INTRODUCTION TO PHILOSOPHTY OF SCIENCE

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Content:

- ✓ Scientific discovery and the rationality of scientific thought
- ✓ The unity of science (the project of unification of scinsecs)
- ✓ From Philosophical analysis to scientific practice
- ✓ Interrelationships of science and other fields of culture

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Part1. Philosophy of Science: Definition, Scope and Aims

Definition & Introductory Remarks

Definition:

The **philosophy of science** is one of the **branches** of **philosophy** which studies **the philosophical foundations, philosophical presumptions and philosophical implications** of **science** in the fields:

1. *Mathematical-logical sciences* like **arithmetic** and **logic**
2. *Natural sciences* like **physics** and **biology** and
3. The *social sciences* such as **psychology** and **economics**

So, in this respect, the philosophy of science is closely related to **epistemology** and ontology. We may speak about **methodology** too.

It seeks to explain such things as:

a) **The nature of scientific statements and concepts;**

b) **The way in which they are produced;**

c) **How science explains, predicts and harnesses nature;**

and

d) **The implications of science** for **society** and for the **sciences** themselves.

So, roughly speaking, **philosophy of science** is **the study, from a philosophical perspective, of the elements of scientific inquiry and of their validity**.

Taken broadly as the **progressive improvement of the understanding of nature**, the intellectual enterprise of **science** originally formed an **integral part of philosophy**, and the two areas of inquiry have never finally separated.

Little more than a hundred years ago, theoretical physics - concerned with the fundamental debate about physical nature - was still described as "**natural philosophy**," as distinguished from the two other chief divisions of abstract discussion, viz., moral philosophy and metaphysical philosophy. In fact, only **during the 20th century**, following the professionalization and specialization of the natural sciences, did the **philosophy of science** become recognized as a **separate discipline.**

Philosophical status of scientific theories, propositions (or statements) and concepts/ entities

We may examine:

First, the **raw material** (or elements) with which scientists have to work in developing their theories about the operations of the natural world and,

Second, the **intellectual steps** (or movements) by which they arrive at a scientific understanding of nature.

It is also appropriate to consider finally the main points of view about the **intellectual status** of the **scientific concepts and doctrines** embodying the understanding of nature that have emerged from the philosophical debate about science.

Beginning with the **epistemic status** of **theoretical propositions** in science, it is well to consider the different claims that are made about the **objectivity of their applications or their truth or both**.

Then, turning to the **ontological status of the scientific concepts or entities**, it is likewise necessary to consider the claims that are made about the **objectivity of their reference or of their meaning or both**.

In either case, the purpose of a philosophical critique of science is to establish just how far the **content and reference** of scientific knowledge can be regarded as a true report **about the actual structure and operations of nature** and just **how far they represent, on the contrary, intellectual constructs or artifacts** in terms of which men have chanced, chosen, or found it desirable to organize their thoughts about the structure and operations of nature.

Starting with the epistemic status of scientific theories, **three main views** can be distinguished:

1) **At one extreme is a strict Realist position**, which underscores the **factual basis** of all scientific knowledge and emphasizes the **logical contingency** that this basis implies for all substantive propositions in science. In this view, all but the most purely formal statements in science make assertions about how the world of nature is constituted and operates in fact. Seen from this Realist standpoint, **every proposition in science**, from the most particular observational report to the most general theoretical principle, simply reports a more or less **comprehensive empirical set of facts about nature** and aspires to be an **accurate, objective mirror** of the more or less universal facts about which it speaks.

2) **At the opposite extreme, there is a strict conventionalist position**, which underscores the **constructive role** of the scientist's own theory articulation and emphasizes the **logical necessity** that is thereby built into the resulting conceptual structure. In this view, all but the most purely **observational statements** in science reflect the **patterns** by which the scientist shapes his **conceptual picture** of the world of nature.

3) **Finally, a wide range of intermediate views seeks to evade the central opposition between Realists and conventionalists**. One representative view of this kind, first made popular by **Mach,** invoked **Kant's attack on things-in-themselves**, viewing the attack as providing grounds for **dismissing all debates about reality and objectivity as inescapably barren and empty**. In its most developed form, this so-called **operationalist position** encourages the philosopher to regard **theoretical propositions** in science as **meaningful only insofar as scientific practice includes specific operations** in terms of which those propositions are given operational meaning.

Three main views should here be distinguished. The central question is, now, **whether the nouns and noun phrases used as technical terms in the theoretical propositions of science rely for meaning on any claim that they refer to objective, external entities**.

Here, too, the **Realist interprets** all of the chief **technical terms** of scientific theory as the **names of objective entities existing in nature independently of**

all human theories and interpretations. In this view, entropy, say - a measure of the increase in randomness that every total system undergoes - is a genuine, objective magnitude that has, at all times, played a crucial part in the operations of nature even though physicists have only recently had the wit to discover it; and it just happens, correspondingly, to be the case - in those parts of the cosmos that can be observed - that the total entropy of an isolated system nowhere decreases.

The **instrumentalist** regards all theoretical notions such as entropy as **intellectual fictions or artifacts** created by the scientist's own theory construction and quite distinct from the natural world of objects, systems, and phenomena that scientific theories have to explain.

Meanwhile, the **phenomenalist** repeats, in the case of the technical terms of science, the same agnostic criticism as that offered by the **operationalist** in the case of its theoretical propositions. In this view, it is simply a meaningless waste of time for scientists to debate the existence of enduring theoretical entities, regarded as external, objective things-in-themselves; just as it is similarly wasteful for them to interpret scientific theories as making, or denying, similar claims about the existence of objective, external states of affairs. Instead, the **terms and concepts of science are all to be understood as the product of so many logical, or semantic, operations or constructions, and questions about their real existence are to be swept aside as damaging metaphysical superstitions.**

Nature, scope, and aims of philosophy of science

The philosophy of science attempts:

First to **elucidate the elements** involved in scientific inquiry - **observational procedures, patterns of argument, methods of representation and calculation, metaphysical presuppositions** – and

Then to **evaluate the grounds of their validity from the points of view of formal logic, practical methodology, and metaphysics**.

The philosophy of science is thus a topic for explicit analysis just as are other subdivisions of philosophy.

The boundaries between these subdivisions are, at certain points, somewhat arbitrary; it is not easy to separate completely, for example, the **philosophical**

validation of scientific hypotheses from the formal study of inductive logic, or the **debate about observation in philosophy of science from that in epistemology**.

Historically, the preoccupations of those who would be called philosophers of science have been of two main kinds:

- **ontological**
- **epistemological**, or **epistemic**

This division reflects a long-standing **distinction between object and subject** - i.e., **between nature**, regarded as that about which man sets out to acquire scientific knowledge, **and man himself**, regarded as the creator and either discoverer or possessor of that knowledge.

Some of the observers claim that since 1920, new directions within physics, particularly in quantum mechanics, have discredited any hard-and-fast distinction between the knower and the known or between the observer and his observation. Nevertheless, the distinction remains relevant on an everyday level and can be cautiously retained for the purposes of initial exposition.

The **ontological preoccupations** of philosophers of science frequently **overlap into the substantive areas of the sciences themselves**, for they explore the general problem:

What kinds of entities and elements or theoretical terms can properly figure in man's scientific theories?

And what sort of existence, or other objective status, do such things possess?

In particular cases, this general problem has inevitably raised **questions of substance as well as of intellectual method**. An early 20th-century debate between two Austrian physicists, Ernst Mach and Ludwig Boltzmann, and a German physical chemist, Wilhelm Ostwald, about the existence and reality of atoms, for instance, involved both substantive issues of physics and chemistry and philosophical issues of a more strictly analytical kind.

