically without circularity. For any attempt to prove it empirically will

have to assume what needs to be proved, namely, that since nature has been uniform in the past, it will or must continue to be uniform in the future. Hume's challenge to any attempt to establish the necessity of causal connections on empirical grounds has become known as his *skepticism* about induction. But Hume never doubted that people think and reason inductively. He just took this to be a fundamental psychological fact about human beings that cannot be accommodated within the confines of the traditional conception of Reason. Indeed, Hume went on to describe in detail some basic "rules by which to judge of causes and effects" (1739/1978, p. 173).

KANT'S AWAKENING

Hume's critique of necessity in nature awoke Immanuel Kant (1724–1804) from his "dogmatic slumber," as he famously stated. Kant thought that Hume questioned the very possibility of science, and Kant took it upon himself to show how science was possible. He claimed that although all knowledge starts with experience, it does not arise from it. It is actively shaped by the categories of the understanding and the forms of pure intuition (space and time). The mind, as it were, imposes conceptual structure on the world, without which no experience could be possible. His central thought was that some synthetic a priori principles must be in place for experience to be possible.

Unlike Newton, Kant thought that proper science is not possible without metaphysics. Yet his understanding of metaphysics contrasted sharply with that of his predecessors. Metaphysics, Kant thought, was a science, in particular, the science of synthetic a priori judgments. Mathematics is a key element in the construction of natural science proper; without mathematics no doctrine concerning determinate natural things is possible. On these grounds, Kant argued that the chemistry of his age was more of an art than a science. The irony, Kant thought, was that though many past great thinkers (Newton in particular) repudiated metaphysics and relied on mathematics to understand nature, they failed to see that such reliance on mathematics made them unable to dispense with metaphysics. For, in the end, they had to treat matter in abstraction from any particular experiences. They postulated universal laws without inquiring into their a priori sources.

As Kant argued in his *Critique of Pure Reason* (1781/1965), the a priori source of the universal laws of nature is the transcendental principles of pure understanding. These constitute the object of knowledge in general. Thought (that is, the understanding) imposes on objects in general certain characteristics in virtue of

which objects become knowable. Phenomenal objects are constituted as objects of experience by the schematized categories of quantity, quality, substance, causation, and community. If an object is to be an object of experience, it must have certain necessary characteristics: It must be extended; its qualities must admit of degrees; it must be a substance in causal interaction with other substances. In his three Analogies of Experience, Kant tried to prove that three general principles hold for all objects of experience: that substance is permanent, that all changes conform to the law of cause and effect, and that all substances are in thoroughgoing interaction. These synthetic a priori principles make experience possible. In particular, there is the universal law of causation, namely, that "everything that happens, that is, begins to be, presupposes something upon which it follows by rule." This is nothing like an empirical generalization. Rather, it is imposed by the mind on objects.

Yet these transcendental principles make no reference to any objects of experience in particular. In his *Metaphysical Foundations of Natural Science* (1786/1970), Kant sought to show how these principles could be concretized in the form of laws of matter in motion. Kant thus enunciated the law of conservation of the quantity of matter, the law of inertia, and the law of equality of action and reaction, and he thought that these laws were the concrete mechanical analogues of his general transcendental principles. These laws were metaphysical laws in that they determined the possible behavior of matter in accordance with mathematical rules. They determine the pure and formal structure of motion, where motion is treated *in abstracto* purely mathematically. It is no accident, of course, that the last two of these laws (the law of inertia and the law of equality of action and reaction) are akin to Newton's laws and that the first law (the law of conservation of the quantity of matter) was presupposed by Newton too. Kant intended his metaphysical foundations of (the possibility of) matter in motion to show how Newtonian mechanics was possible. But Kant also thought that there are physical laws that are discovered empirically. Though he held as true a priori that matter and motion arise out of repulsive and attractive forces, he claimed that the laws of particular forces, even the law of universal attraction as the cause of gravity, can only be discovered empirically.