Until recently the **epistemic concerns** of the subject have been **more purely philosophical,** though this autonomy is now being challenged by studies in psychology that explore cognitive processes and others in sociology that examine the conditioning of cognition through interpersonal and group relationships. **Epistemologically**, philosophers of science have analyzed and evaluated the concepts and methods employed in studying natural phenomena and human behavior, whether individual or collective.

This analysis has covered the general concepts and methods characteristic of all scientific inquiries and also the more particular ones that distinguish the subject matters and problems of different special sciences.

In treating the epistemic issues that arise about science and scientific procedures, the emphasis here is placed upon their **consideration in general terms**; the concepts and methods peculiar to a discipline, say, sociology, are not discussed here.

With its vast range of concerns, the philosophy of science has attracted the attention of **men with very different professional backgrounds and interests**.

At one extreme, as in the writings of Ernest Haeckel, the German Darwinian evolutionist, it has merged into a sweeping kind of **popular science.**

At an opposite extreme, as in 20th-century Logical Positivism, it has been treated as **an extension of formal logic and conceptual analysis**.

Between these extremes, as in the work of the British astrophysicist Arthur ddington and the German quantum physicist Werner Heisenberg, the philosophy of science has moved to the **frontiers of the sciences** and has directly confronted **problems about the existence, status, and validity of theoretical entities and concepts**.

Correspondingly, the philosophy of science has been approached in very diverse spirits, ranging from the **highly abstract and mathematical** to the **concrete and historical** and from the **severely positivistic** to the **frankly theological**.

A) From **René Descartes** to the Logical Positivist **Otto Neurath** the success of pure mathematics and logic has inspired the mathematically minded to cast the whole of natural science into a single formal system after the **pattern of geometry**.

B) Their opponents - from John Locke to N.R. Hanson, a recent U.S. philosopher of science - have sought the **proper basis of man's intellectual confidence in the nature of scientific investigation**, regarded as a human activity.

C) Equally, positivists such as **Hans Reichenbach** have looked to philosophy for proof that **scientific inquiries alone can provide knowledge worthy of the name.**

D) While theists such as **Pierre Duhem** have argued that the claims of science are inherently limited and so leave room for other, more embracing varieties of metaphysical and religious truth.

This diversity of concerns and approaches within the philosophy of science has **affected its relation with other neighbouring disciplines**.

On a practical plane, for instance, different philosophical interpretations have implied different procedures for testing and assessing the strength of rival concepts and hypotheses. Thus, **no clear dividing line can be drawn between the philosophical analysis of scientific theories and the statistical analysis of scientific procedures and experiments or, notably, between the philosophy of science and the history of scientific ideas.**

On a more general and abstract level, the philosophy of science **has never been definitively separated from metaphysics and epistemology.**

Indeed, some 20th-century philosophers - for instance, the U.S. philosophical logician **Willard V. Quine** - effectively restrict the legitimate areas of metaphysics and epistemology to what have here been called the **ontal and epistemic aspects** of the philosophy of science. **In Quine's view, the traditional ontological problem of what there is in the world as man knows it must be attacked by a logical analysis of the claims about what kinds of things exist that are implicit in alternative theoretical systems.**

Meanwhile, the work of such **cognitive psychologists** as Switzerland's **Jean Piaget is eroding the barriers between the logical analysis of conceptual systems, the psychological investigation of thought processes, and the epistemological validation of intellectual procedures.** Piaget, for instance, based his investigations into the acquisition of concepts on a philosophy akin to that of Kant, and, though a psychologist himself, Piaget referred to certain aspects of his work as “genetic epistemology,” a philosophical designation.

So, **the present survey of the philosophy of science contains no effort to prejudge the central question, whether the methods of logical analysis alone are legitimate or whether at certain points the subject legitimately overlaps into such neighbouring subjects as cognitive psychology, the history of science, and epistemology**. Philosophers of science themselves have been sharply divided, some rejecting any alliance except with logic, others cultivating its wider historical and behavioral connections; and both points of view must thus be taken into account.

Part2: Philosophy of Science: Historical Development

1. From classical (pre-Socratic) to medieval periods

At the outset the **problems of science** were **as much those of method as of substance** and were inseparable from those of what has long been called **natural philosophy**.

The first attempts to move beyond traditional mythologies to a rational account of nature, beginning with the **Ionian and south Italian philosophers** around 600 BC, involved the sorts of elements that any such account should comprise.

Were, for example, the **diverse phenomena** of nature **manifestations of one single enduring form of matter or of several elementary substances** mixed together?

Was the fundamental substance **continuous and fluid-like, or discrete and atomic?**

Others asked whether the observed forms of phenomena were **evidence, rather, of some universal, underlying mind or of a variety of coexisting kinds of spirit responsible for phenomena** having different orders of complexity.

The Pre-Socratics based their answers at least as much on **epistemic** grounds - i.e., on what type of account would be genuinely intelligible - as on **ontal or empirical** ones - i.e., on what sorts of enduring entities could possibly have, or be found in experience to have, the required kind of existence.

Their answers ranged:

from the **ontal Realism** of **Parmenides** - foremost **Eleatic** philosopher according to whom all changes are transitory appearances concealing the mutual relations of deeper, unchanging realities

to the **critical skepticism** of **Heracleitus** - the **Ephesian** philosopher according to whom nothing in nature as man knows it can ever have this Parmenidean reality, and everything empirical is in flux.

Though **Plato** and **Aristotle** displayed more precise concern for actual cases, their philosophies of science still rested on the **same mixture of ontological, epistemological, and empirical** considerations.

Questions about nature discussed in **Plato's** ***Timaeus*** and **Aristotle's** ***Physics***, for instance, **were neither purely metaphysical nor purely empirical** in character though they had a **methodological aspect** akin to that of **modern philosophy of science**.

Mathematical entities alone, Plato argued, have the sort of enduring intelligibility that Parmenides had rightly demanded of the **ultimate constituents** in a rational science of nature. Thus, **only a physical theory built on a numerical and geometrical framework will reveal the truly permanent structures and relationships behind the evident flux of phenomena.**

Within such a theory, all inferences will be

self-evidently valid at all times

and so,

exempt from the mutability of empirical events.

Correspondingly, the numbers and figures of formal mathematics will have an immutability denied to familiar physical objects.

Planetary astronomy and the **theory of matter** were, in Plato's view, scientific fields within which this **mathematical methodology** showed immediate promise. The movements of the planets must be explained by constructions drawn from three-dimensional geometry, and the physics of matter seemingly involved atoms with shapes reflecting the geometry of the **five regular solids** (the tetrahedron, dodecahedron, etc.). In either case the **mathematical theories** themselves would alone be **fully exact and intelligible**, whereas **empirical objects and processes** could be no more than **transitory and approximate illustrations** of the enduring entities and theoretical relations underlying them.

Given that **Aristotle's scientific preoccupations** were centred on **marine biology** rather than planetary movements, he naturally developed a very **different scientific methodology**.

In his view, **mathematical entities and relations were too completely general and too remote from actual experience to explain the qualitative details of empirical entities**. So the **ultimate elements of nature** must be not Plato's abstract mathematical forms that supposedly existed apart from actual phenomena but rather **certain more specific entities**, **recognizable within the familiar sequences of empirical experience**.

Instances of such basic essences could be discovered in the typical life cycles of different creatures. For example, the morphogenesis of a seed exemplifies the "coming into being" of the corresponding type of animal or plant, of which the mature specific form - as defined by its essence - is the natural destination of its development. Having recognized the natural destinations toward which natural processes of different kinds were directed, it was then possible to construct a comprehensive classification of essences in terms of which the whole natural world would, in principle, be intelligible.

Explanations within such an all-embracing natural history might not be self-evidently general and immutable, as were those of Plato's geometry, but the theoretical inferences involved would be no less deductive or necessary. It would also account directly for the specific qualitative characters of different observed objects and processes.

The themes stated by Plato and Aristotle are still represented today by **two rival approaches** to the philosophy of science:

- **One (Platonic) based in logic**
- **The other (Aristotelian) based in the history of science**

Between them they dominated the subject during the later period of Greek antiquity, otherwise notable only for the debate between **the Atomist successors of Democritus and Epicurus** and the Stoic philosophers, led by Zeno of Citium. This debate provided the first profound analysis of the strengths and weaknesses of atomistic explanation. Epicureans argued for a purely **corpuscular view** in which the individual units of matter moved quite independently, except when they were in actual contact. For the Stoics the empirical world was intelligible only in terms of interactions and stable patterns maintained by harmonies operative at a distance.

These two debates - **between Platonists and Aristotelians and between Stoics and Epicureans -** presented clearly and for the first time the **chief alternative modes of explanation available to science** and analyzed their possibilities and limitations in general terms.

More than 2,000 years before the rise of modern thermodynamics and field theory, for instance, Aristotle had already recognized the difficulties of explaining changes in physical state (e.g., melting and evaporation) within a purely atomistic theory of matter. Even earlier, Plato had demonstrated the

possibility of a unified mathematical explanation of the differences between different kinds of material substance. In the 20th century the theoretical physicist Werner Heisenberg cited pre-Socratic arguments regarding the ultimate constitution of nature as relevant to contemporary problems.

By contrast with the period before Euclid the geometer (i.e., to 300 BC), **The ensuing Hellenistic, Islamic, and medieval periods added little to the understanding of scientific methodology and explanation.**

From the **Alexandrian astronomer Ptolemy**, who detailed the geocentric theory, most natural philosophers deliberately restricted their intellectual claims in an **instrumentalist manner** - i.e., by endeavouring merely to "**save the phenomena**" by devising successful mathematical procedures for predicting, for example, lunar eclipses and planetary motions.

In this way they **disregarded the mechanisms responsible for those phenomena**, thus preserving the computational techniques of the sciences from the risk of conflict with theology for the following 1,250 years, until the time of Copernicus.

In the High Middle Ages, accordingly, the possibility that man could make himself the intellectual master of nature was largely neglected. Human understanding was dependent on God's illumination. The reliability of scientific knowledge lay not in the merits of its methodology but in the divine grace. Hence, man had no direct line of access to nature; the only road to knowledge was through the divine mind.

Thus**, all the central questions in the philosophy of science** were restated as **theological questions about the relationship between God's omniscience and the more limited knowledge of man**. In this context the metaphor of "**illumination**" was taken so seriously in the 13th century that the subject of optics was cultivated by some distinguished scholars as much for its theological implications as for its physical content.

2. From Renaissance to Kant

Although the intellectual Renaissance of the 16th and 17th centuries was accompanied by a secularization of learning, which **shifted the centre of philosophical and scientific debate from monasteries to universities - and even to salons** - the **link between philosophy and theology** was not abruptly snapped. **Descartes, Newton, and Leibniz**, the leading scholars of the time, were concerned to demonstrate that their positions were compatible with sound theology. Medieval controversies about human knowledge and divine grace found an echo in such arguments as Descartes's assertion that the rational methods of inquiry can be relied on only provided that God does not deliberately deceive us.

Two new factors, however, combined to give the 17th-century debate about **scientific methodology** a new autonomy.

First, philosophy now posed the central questions in the philosophy of science - both about the **origins and functions of scientific concepts** and about the **structure and validity of scientific arguments** - and faced them directly, instead of only as refracted through a theological prism.

Second, these questions were **acquiring an immediate relevance and significance**, simply because men were then launching new, empirically based theories of nature with a seriousness unknown for some 1,200 years.

Between 1600 and 1800 the debate in the philosophy of science was barely separable from that within science itself.

From Bacon and Galileo by way of Descartes and Leibniz to Laplace and Kant, the major participants in the philosophical debate played significant roles on the scientific stage as well.

Thus, **Bacon** and **Descartes** both attempted to formulate explicitly a **new method for the improvement of the intellect** - i.e., **codifying the rational procedures of science in a way that would free them from arbitrary and unfounded or superstitious assumptions** (Bacon's idols) **and ground them in a logically impregnable manner on the properties of** “clear and distinct,” or self-evidently valid concepts **(as distinguished by Descartes)**.

To be sure, the two men offered different recipes for a **rational science** and described the outcome of a properly conducted scientific inquiry in quite **different terms**.

On the one hand, Bacon was preoccupied with **empirically observed facts** as the starting point for all science and relied on theories only insofar as they were derived from those facts. Ideally, he held that the scientist should provide an **exhaustive enumeration of all examples of the empirical phenomenon** under investigation as a preliminary to identifying the natural “**form**” of which they were the manifestation. Though Bacon remained unclear about the exact character of the abstraction involved, he is commonly assumed to have claimed that theoretical propositions in science are justified only if they have been deduced formally from such an enumeration.

On the other hand, in contrast to such so-called "**Baconian induction**," **Descartes** focussed upon the problem of **constructing self-consistent and coherent deductive systems of theory**, within which argument would proceed with the formal security familiar in Euclidean geometry.

So,

Bacon **reacted against the Scholastic reliance on Aristotle's authority by calling for a return to firsthand experience,**

But,

Descartes **reacted against the Skepticism of 16th-century humanists by pointing to mathematics as the pattern to which all genuinely certain knowledge about nature could aspire**.

Inasmuch as **Euclid's axioms, definitions, and postulates** had captured the intrinsic characteristics of spatial relations and provided a theoretical starting point from which the whole of geometry could be deductively inferred, the task for 17th-century physics was to extend Euclid's intellectual structure by adding further, equally self-evident axioms, definitions, and postulates.

Only in this way could the theories of motion, magnetism, and heat - eventually those of physiology and cosmology, too - achieve the same necessary deductive authority.

Descartes set out to show, in his ***Principia Philosophiae***, **how all of the familiar phenomena of physics could be accounted for by a single, fully comprehensive system of mathematical theory, based on Euclidean foundations and conforming to his own deductivist principles.**

The arguments of Bacon and Descartes were really manifestos.

Both offered intellectual programs for a natural science yet to be built, and, while it is true that during the next 150 years, **Galileo, Newton, and many others** actually constructed the new physical science for which the philosophers had been calling, it is also true that the **form of the resulting theories was**, nonetheless, **not exactly what either man had foreseen**.

On the one hand, there was little Baconian induction in Newton's intellectual procedures.

On the other hand, though Newton was powerfully influenced by Descartes's mathematical example, **he followed his methodological maxims only up to a point**.

Granted that the theory of motion and gravitation of Newton's ***Principia*** did indeed conform to Descartes's recipe - adding further dynamical axioms, definitions, and postulates to those of Euclid's geometry - Newton nonetheless made no pretense of proving, in advance of empirical evidence, that these additional assumptions were uniquely self-evident and valid.

Instead, he treated them as working assumptions to be accepted **hypothetically** for just so long as their consequences threw light, in exact detail, on hitherto-unexplained phenomena. Inevitably, the epistemic claims to be made on behalf of such explanations fell short of Descartes's full "deductivist" ambitions.

In this way, Newton devised in practice what philosophers of science have since labelled the **hypothetico-deductive method**, in which, as theorized by Descartes, the **proper form of a theory is seen as a mathematical system in which particular empirical phenomena are explained by relating them back deductively to a small number of general principles and definitions.**

The method, however, **abandons the Cartesian claim** that those principles and definitions can themselves be established, finally and conclusively, before inquiring what light their consequences throw on actual scientific problems and phenomena.

From 1700 on, the terrain of debate in the philosophy of science shifted.

At first, **attacks on Newton's methods and assumptions by Leibniz, Berkeley, and the remaining Cartesians continued, from different points of view**.