His predecessors, Kant thought, had failed to see the hierarchy of laws that make natural science possible: transcendental laws that determine the object of possible experience in general, metaphysical laws that determine matter in general, and physical laws that fill in the actual concrete details of motion. Unlike the third kind, laws of the first two kinds require a priori justification and are necessarily true. Though philosophically impeccable, Kant's architectonic suffered severe blows in the nineteenth and early twentieth centuries. The blows came, by and large, from science itself. Creating an explosive mixture that led to the collapse of Kant's synthetic a priori principles were the crisis of Newtonian mechanics, the emergence of Albert Einstein's special and general theories of relativity, the advent of quantum theory, the emergence of non-Euclidean geometries and their application to physics, Gottlob Frege's claim that arithmetic, far from being synthetic a priori, was a body of analytic truths, and David Hilbert's arithmetization of geometry, which proved that no intuition was necessary. It is no exaggeration to claim that much of philosophy of science in the first half of the twentieth century was an attempt to come to terms with the collapse of the Kantian synthetic a priori and to re-cast (or even cast to the wind) the concepts of the a priori and the analytic so as to do justice to developments in the sciences.

WHEWELL VERSUS MILL

The nineteenth century saw the culmination of Newtonian mechanics, mostly in the able hands of Pierre-Simon Laplace (1749–1827) and his followers. The Newtonian framework was extended to capture other phenomena, from optics, to heat, to electricity and magnetism. But Kant's philosophy was very much the doctrine that almost every serious thinker about science had to reckon with. William Whewell (1794–1866) took from Kant the view that ideas (or concepts) are necessary for experience in that only through them can facts be bound together. He noted, for instance, that induction gives rise to a "new mental element." The concept of elliptical orbit, he thought, was not already there in the astronomical data employed by Johannes Kepler, but was a new mental element added by Kepler. But, unlike Kant, he thought that history (and the history of science in particular) had a key role to play in understanding science and its philosophy. He analyzed this role in *The Philosophy of the Inductive Sciences, Founded upon Their History* (1840). Each science grows through three stages, Whewell thought. It begins with a "prelude," in which a mass of unconnected facts is collected. It then enters an "inductive epoch," in which the useful theories of creative scientists bring order to these facts—an act of "colligation." Finally, a "sequel" follows, where the successful theory is extended, refined, and applied. Whewell strongly emphasized the role of hypotheses in science. Hypotheses can be proven true, he

thought, by a "consilience of inductions," by which he meant the theoretical unification that occurs when a theory explains data of a kind different from those it was initially introduced to explain, and when a theory unifies hitherto unrelated domains. Indeed, Whewell found in the consilience of inductions a criterion of truth.

His contemporary John Stuart Mill (1806–1873) took an empiricist turn. Mill was a thoroughgoing inductivist who took all knowledge to arise from experience through induction. He even held that the law of universal causation, namely, that for every event there is a set of circumstances upon which it follows as an invariable and unconditional consequent, is inductively established. Hence, Mill denied that there could be any certain and necessary knowledge. But Mill also tried to delineate the scientific method so that it leads to secure causal knowledge of the world. In *A System of Logic, Ratiocinative and Inductive* (1843/1911) he put forward the method of agreement and the method of difference. According to the first, the cause is the common factor in a number of otherwise different cases in which the effect occurs. According to the second, the cause is the factor that is different in two cases that are similar except that the effect occurs in one, but not the other. In effect, Mill's methods encapsulate what is going on in controlled experiments. Mill was adamant, however, that his methods work *only if* certain substantive metaphysical assumptions are in place: that events have causes, that events have a *limited* number of possible causes, and that the same causes have the same effects, and conversely.

Mill was involved in a debate with Whewell concerning the role of novel predictions. Unlike Whewell, Mill thought that no predictions could *prove* the truth of a theory. He suggested that a hypothesis could not be proved true on the basis that it accounts for known phenomena, since other hypotheses may fair equally well in this respect. He added that novel predictions cannot provide proof either, since they carry no extra weight over predictions of known facts. Mill's target was not just the crude version of the method of hypothesis. He wanted to attack the legitimacy of the rival substantive assumption featured in Whewell's more sophisticated view, namely, that elimination of rival hypotheses can and should be based on explanatory considerations. The difference between Mill and Whewell was over the role of substantive explanatory considerations in scientific method. The debate continues.