But by 1740 the time for both manifestos and objections was past; **the basic scientific soundness of Newton's concepts was no longer in doubt**, and the philosophical question thus became retrospective, viz., How had Newton done it? **Over this new question, 18th-century philosophers were divided into three camps:**

1. **Empiricism**
2. **Rationalism**

3. Kantianism

There were those, like the **Scottish Skeptic David Hume**, who believed that **Newton's philosophy conformed to the Empiricist maxims of Francis Bacon and John Locke.**

Again, there were those - like the **Swiss mathematician Leonhard Euler** and **Immanuel Kant in his younger days** - who assumed that **Newton's physical principles would eventually be put on a fully demonstrative or self-evident basis as required by Cartesian Rationalism**.

Neither of these positions proved entirely successful, as Kant himself came to recognize:

> **The Empiricists** failed to do justice to the deductive rigour of Newton's theoretical arguments;

And,

> **The Rationalists** could not rigorously demonstrate the mathematical uniqueness of Newton's system.

As was already known, even Euclidean geometry, which involves the axiom of parallels could no longer claim a formal uniqueness. It had been shown, in 1733 and again in 1766, that alternative geometrical systems can consistently be developed in which the axiom of parallels is replaced by other mathematically acceptable alternatives.

Clearly, the authority claimed for Newton's concepts and methodology could no longer be sustained in the old Rationalist way; thus a **third alternative**, that of **Kantianism**, arose.

One of the prime goals of Kant's so-called **critical philosophy**, with its famous so-called **transcendental method**, in which knowledge reflects the **categorial structure of the mind**, was to provide an alternative philosophical justification of Newton's results. The system of concepts used in Euclidean geometry and Newtonian physics is uniquely relevant to man's actual experience, Kant argued, **not because the empirical applicability of their principles is self-evident** - no such self-evidence can tell the inquirer anything about external nature. Still less is it because their inductive support is so strong - no Baconian argument can yield the required kind of certainty. Rather, it is **because the scientist can arrive at a coherent, rational system of empirically applicable explanations only by constructing his theories around just those (Euclidean and Newtonian) concepts**. He could, in fact, go even further. Euclidean axioms are required, Kant claimed, not merely for science alone; they specify explicitly cognitive structures (of the mind) that are implicitly involved also - as so-called forms of intuition (specifically of space and time) - in the prescientific rational organization of sensory experience into a coherent, intelligible world of substantial objects seen as interacting by causal processes.

A grasp of Kant's transcendental method would then enable a thinker to recognize (or so Kant hoped) how and in what respects the use of his established system of rational forms and categories is **indispensable** alike for any coherent understanding or even for any experience.

3. From Kant to Empiriocriticism and Conventionalism

Kant's philosophical enterprise took a long time to digest. A century later, in the 1880s, philosophers of science as different in other ways as the **Austrian phenomenalist Ernst Mach and Heinrich Hertz** were both pursuing questions opened up by Kant.

In general terms, Kant's central thesis - i.e., knowledge is given through *a priori* concepts and categories - has proved extremely fertile: it has helped in the analysis of theory construction, and in sensory psychology.

Today there are some criticisms against Kant’s framework of sensory and intellectual organization of Euclidean geometry and of fundamental Newtonian physics and the prescientific notions of substance and cause.

Kant’s proof, as is known today, **was misguided**, for its thesis is simply not the case. This is so, not merely because alternative systems of geometry and dynamics can be developed consistently in mathematical terms (for Kant himself was aware of that fact); rather, it is so because **20th-century astrophysics and quantum mechanics** have succeeded in giving **non-Euclidean and post-Newtonian concepts** an entirely coherent empirical application in the scientific explanation of natural phenomena - and this was something that Kant was not prepared to contemplate.

Pure mathematics aside, indeed, Kant and most of his immediate successors were convinced that Euclid and Newton between them had somehow hit on a uniquely adequate system of geometry and physics - if not on the final mathematical truth about nature.

For about 100 years, then, the epistemic foundations and ontal commitments of this so-called classical science were largely taken for granted. The 19th-century debate in philosophy of science, accordingly, concentrated on peripheral topics. The validity of the classical system having been assumed, the questions remaining for debate involved only its interpretation and implications; and the resulting positions can be classified under the headings of **mechanistic** (or **Materialist**) and **Idealist doctrines**, respectively.

The Idealists took to heart Kant's thesis that the **cognitive structure of experience** is imposed upon nature rather than discovered in it. The psychology of sensory perception, for example, previously barred from direct scientific study by Descartes's absolute separation of mind from matter, was now opened up for exploration.

For most of Kant's successors, however, the Idealist road led away from philosophy of science into other areas - particularly, into political ideology, philosophy of history, and sociology.

Thus, it was not until well into the 20th century that the distinguished joint relativity-quantum theorist **Sir Arthur Eddington,** in his ***Fundamental Theory*** (1946), once again took up seriously the basic task of Kantian Idealism, viz., that of demonstrating, on ***a priori*** epistemological principles, that man's physical interpretation of nature embodies certain necessary structures imposed on physics by the character of his theoretical procedures themselves.

Meanwhile, the 19th-century **mechanistic Materialists** were disregarding Kant's central insights and concentrating instead on the apparent implications of the Newtonian system for other branches of science.

There was one promising exchange about the central epistemic issues in the philosophy of science in mid-century, that which took place between **William Whewell**, a British philosopher and historian of science noted for his work on the theory of induction, and **John Stuart Mill**; but this was abortive. The debate ended in cross-purposes, largely on account of differences in temperament and preoccupations. Whewell's knowledge - not merely of contemporary physical science but of its whole historical background - was both broad and detailed. The **mathematical necessity** of arguments such as those in Newtonian dynamics impressed him quite as much as it had impressed Kant; but he gave a less grandiose account of the reasons for this necessity. Whewell's philosophy, a **Kantian variation on Newton's hypothetical-deductive method**, was **historicized**: it was only by a progressive approach that physicists arrived at the most coherent and comprehensive systems of what Whewell called “consilient” hypotheses - or separately derived, yet concordant, sets of laws - that were compatible with the empirical knowledge then at their disposal.

Mill, on the other hand, principally concerned with the methodology of the social sciences, concentrated on the **observational basis of science to the neglect of its theoretical organization and so emphasized the contingent, or unnecessitated, nature of all genuine empirical knowledge**.

In consequence, it was not until the 1890s and early 1900s that serious doubts grew up about the **finality of the Newtonian synthesis**; and the writings of **Ernst Mach, Heinrich Hertz, Max Planck, Pierre Duhem, and others** inaugurated the new phase of far-reaching **critical reanalysis characteristic** of 20th-century philosophy of science.

All of these men stood back and looked at the Euclidean and Newtonian systems with fresh and less committed eyes. They had learned Kant's lesson about the constructive character of formal theories, without sharing his belief in the unique rationality of the classical synthesis.

The central topics of their discussions turned on the best ways of **restating the Kantian problem**.

Granting that the intellectual activity of theory construction has the effect of building a physical necessity into man's theoretical arguments, they asked, what then follows, ontologically, about the reality or conventionality of the resulting atoms, forces, electrons, etc., and what can be said, epistemologically, about the cognitive status and logical validity of its theoretical principles?

At one extreme **Ernst Mach**, an Austrian physicist and philosopher, and **Richard Avenarius**, the author of a philosophy known as **empiriocriticism**, expounded a **sensationalist form of Empiricism** reminiscent of **David Hume**, who had insisted that all ideas be traceable to “impressions” (sensations).

On their view, theoretical concepts are intellectual fictions, introduced to achieve economy in the intellectual organization of sensory impressions, or observations, for which alone ontal primacy can be claimed.