CONVENTIONALISM

The inductivist tradition that flourished in England in the nineteenth century was challenged by the rise of French conventionalism. The work of Henri Poincaré (1854–1912) on the foundations of geometry raised the question of whether physical space is Euclidean. In *La science et l'hypothèse* (*Science and Hypothesis*; 1902/1952), Poincaré took this question to be meaningless, because, he suggested, one can make physical space possess *any* geometry one likes, provided that one makes suitable adjustments to one's physical theories. Consequently, he called the axioms of Euclidean geometry "conventions" (definitions in disguise). He extended his geometric conventionalism further by arguing that the principles of mechanics are also conventions. Conventions, for Poincaré, are general principles that are held to be true but whose truth can neither be the product of a priori reasoning nor be established on a posteriori grounds. But calling general principles "conventions" did not imply, for Poincaré, that their adoption (or choice) was arbitrary. He stressed that some principles were more convenient than others. He thought that considerations of simplicity and unity, as well as certain experiential facts, could and should guide the relevant choice. Indeed, he envisaged a hierarchy of the sciences in which the axioms of Euclidean geometry and the principles of Newtonian mechanics are in place (as ultimately freely chosen conventions) so as to make possible empirical and testable physical science.

Though Poincaré took scientific theories to be mixtures of conventions and facts, he favored a structuralist account of scientific knowledge that was Kantian in origin. The basic axioms of geometry and mechanics are (ultimately freely chosen) conventions, and yet, he thought, scientific hypotheses proper, even high-level ones such as Maxwell's laws, are empirical. Faced with discontinuity in theory change (the fact that some basic scientific hypotheses and laws are abandoned in the transition from one theory to another), he argued that there is, nonetheless, substantial continuity at the level of the mathematical equations that represent empirical and theoretical relations. From this, he concluded that the theoretical content of scientific theories is structural, by which he meant that a theory, if successful, correctly represents the *structure* of the world. In the end, the structure of the world is revealed by structurally convergent scientific theories.

THE RISE OF ATOMISM

The beginning of the twentieth century was marked by a heated debate over atomism, an emergent scientific theory that posited unobservable entities, atoms, to account

for a host of observable phenomena (from chemical bonding to Brownian motion). Though many scientists adopted atomism right away, there was strong resistance to it by other eminent scientists. Ernst Mach (1838–1916) resisted atomism on the basis of the empiricist claim that the concept of atoms was radically different from ordinary empirical concepts, and hence problematic. Resistance to atomism was best exemplified in the writings of Pierre Duhem (1861–1916). In *La théorie physique, son objet, sa structure* (*The Aim and Structure of Physical Theory*; 1906/1954), he put forward an antiexplanationist form of instrumentalism that sharply distinguished science and metaphysics, and claimed that explanation belongs to metaphysics and not to science.

But Duhem's theory of science rested on a restricted understanding of scientific method that can be captured by the equation "scientific method = experience + logic." On this view, whatever cannot be proved from experience with the help of logic is irredeemably suspect. To be sure, theories, as hypothetico-deductive systems, help scientists classify and organize the observable phenomena. But, for Duhem, the theoretical hypotheses of theories can never be confirmed or accepted as true. At best, they can be appraised as convenient or inconvenient, empirically adequate or empirically inadequate, classifications of the phenomena. Ironically, Duhem himself offered some of the best arguments against his own instrumentalist conception of theories. The most central one comes from the possibility of *novel* predictions. If a theory were just a "rack filled with tools," it would be hard to understand how it can be "a prophet for us" (Duhem 1906/1954, p. 27).

Duhem was a strong critic of inductivism. He argued that observation in science is not just the act of reporting phenomena. It is the interpretation of phenomena in the light of some theory and other background knowledge. This thesis, known as the view that observation is theory-laden, resurfaced in the 1960s, at that time drawing on a mass of empirical evidence coming from psychology to the effect that perceptual experience is theoretically interpreted. Duhem also stressed that there can be no crucial experiments in science, since no theory can be tested in isolation from other theories (and auxiliary assumptions), and consequently, that any theory can be saved from refutation by making suitable adjustments to collateral theories or auxiliary assumptions.

THE A PRIORI SET IN MOTION

Though battered by developments in physics and mathematics, the Kantian conception of a priori principles did find a place of sorts in the work of the neo-Kantian school of Marburg, Germany. In *Substance and Function* (1910/1923), Ernst Cassirer (1874–1945) argued that, though mathematical structures are necessary for experience, in that phenomena can be identified, organized, and structured *only* if they are embedded in such structures, these structures need not be fixed and immutable for all time. He thought that mathematical structures, though a priori (since they are required for objective experience), are revisable yet convergent: Newer structures accommodate old ones within themselves.

But it was Hans Reichenbach (1891–1953), in *The Theory of Relativity and A Priori Knowledge* (1921/1965), who unpacked the two aspects of Kant's conception of the a priori: that a priori truths are necessarily true, and that they structure objects of knowledge. Reichenbach rejected the first aspect of a priori knowledge, but insisted that the second aspect was inescapable. Knowledge of the physical world, he thought, requires principles of coordination, that is, principles that connect the basic concepts of the theory with reality. These principles he took to structure experience. Mathematics, he thought, was indispensable precisely because it provided a framework of general rules for coordinating scientific concepts and reality. Once this framework is in place, a theory can be presented as an axiomatic system, whose basic axioms (what Reichenbach called "axioms of connection") are empirical. Against Kant, Reichenbach argued that a priori principles of coordination, though they structure objects of knowledge, can be rationally revised in response to experience. He was naturally led to conclude that the only workable notion of the a priori is one that is *relativized*.

LOGICAL POSITIVISM

The influence of Moritz Schlick (1882–1936) on the philosophical course of events can hardly be exaggerated. Armed with the notion of convention, he and his followers, the logical positivists, tried to show that there can be no synthetic a priori at all. They extended conventionalism to logic and mathematics, arguing that the only distinction possible is between empirical (synthetic a posteriori) principles and conventional (analytic a priori) ones. In particular, though they thought that empirical science requires a logico-mathematical framework to be in place before theories can get any grip on reality, this conventional and analytic framework is purely *formal* and is *empty* of factual content. Accordingly, all a priori knowledge is analytic. Moreover, the logical positivists' conventionalist account of analyticity implies that grasping a priori (or analytic) truths requires no special faculty of intuition and that having epistemic access to a priori

(or analytic) truths presents no deep philosophical problem. Accompanying the doctrine that analytic truths are definitions or stipulations was the so-called linguistic doctrine of necessity: that all and only analytic truths are necessary. In the spirit of Hume, this doctrine excised all necessity from nature, and had already played a key role in Ludwig Wittgenstein's *Tractatus Logico-Philosophicus*.

The logical positivists adopted an empiricist criterion of meaning known as the verification principle. Nonanalytic statements, that is, synthetic empirical statements, are meaningful (cognitively significant) if and only if their truth can be verified in experience. In slogan form, the meaning is the method of verification. The logical positivists used this criterion to show that statements of traditional metaphysics were meaningless, since their truth (or falsity) made no difference in experience.

Soon after the foregoing criterion of meaning was adopted, a fierce intellectual debate started among members of the Vienna Circle, a debate that spanned a good deal of the 1930s and came to be known as the "protocol-statements debate." Protocol statements were supposed to capture the content of scientists' observations in such a basic form that they can be immediately verified. One issue was whether protocol statements are (should be) expressed in physical-object language ("The needle points to 2 on the dial") or in phenomenal language ("A black line overlies a "2" shape on a white background"). Though the balance soon turned in favor of the former, Rudolf Carnap (1891–1970), following Schlick, did toy with the idea that protocol statements need no justification, for they constitute the simplest states in which knowledge can be had. But he was soon convinced by the arguments of Otto Neurath (1882–1945) that there are neither self-justified protocol statements nor statements not subject to revision, if only because the processes that yield them are fallible. Instead of abandoning the claim that science provides knowledge, on the grounds that this knowledge cannot be certain, Carnap opted for the view that scientific knowledge falls short of certainty. Armed with Alfred Tarski's account of truth, he claimed that the truth of a scientific statement is no less knowable than the statement itself.