Correspondingly, **all claims to scientific knowledge had epistemic validity for them only insofar as they could be grounded in such sense impressions.**

At other extreme, as against this instrumentalist or reductionist position, **Max Planck** defended a **qualified Realism**, which, at the least, expressed the ideal toward which all conceptual development in physics proceeds; for, **without a belief in the enduring reality of external nature, he argued, all motive for theoretical improvement in science would vanish.**

Between these two extremes, Henri Poincaré, equally distinguished in mathematics and the philosophy of science, and **Pierre Duhem**, a French theoretical physicist, occupied a range of intermediate so-called **conventionalist positions**, which attempted **to do justice to the arbitrary elements in theory construction** while **avoiding the sort of radical doubt about the ontal status of theoretical entities** that led **Mach** into lifelong **Skepticism** about the reality of atoms.

4. The Vienna Circle and after (Logical Positivism, its proponents and opponents)

In the mid-20th century, **debate in the philosophy of science** became notably **detailed, elaborate, and critical**; those 50 years, in fact, have seen the subject finally achieving the status of a well-established professional discipline.

Among the causes of this development have been the profound changes that have taken place since 1900 within theoretical physics and other fundamental branches of natural science.

So long as the **classical synthesis of Euclid and Newton** retained its unquestioned authority, there had been little occasion to probe its ontological and epistemological bases at all deeply; but **relativity theory** and **quantum mechanics** posed a frontal challenge to that synthesis and inevitably provoked **critical and philosophical questions about the validity of the methods and assumptions on which it had relied**.

Consequently, **between 1920 and 1940** there arose a renewed interaction between **theoretical physicists and philosophers of science** - especially between the **Viennese Positivists and the authors of the new quantum mechanics**.

The main themes of the subsequent debate were largely those introduced into discussion in the period around 1900.

Mach's critical reductionism, in which he tried to reduce all knowledge to statements about sensations, was a prime source both of the **Positivism and Logical Empiricism of the Vienna Circle** and also of the **epistemological**

theories about sense-data and logical constructions developed in Britain about the same time by **Bertrand Russell**, by **G.E. Moore**, and by others.

Meanwhile, the qualified **Realism of Planck and Hertz** was carried further by such men as **Norman Campbell**, an English physicist known for his sharpening of the distinction between laws and theories, and **Karl Popper**, both of whose views reflect the **explicit methodology** of many working scientists today.

A notable **exception** would be the Positivistic followers of **Niels Bohr** in the **Copenhagen school** of theoretical physics. Finally, there has continued to be substantial support for intermediate, conventionalist compromises, with **Kantian overtones**, along the general lines developed by **Poincaré and Duhem**.

From the rich complexity of modern philosophy of science, **two main strands** may be selected for special mention here.

The first is the strand of **neo-Humean Positivism**, which first developed in the Vienna Circle and has flourished more recently in the United States and has been fundamentally preoccupied with epistemological issues.

While largely **abandoning Mach**'s belief that sensations are the sole ultimate ground of knowledge, its proponents have continued, with Mach, to regard **theoretical entities as fictions or logical constructs**, the validity of which depends entirely on the capacity to give them a basis in empirical observations.

This neo-Humean position has derived much encouragement, if not formal confirmation, both from **Einstein's emphasis on the essential role of the observer** in relativity physics and from the **attack** from the side of **quantum theory** made by the German physicist **Werner Heisenberg on any sharp distinction**, at the subatomic level, **between the observer, his observation, and the system observed**.

The Logical Positivists and Empiricists harnessed to these epistemic arguments a **formal apparatus** taken over from the philosophy of mathematics - specifically, from **Russell and Whitehead**'s ***Principia Mathematica*** (1910-13).

In their view, the activity of theory construction is logically equivalent to the creation of **propositional systems**, in which groups of propositions are ideally set out in axiomatic form. So interpreted, the **hypothetico-deductive method** becomes a recipe for devising a succession of progressively more comprehensive axiom systems, based on alternative sets of **general postulates** (or **primitive propositions**) posited without proof, from which particular, empirical propositions can be inferred. As in the case of the special theory of relativity, these particular propositions - for instance, that the axis of Mercury's orbital ellipse will precess at a certain rate - can then be used to validate the general postulates by comparing them with actual experience thus - directly or indirectly - substantiating the more general **primitive propositions** as well.

The subsequent debate within the Viennese school has been concerned, very largely, with the exact character and force of this substantiation - whether it be **verification, confirmation, or corroboration and/or falsification**.

At its most ambitious extreme, the Viennese school aimed at constructing a **single system of unified science**, by which the entire corpus of positive knowledge would be embraced in a single, all-embracing axiom system to be constructed **around Russell's abstract symbolic logic**.

According to this program, all truly scientific knowledge must, first of all, be validated by appeal to neutral empirical observations, on pain of being dismissed as meaningless; and it must then be incorporated into the larger scheme of unified science.

The second, as the strongest opposition to this Empiricist or Positivist strand, is a **Neo-Kantian strand** that **has questioned the very possibility of identifying the pool of theoretically neutral observations necessary for substantiating or discrediting alternative theories in a strictly logical manner.** This Neo-Kantian strand in 20th-century philosophy of science was inaugurated by the pioneering thinkers **Heinrich Hertz** (in electromagnetic wave theory) and **Ludwig Wittgenstein** (in philosophy of language).

Rejecting Mach's central epistemological questions about sensations and ideas, these men have started instead from Kantian questions about the use of representations or models in the explanation of phenomena. Hertz's treatise on ***The Principles of Mechanics*** (1894), for instance, expounded **Newtonian dynamics** as **a formal representation** that **logically entailed empirical conclusions only insofar as the phenomena concerned were already describable in terms drawn from the theory itself**; and **Wittgenstein**'s ***Tractatus Logico-Philosophicus*** (1922) extended Hertz's analysis to provide a **general philosophical theory of language** as an instrument for the representation of facts.

The implications of this approach for the philosophical analysis and methodology of science have been explored further by some of **Wittgenstein's pupils and successors**, who have shifted the focus of discussion **away from the verification of scientific propositions to the establishment of scientific concepts and theories**; by highlighting the problem of conceptual change, **they**

have revived interest in the philosophical significance of the history of scientific ideas. For, from this point of view, logical questions about the structure of propositional systems must be joined by other, equally fundamental rational questions about the manner in which different theoretical systems come to succeed one another (see below **Conceptual change and the development of science**).

During this same period, the remarkable changes taking place within such sciences as **theoretical physics, biochemistry, and psychology** have been provoking philosophical discussions among scientists themselves.

For instance, the **displacement of classical Newtonian physics by Heisenberg's quantum mechanics** has stimulated a new round of arguments about **causality and determinism**, with some people hailing **Heisenberg's Principle of Indeterminacy** as giving **human free will** the toe hold that rigorous 19th-century determinism did not seem to allow.

Methodologically, since 1940 one new centre of philosophical debate has developed, this time in the **behavioral sciences**. Ever since **Descartes and Hobbes**, there has been sharp disagreement about the legitimacy of extending the methods and categories of physical science to the sphere of the higher, distinctively human mental processes; and, even in the 1970s, theoretical psychologists were still far from agreed in their explanations of **human behaviour**.

Some psychologists insist that **human actions** are subject to laws and mechanisms of the same kind as physical processes; others **deny** that any direct **analogy** exists **between rules of conduct and laws of nature**. Currently, this dispute is most lively in the psychology of language. Behaviorists follow **B.F. Skinner**, an American psychologist, in rejecting any distinctive class of mental laws and processes, whereas **cognitive psychologists and generative grammarians, led by Noam Chomsky**, argue that linguistic activities are creative and rule conforming in respects that no behaviorist can explain.

In **sociology and anthropology**, equally, the 20th century has been a period of **methodological controversy**. Here the unresolved conflict has to do with the significance of history in the explanation of collective human behaviour. On one side, there is, in sociology, a school of so-called **structuralists** or **functionalists** that follows another American scholar, **Talcott Parsons**; or, in **anthropology**, there are the British ethnologists **A.R. Radcliffe-Brown and Broniseaw Malinowski who regard all of the cultural practices and social institutions that function within a given community at any time as related together systematically within an overall structure**: to explain any one of those practices or institutions, they hold, it is enough to show how it connects up with all of the other **contemporaneous aspects of the culture**.