In the course of the 1930s, the concept of verifiability moved from a strict sense of being provable on the basis of experience to the much more liberal sense of being confirmable. The chief problem was that the strong criterion of cognitive significance failed to deliver the goods. In addition to metaphysical statements, many ordinary scientific assertions, those that express universal laws of nature, turn out meaningless on this criterion,

precisely because they are not, strictly speaking, verifiable.

According to the logical positivists, Hilbert's approach to geometry and the Duhem and Poincaré hypothetico-deductive account of scientific theories, if combined, offer a powerful and systematic way to present scientific theories. The basic principles of the theory are taken to be the axioms. But the terms and predicates of the theory are stripped of their interpretation, or meaning. Hence, the axiomatic system itself is entirely formal.

The advantage of the axiomatic approach is that it lays bare the logical structure of the theory, which can then be investigated independently of the meaning, if any, one may assign to its terms and predicates. However, as a formal system, the theory lacks any empirical content. For the theory to acquire such content, its terms and predicates have to be suitably interpreted. It was a central thought of the logical positivists that a scientific theory need not be completely interpreted to be meaningful and applicable. They claimed that it is enough that only *some* terms and predicates, the so-called observational ones, be interpreted. The other terms and predicates of the theory, in particular, those that, taken at face value, purport to refer to unobservable entities, were deemed theoretical and were taken to be only partially interpreted by means of correspondence rules. It was soon realized, however, that the correspondence rules muddle the distinction between the analytic (meaning-related) part and the synthetic (fact-stating) part of a scientific theory—a distinction that was central in the thought of the logical positivists. For, on the one hand, the correspondence rules specify (even if only partly) the meaning of theoretical terms, and on the other hand, they contribute to the factual content of the theory.

A GHOSTLY DISTINCTION

A key idea developed in Carnap's *Logical Syntax of Language* (1934/1937) was that the development of a general theory of the logical syntax of the logico-mathematical language of science would provide a neutral framework in which scientific theories are cast and studied, scientific concepts (e.g., explanation, confirmation, laws, etc.) are explicated, and traditional metaphysical disputes are overcome. The project required a sharp analytic-synthetic distinction. Philosophical statements would be analytic (about the language of science), and scientific statements would be synthetic (about the world). A central (and stable) tenet of Carnap's was the principle of tolerance. Since the choice of a language is a conventional matter (to be evaluated only in terms of its practical fruitfulness), the

aim of philosophy of science, Carnap held, is to make clear the different language *forms* adopted by rival parties in philosophical and scientific disputes (e.g., the dispute between logicists and intuitionists in mathematics, or between realists and idealists, Platonists and nominalists, scientific realists and instrumentalists in philosophy of science). Far from being genuinely factual, these disputes, Carnap thought, center on suitable choices of a language. The principle of tolerance is thus part of Carnap's attempt to eliminate metaphysical "pseudoproblems" from the sciences. It formulates a metatheoretical standpoint in which issues of ontology are replaced by issues concerning logical syntax.

Carnap's project in *The Logical Syntax of Language* came to grief. This was the result of many factors, but prominent among them were Tarski's work on truth (which suggested that truth is an irreducibly semantic notion) and Kurt Gödel's incompleteness theorem. Though Carnap was fully aware of Gödel's limitative results, his own attempt to provide a neutral, minimal metatheoretical framework (the framework of "General Syntax" [1934/1937, pt. IV]) in which the concept of analyticity was defined fell prey to Gödel's proof that some mathematical truths are not provable within such a system.

The notion of analytic a priori truths came under heavy attack from W. V. O. Quine (1908–2000). In "Two Dogmas of Empiricism" (1951), Quine argued that the notion of analyticity is deeply problematic, since it requires a notion of cognitive synonymy (sameness of meaning) and there is no independent criterion of cognitive synonymy. Quine's chief argument against the analytic/synthetic distinction rested on the view that "analytic" was taken to mean unrevisable. If analytic statements have no empirical content, experience cannot possibly have any bearing on their truth-values. So analytic statements cannot undergo truth-value revision. But, Quine argued, nothing (not even logical truths) is unrevisable. Hence, there cannot be any analytic truths. Here Quine took a leaf from Duhem's book (and also from Carnap's book). Confirmation and refutation are holistic; they accrue to systems (theories) as a whole and not to their constituent statements, taken individually. If a theory is confirmed, then everything it says is confirmed. Conversely, if a theory is refuted, then *any* part of it can be revised (abandoned) to restore accord with experience. The image of science that emerged had no place for truths with a special status: all truths are on a par. This leads to a blurring of the distinction between the factual and the conventional. What matters for Quine is

that a theory acquires its empirical content as a whole, by issuing in observational statements and by being confronted with experience.

The cogency of Quine's attack on the a priori rests on the cogency of equating the notion of a priori with the notion of unrevisable. We have already seen a strand in post-Kantian thinking that denied this equation, while holding onto the view that some principles structure experience. It might not be surprising, then, that Carnap was not particularly moved by Quine's criticism. For he *too* denied this equation. Quine, however, did have a point. For Carnap, (a) it is rational to accept analytic statements within a linguistic framework; (b) it is rational to reject them when the framework changes; and (c) all and only analytic statements share some characteristic that distinguishes them from synthetic statements. Even if Quine's criticisms are impotent against (a) and (b), they are quite powerful against (c). The point was simply that the dual role of correspondence rules (and the concomitant Hilbert-style implicit definition of theoretical terms) made drawing this distinction impossible, even *within* a theory. Carnap spent a great deal of effort to develop the characteristic specified in (c). In the end, he had to reinvent Ramsey sentences to find a plausible way to draw the line between the analytic and the synthetic (Psillos 1999, chap. 3).

The challenge to the very possibility of a priori knowledge was a key factor in the *naturalist turn* in the philosophy of science in the 1960s. The emergence of naturalism was a real turning point in the philosophy of science, because it amounted to an ultimate break with neo-Kantianism in all its forms. By the 1960s, philosophy of science had seen the advent of psychologism, naturalism, and history of science.

See also Bayes, Bayes' Theorem, Bayesian Approach to Philosophy of Science; Constructivism and Conventionalism; Laws of Nature; Laws, Scientific; Philosophy of Science, Problems of; Scientific Realism.

Bibliography

Aristotle. *Posterior Analytics*. 2nd ed. Oxford, U.K.: Clarendon Press, 1993.

Bacon, Francis. *The New Organon* (1620), edited by Fulton H. Anderson. New York: Macmillan, 1960.

Burtt, Edwin A. *The Metaphysical Foundations of Modern Physical Science*. 2nd ed. London: Routledge and Kegan Paul, 1932.

Carnap, Rudolf. *The Logical Syntax of Language* (1934). Translated by Amethe Smeaton. London: Kegan Paul, 1937.

Cassirer, Ernst. *Substance and Function* (1910). Translated by William Curtis Swabey and Marie Collins Swabey. Chicago: Open Court, 1923.

Cohen, I. Bernard. *The Birth of a New Physics*. London: Penguin, 1985.

Descartes, René. *Principles of Philosophy* (1644). In *The Philosophical Writings of Descartes*, Vol. 1. Translated by John Cottingham, Robert Stootoff, and Dugald Murdoch. Cambridge, U.K.: Cambridge University Press, 1985.

Duhem, Pierre. *The Aim and Structure of Physical Theory* (1906). Translated by Philip Wiener. Princeton, NJ: Princeton University Press, 1954.

Duhem, Pierre. *To Save the Phenomena* (1908). Translated by Edmund Doland and Chaninah Mascher. Chicago: University of Chicago Press, 1969.

Duns Scotus, John. *Philosophical Writings: A Selection*. Translated by Allan Wolter. Indianapolis, IN: Hackett, 1987.