On the other side, a more **historically minded school**, notably the German "**critical Marxists**" (such as **Jürgen Habermas**), emphasizes the **dynamic, developing character of social structures and relationships**. Here again, the **methodological debate** is still in progress, and its eventual outcome cannot yet be clearly foreseen.

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Part3. Philosophy of Science: General Topics

Important elements of scientific enterprise

Now we define the recurrent problems that have played a central part in the philosophical debate about science and the crucial elements that any adequate philosophy of science must include in its account.

From the beginning, scientists themselves have been interested not merely in cataloging and describing the world of nature as they find it but in **making the workings of nature intelligible with the help of compact and organized theories**. Correspondingly, philosophers of science are obliged to consider:

Not merely **nature in isolation** - as a mere assemblage of empirical facts - but also,

The **manner** in which man himself perceives and interprets those facts when bringing them within the grasp of an intelligible theory and,

The **respects** in which the validity of the resulting theoretical ideas (or concepts) is affected by that processing of the empirical data.

Historically speaking, the problems posed by this interaction of man and nature have been complex and confused. Though philosophers of science face, even today, many of the same questions that were already being debated in classical Athens, the range and relevance of those questions has been greatly clarified in the meanwhile. For instance, when philosophers in the 17th century analyzed the **nature and possible scope of a mathematical and experimental account of nature**, they helped to clear the ground for Newton to develop the intellectual program and methodology of modern theoretical physics.

Methodological clarification in the philosophy of science has, in this way, repeatedly led to creative advance in science itself and so given rise, in turn, to new experience on which philosophers can draw in taking their methodological analyses further.

It is easy enough to list the chief elements that must find a place in any philosophy of science, but problems arise in mapping the relations between them.

1. Empiricism

Empiricism is the **world view** that knowledge derives from experience of the world, in contrast to **rationalism** which holds that knowledge derives from **reason**. In this sense, scientific statements are subject to and derived from

1. **experiences**
2. **experiments**
3. **observations.**

Scientific theories are developed and tested through **experiments** and **observations**. They are used to gather information through our senses, via **empirical methods** that many humans are capable of experiencing. Once reproduced widely enough by many scientists, this information counts as ***evidence***, upon which the scientific community bases its **explanations** of how things work.

Observations involve **perception**, and so are themselves cognitive acts.

Scientists attempt to use **induction**, **deduction** and **quasi-empirical methods**, and invoke key **conceptual metaphors** to work observations into a coherent, self-consistent structure.

2. Empirical data and their theoretical interpretation

First are the **empirical elements**. **The task of science is to explain actual events, processes, or phenomena in nature; and no system of theoretical ideas, technical terms, and mathematical procedures - or mathematical procedures alone - qualifies as scientific unless it comes to grips with those empirical facts at some point and in some way and helps to make them**

more intelligible (Einstein and confirmation of his theory of relativity through observation of solar eclipse).

On the one hand, the facts in question may be discovered by using **observational methods** - i.e., by recording them as and when they occur naturally, without employing any special contrivances affecting their occurrence. This situation is, of course, the normal case in **astronomy**, in which the objects of study cannot be influenced or controlled.

On the other hand, they may be discovered by using **experimental methods** - i.e., by devising special equipment or apparatus with the help of which those processes or phenomena are caused to occur on demand and under specially controlled conditions. In that case, the scientist can attack scientific problems - to use **Kant**'s vivid metaphor - by "**putting Nature to the question**," as in much of physics and fundamental biology.

Either way, some **philosophical difficulties arise**.

In the first place **there are some difficulties about the results of the scientist's empirical studies: for he must ask:**

1. **How such raw empirical facts can be stated and described in a way that throws light on the scientist's own theoretical problems;**
2. **Do all empirical facts whatever serve as raw material for science?**
3. **Or is this true only of those that have been preselected for their theoretical relevance - or even, to some extent, reshaped to ensure it?**
4. **Is a scientist concerned with every particular empirical event, as such, or only with general phenomena or regularities recognizable in those events?**

Different schools of philosophers treat this raw material in very different ways.

In the second place, there are **conceptual elements**. Every science employs its own characteristic **abstractions, terminology, and techniques of interpretation and explanation,** which can be of very different kinds. They may be **ideal types, as in gas theory and parts of sociology; conservation**

principles, as in dynamics and energetics; taxa, as in biological systematics; particles or constituents, as in genetics and subatomic physics; models or flow diagrams, as in econometric analysis.

Such conceptual elements are the intellectual keys by which phenomena are made intelligible, and a most active philosophical debate has turned around the part they play in the interpretation of phenomena.

If, for instance, the **idea of particles or ultimate constituents of matter** is regarded as a concept created by scientists for the purpose of their own theoretical analysis,

Can an **independent existence** then be claimed for such theoretical entities **in the world of nature** itself?

Or must all such ideas be regarded as **fictions or constructs** for which the claim to reality goes no further than the paper on which the scientific explanations are written?

Similarly, if the **theoretical descriptions of nature** arrived at in science are unavoidably **idealized and abstract**,

Does this imply that the necessity attaching to arguments in, say, theoretical physics is itself only an **artifact of scientists' own procedures for interpreting phenomena**?

Or can one, after all, speak of **natural events** themselves **as happening "of necessity"**?

In the third place, every **natural science** includes also **formal** and **mathematical elements** or **mathematical elements** alone. These may be **mathematical algorithms, or procedures of calculation, like those used in computational astronomy since Babylonian days, or like the computer programs that are their 20th-century counterparts; or geometrical constructions, as in certain branches of optics; or methods of graphical analysis, such as those used in handling statistical data; or the axiomatic systems by which, from classical times on, geometry and physics have been organized into formal schemata of propositions bound together by logical relations.**

Philosophers in the Platonic tradition give such formal elements special consideration, viewing as **authentically intelligible** only those theories the content of which can be presented explicitly in **formal, and preferably in**

mathematical, systems of propositions. Theories of this kind alone are capable - as **Gottlob Frege** expressed it - of employing "**concepts in their pure form**".

Thus, 20th-century philosophers of science have devoted much time and effort to the question:

How far, and on what conditions, can other branches of natural science (e.g., quantum mechanics or genetics) be cast in the same definitive, axiomatic form as classical mechanics and electrical theory?

Or is this formal construction itself merely a human convenience, adopted to simplify the handling of the empirical data, which reveals nothing more about the underlying structure of nature itself?

Each of these three groups of elements poses **problems** about which **philosophers of science** are still in deep **disagreement**; and these differences of view can usefully be illustrated by indicating the various approaches adopted by members of rival schools when discussing each of the groups.

At one extreme can be cited philosophers of **a radically Empiricist frame of mind**, who regard it as important, above all, to emphasize the **empirical foundations of scientific knowledge**.

On this view,

General theoretical principles have authentic scientific content only when interpreted as **empirical generalizations** about directly grasped empirical data; and, correspondingly,

Abstract theoretical entities must be understood as logical constructions from more fundamental elements that can be directly identified in empirical experience. (This belief, of course, was the basis of **Mach**'s conclusion that submicroscopic **atoms** were merely intellectual **fictions** and derived their scientific meaning entirely from the macroscopic sense experiences that they were used to explain.)

At the other extreme, philosophers of a **fully Rationalist, or Cartesian**, bent can be cited who **reject** the idea that **raw empirical facts**, in and of themselves, **display any intelligible or law-governed relationships** whatsoever. For them, as for Plato, **the scientist's bare experience of nature** is a **disorganized aggregate unless and until he is able to discover some rational structure or principles relating these disconnected facts to a larger, more intelligible whole**.

Both of these approaches, the **Empiricist** and the **Rationalist**, emphasize valid and important points; but, in their extreme forms, they give rise to difficulties that are probably insuperable.