Gower, Barry. *Scientific Method: An Historical and Philosophical Introduction*. London: Routledge, 1998.

Grant, Edward. *Physical Science in the Middle Ages*. Cambridge, U.K.: Cambridge University Press, 1977.

Hume, David. *A Treatise of Human Nature* (1739), edited by L. A. Selby-Bigge. 2nd ed. edited by P. H. Nidditch. Oxford, U.K.: Clarendon Press, 1978.

Kant, Immanuel. *Critique of Pure Reason* (1787). Translated by Norman Kemp Smith. New York: St. Martin's Press, 1965.

Kant, Immanuel. *Metaphysical Foundations of Natural Science* (1786). Translated by James Ellington. Indianapolis, IN: Bobbs-Merrill, 1970.

Lindberg, David C., ed. *Science in the Middle Ages*. Chicago: University of Chicago Press, 1978.

Locke, John. *An Essay concerning Human Understanding* (1689). Oxford, U.K.: Clarendon Press, 1975.

Losee, John. *A Historical Introduction to the Philosophy of Science*. 4th ed. Oxford, U.K.: Oxford University Press, 2001.

Mill, John Stuart. *A System of Logic, Ratiocinative and Inductive* (1843). 8th ed. London: Longmans, Green, 1911.

Ockham, William. *Philosophical Writings: A Selection*. Translated by Philotheus Boehner. Indianapolis, IN: Hackett, 1990.

Poincaré, Henri. *Science and Hypothesis* (1902). New York: Dover, 1952.

Psillos, Stathis. *Scientific Realism: How Science Tracks Truth* (1999). London: Routledge.

Quine, W. V. O. "Two Dogmas of Empiricism." *Philosophical Review* 60 (1951): 20–43.

Reichenbach, Hans. *The Theory of Relativity and A Priori Knowledge* (1921). Translated by Maria Reichenbach. Berkeley: University of California Press, 1965.

Thayer, H. S., ed. *Newton's Philosophy of Nature: Selections from His Writings*. New York: Hafner, 1953.

Whewell, William. *The Philosophy of the Inductive Sciences, Founded upon Their History*. London: J. W. Parker, 1840.

Whewell, William. *Theory of Scientific Method*. Indianapolis, IN: Hackett, 1989.

Stathis Psillos (2005)

PHILOSOPHY OF SCIENCE, PROBLEMS OF

The scope of the philosophy of science is sufficiently broad to encompass, at one extreme, conceptual problems so intimately connected with science itself that their solution may as readily be regarded a contribution to science as to philosophy and, at the other extreme, problems of so general a philosophical bearing that their solution would as much be a contribution to metaphysics or epistemology as to philosophy of science proper. Similarly, the range of issues investigated by philosophers of science may be so narrow as to concern the explication of a single concept, considered of importance in a single branch of science, and so general as to be concerned with structural features invariant to all the branches of science, taken as a class. Accordingly, it is difficult to draw boundaries that neatly separate philosophy of science from philosophy, from science, or even from the history of science, broadly interpreted. But we can give some characterization of the main groups of problems if we think of science as concerned with providing descriptions of phenomena under which significant regularities emerge and with explaining these regularities. Problems thus arise in connection with terms, with laws, and with theories where a theory is understood as explaining a law and a law is understood as stating the regularities that appear in connection with descriptions of phenomena.

TERMS

Ordinary language provides us the wherewithal to offer indefinitely rich descriptions of individual objects, and, as a matter of logical fact, no description, however rich, will exhaustively describe a given object, however simple. Science chooses a deliberately circumscribed vocabulary for describing objects, and scientists may be said to be concerned only with those objects described with the vocabulary of their science and with these only insofar as they are so describable. Historically, the terms first applied by scientists were continuous with their cognates in ordinary speech, just as science itself was continuous with common experience. But special usages quickly developed, and an important class of philosophical problems concerns the relation between scientific and ordinary language, as well as that between those terms selected for purposes of scientific description and other terms that, though applicable to all the same objects as the former, have no obvious scientific use. Scientists from Galileo Galilei to Arthur Eddington have sometimes tended to impugn as unreal those properties of things not