As to the Empiricist approach, the credentials of any scientific concept or theory certainly depend to a substantial extent on its basis in **empirical experience**. It is questionable whether sense impressions alone could ever serve as evidence for any scientific position, as Mach and the sense-data philosophers assumed. All genuine scientific observations, as Kant expressed it, have the form of judgments - i.e., are expressed in statements answering questions formulated beforehand.

As to the Rationalist approach, one of the chief tasks for philosophers of science is certainly to account for the **rational interconnections** that give scientific explanations their characteristic intelligibility. In this respect, such men as **Descartes, Kant, and Hertz** have deepened the philosopher's understanding of the scientific enterprise by obliging him to recognize the ways in which the **intellectual organization of scientific theories rests on the scientist's own constructive activities, rather than on the specific facts.**

Yet, it would again be **misleading** to use this fact as an excuse for regarding physical theories - to echo a phrase of Einstein's - as entirely "free creations of the human mind".

While the step from observations to theories does not rest on formal entailments alone, it would be an equally serious counter-exaggeration to suggest that theory construction is totally arbitrary or unconstrained by the imperative demands of the specific problems to be solved.

The outstanding task for most philosophers of science is, accordingly, to find an acceptable **middle way between the Rationalist and Empiricist** extremes and thus to do justice both to the empirical foundations of theories and to their internal organization.

The different emphases of philosophers commonly reflect, at most, differences in their substantive preoccupations.

1. Those who are interested (as was **Mill**) in **possible methods for developing the human or social sciences** naturally place most stress on the **empirical basis of scientific knowledge**.
2. Those who are familiar (as was **Whewell**) with the **actual outcome of theory construction in established sciences**, such as physics,

naturally underscore the **systematic coherence and structure of scientific understanding**.

3. Those who are concerned with the **nature and validity of historical understanding** (as **Giambattista Vico** was) likewise end by giving a very different account of certainty and necessity from those (like **Descartes**) whose **ideal** of scientific knowledge is a **formal, mathematical** one.

3. The empirical procedures of science

Along with the three groups of elements already discussed, each phase in the scientific enterprise - **empirical, conceptual / interpretative and formal** - involves its own characteristic procedures.

On the level of empirical observation and description, **three topics** may be briefly touched upon.

First, there are the **procedures of measurement** through which scientists arrive at quantitative estimates of the variables and magnitudes considered in their theories. By now, there is a well-developed body of knowledge upon which scholars are agreed about many of the techniques to be employed in practice in the measurement of empirical quantities, in the calculation of probable errors, and so on.

About the **deeper significance of measuring procedures** and their outcomes, however, there are still unresolved philosophical disputes. These disagreements reflect the same differences of approach already noted.

Thus,

1. **Some philosophers** regard any **scientific theory concerned with measurable** (or quantifiable) **magnitudes as intrinsically superior to a qualitative** one, however rich and well organized the latter may be.

2. **Others,** by contrast, would argue that any insistence on employing numerical measures at all costs, even in such a science as, for example, systematic biology, can only lead the investigator to misconceive the true nature of the problems involved.

3. **Philosophers of an extremely Empiricist or Positivist persuasion** have sometimes interpreted the **experimental procedures for measuring theoretical magnitudes** in, for example, physics **as providing implicit definitions of the associated technical terms** - the so-called operational definitions - and have thus felt able to claim that the logical entailments that the scientist is seeking between observations and theories are established by linguistic fiat.

Secondly, there are **statistical analytical procedures** for the design of scientific experiments. The **mathematical techniques** employed for this purpose are, in fact, closely related to those involved in the theories of measurement, probable error, and others.

In this area, the connection between philosophical discussions of **inductive logic** and the **practical procedures** of working scientists is at its closest. Whereas a **religious scientist (Blaise Pascal) and an astronomer (Pierre, marquis de Laplace)**, all mathematicians as well, **analyzed the philosophical foundations of the modern calculus of probabilities** in the 17th and 18th centuries, 20th-century mathematicians and inductive logicians have similarly explored **the intellectual basis for the design and interpretation of significant experiments**; and, by now, the relevant procedures form a branch of mathematical statistics with many valuable applications, particularly in fields such as sociology and economics, in which large numbers of variables are involved.

Thirdly, the initial handling of the scientist's empirical data requires him to employ **procedures of systematic classification**. The nature and validity of scientific classification procedures and of the **species, genera, families, and so on** into which scientists divide their empirical subject matter have been the subject of a long and contentious debate.

In some areas of thought, the preliminary identification and classification of the empirical material still raises contentious philosophical questions. When the sociologist theorizes about social groups or systems in the human sciences, for instance, he has to decide what collections of men and institutions do, or do not, fall under those general headings.

Can objective tests be found for identifying natural units of sociological analysis?

Or, is this choice of units merely set up for the sociologist's own convenience?

This **uncertainty** about the very subject matter of sociology is itself an obstacle to the creation of an agreed-upon body of social theory.

Comparable difficulties can arise in **anthropology, linguistics, and psychology** as well.

It is, therefore, not surprising that some critics have even questioned whether these disciplines can truly be called sciences.

The character of these continuing difficulties underscores one point of general significance about the **relation of empirical evidence to scientific theories**:

Though philosophers may find it necessary to **distinguish the empirical phases, elements, and procedures of science from the theoretical ones analytically**, it does not follow that **they can be kept wholly separate in actual practice**. **Satisfactory measuring procedures, experimental designs, and systematic classification principles are, no doubt, necessary preconditions for effective theorizing; but they themselves are subject, in turn, to revision and refinement in the light of subsequent theoretical considerations.**

In arriving at his dynamical theories, for example, **Newton** had to begin by relying on older commonsensical notions of **effort, weight, and amount of movement**; but he soon replaced these by the more exact, theoretically defined concepts of **force, mass, and momentum**, and this change reacted back onto the empirical procedures of physics also.

Likewise in other fields of science, the decision as to whether or not the outcome of any empirical procedure is scientifically relevant or significant soon ceases to be a purely empirical question, as theoretical changes react back onto those empirical procedures and compel the scientist to modify his manner of collecting and describing the supposedly raw data of science.

In this way, the empirical evidence by which his scientific conclusions are justified rapidly loses its pure and theoretically neutral character.

4. The formal structures of science and scintific theories

Now we consider the **formal structures of scientific theory** that is (along with the problem of **conceptual change**) one of those aspects of the scientific enterprise that have dominated **recent debate** in the philosophy of science.

Since 1920, most analytical philosophers of science have explicitly based their program on a presupposition inherited from **Descartes and Plato**, viz., that the

intellectual content of any natural science can be expressed in a formal propositional system, having a definite, essential logical structure - what a leading American philosopher of science, **Ernest Nagel**, concisely called "**the structure of science**" in his book of that title (1961).

One immediate inspiration of this program was the work of **David Hilbert**. To make the **methods of mathematical proof** more explicit and more perspicuous and thus more rigorous, **Hilbert** employed the techniques of

1. **Formalization,** a **reduction to relations** while disregarding the nature of the relata, and,
2. **Axiomatization,** a **tracing of entailments back to accepted axioms**.

The same techniques were taken over into the **philosophy of mathematics** by a pioneer German logician, **Gottlob Frege**, and into **symbolic logic** by **Bertrand Russell and his collaborator Alfred North Whitehead**; and, from 1920 on, the **Viennese Positivists and their successors** attempted to employ them in the **philosophy of science** also, hoping to demonstrate the validity of formal patterns of scientific inference by the straightforward extension of methods already familiar in deductive logic.

According to the resulting program, the **primary task for the philosophy of science** was to **repeat** in quite general terms the **kind of analysis** by which, in the science of mechanics, **Heinrich Hertz** had already sorted out the formal aspects of science from its empirical aspects. The program was founded on the expectation that it would be possible,

First, to demonstrate the **existence of formal structures** that were essential to any science, properly so-called, and,

Second, to **identify the nature of scientific laws, principles, hypotheses, and observations by their characteristic logical functions**.

Once this had been done, rigorous formal definitions could then be given of **validity, probability, degree of confirmation, and all of the other evidential relations** involved in the judgment of scientific arguments.

The actual working out of this program has involved complex and highly technical investigations, in the course of which great ingenuity has been displayed - as, for instance, by **Rudolf Carnap**, in his system of inductive logic, for the **criticism of arguments** in support of empirical generalizations, and by **Hans Reichenbach** in his **analysis of probabilistic arguments**.

The goal of a purely formal analysis of scientific inference has generated difficulties, for instance, by tempting logicians to play down important differences between **mere descriptive generalizations** about natural phenomena and the **explanatory theories** (laws, principles, etc.) that a scientist develops to make those phenomena intelligible.

One distinguished supporter of this program, **Carl Hempel**, originally a member of the Berlin group (founded by Reichenbach and allied with the Vienna Circle), has discussed what he calls the **theoretician's dilemma**:

If the task of explaining natural phenomena requires a proof that the character of those phenomena is formally entailed by the conditions of their occurrence, taken together with certain straightforward generalizations based on previous empirical experience, and if those empirical generalizations include references to hypothetical entities, then the theorist is faced with an invidious choice: for, in that case, either his generalizations (his laws) do in fact provide a logical link between the conditions of the phenomena and their actual occurrence and the assumption of hypothetical entities is formally superfluous; or else they do not succeed in doing so, and that assumption will not have strictly explained the phenomena.

Clearly, this dilemma can be evaded only by challenging the identification of laws with generalizations and insisting that any appeal to laws of nature always involves the scientist in **reinterpreting natural phenomena, not in merely generalizing about them**.

Looking **beyond the internal structure of inductive logic**, the **dubious equation of scientific laws with empirical generalizations** has also been criticized on the ground that it treats the content of those laws as matters of happenstance, far more accidental or contingent than those expressed in any genuine law of nature. In the opposing view, the **explanatory force** of, say, the physicist's law of inertia is totally different from that of such a generalizing statement as “All swans are white”; and one can learn nothing about the validity of actual physical arguments unless his philosophical analysis respects that crucial difference.

It has not proved easy, however, to analyze the formal structure of the sciences in any less abstract manner than that of the Viennese Positivists or to give a true representation of the working language and arguments of science. In his *Essay on **Metaphysics*** (1940), **R.G. Collingwood**, a British philosopher and historian, made one striking attempt, in which **the formal structure of intellectual systems was explained in terms not of direct entailments between more or less universal propositions but rather of mutual presuppositions between more or less general concepts**.

In this account, the **principle of inertia was not the most universally true assertion in dynamics but was, rather, the most generally applicable presupposition, or principle of interpretation**.

Such an account has the merit of **explaining why, within a particular science, certain formal patterns of argument carry the apparent necessity that they do**; but at the same time **it lays itself open to the charge of yielding too much to relativism and so of destroying the objectivity of scientific knowledge** by giving the impression that the conceptual structures of science are imposed on phenomena by the arbitrary choice of the scientific theorist himself.

The late 1960s, accordingly, saw a renewal of questioning about the original assumption, viz., that the entire intellectual content of a science can be captured in a propositional or presuppositional system. Certain of the doubts about this thesis revive criticisms put forward at the turn of the century by a U.S. Pragmatist, **Charles Sanders Peirce**, who argued that the **logical status of the theoretical terms and statements in a science is subject to historical change as the conceptual organization of the science develops**. (This same insight has been explored more recently by a U.S. logician, **Willard Quine**, who **rejects any attempt to classify statements within scientific theories using the traditional hard-and-fast dichotomies – contingent-necessary and synthetic-analytic - as fallacious and dogmatic**.) Other criticisms of the thesis go deeper.

Justification in science

If this situation is true of the **earlier stages in discovery**, it is no less true in the case of **justification** itself.

Here again, from 1920 on, the debate in the philosophy of science focussed predominantly on **two sharply opposed positions**, both of which appear in retrospect to be excessively narrow.

On the one hand, **Empiricist philosophers** argued for a view that made **prediction the crucial test of scientific validity**;

On the other hand, philosophers of a more **Rationalist temperament** saw **coherence and scope** **as the crucial requirements**.

For Empiricists, the fundamental presupposition is that the facts justifying changes in scientific ideas are both **intellectually prior to the theories** that are, in due course, developed to explain them and also **capable of being recognized independently and in advance of all theory construction**.

Given this presupposition, they regard **prediction and validation** as the crucial and distinctive steps in scientific procedure, arguing that, to establish the validity of any general scientific proposition, it is necessary to show that the theoretical generalization of which the validity is in question entails particular factual statements that are borne out by **independent empirical observations**. This validation process then involves two essential steps:

1. the formal step of inferring novel predictions from the theory and
2. the empirical step of comparing those predictions with the facts and so confirming the theory or proving it false.

On closer inspection, both steps in the received Empiricist procedure face serious difficulties:

As to step (1), **there appears to be no objection to the idea of deducing particular factual predictions directly from theoretical hypotheses**, so long as one accepts the Empiricist interpretation of laws of nature as universal empirical assertions on the same logical level as "All polar bears are white." Once that interpretation is questioned, however, it is less clear that direct deductive inferences from theory to fact are always practicable.

On the contrary, if theoretical laws and purely empirical reports are framed in terms of distinct and diverse sets of concepts, no general procedure can be available for passing deductively from one to the other. For the theory will then be a **reinterpretation** of the facts, **not a mere generalization** from them.

Similarly, **with step (2)**, an **empirical confrontation** of theories and facts gives rise to a more complex range of choices than those implied by the Empiricist account.

When faced with discrepancies between prediction and observation, scientists certainly have to modify their theoretical explanations; but this modification can normally be made in any of several alternative ways.

For instance,

The theoretical relevance of a particular observation may be questioned; or

Some alternative theoretical interpretation may be put forward; or

Further refinements may be made within the structure of the theory concerned - and all of this can be done before any question arises of a direct and necessary conflict between the discordant observation and the general theoretical doctrine under investigation.

The **rival, constructivist position** derives its attractions from such objections as these. This position follows lines of thought already sketched by the French theoretical physicist **Pierre Duhem** at the turn of the 20th century.

On this account, the essential test of a science is that it should provide **coherent, consistent, and wide-ranging theoretical organizations**. Empirical facts will then be recognized as scientifically relevant only to the extent that they exemplify these interpretations and make them more discriminating.

Thus, no single factual observation can ever serve as a logically crucial experiment and confirm or refute any one specific doctrine conclusively, taken apart from a whole complex of theory and interpretation.

If **these two philosophical approaches** are reconsidered today **against** a broader and more **historical background**, however, they no longer appear to be either as exhaustive or as contradictory as they did in the 1920s and 1930s. By choosing suitable illustrations, of course, one can make each position highly attractive and plausible since, in one situation or another, the rational considerations that carry genuine weight in the actual justification of novel scientific theories include both **predictive success and conceptual coherence**.

But the "**Book of Nature**," as **Galileo** called it, is like **Holy Scripture**: it offers texts to suit all occasions and purposes. And, on second thought, it can be argued that both Empiricist and constructivist philosophers **oversimplify** the justification process in science and the criteria by which scientists judge the validity of novel concepts and theories.

Far from there being any single or simple test of validity, the question whether **predictive success or coherence, simplicity, historical authenticity, or mechanical intelligibility** is the key consideration must be considered afresh from case to case, with an eye to the specific demands of each new scientific problem situation.

Within the historically developing enterprise of science, intellectual problems arise of many different types, depending both upon the kinds of subject matter under investigation and upon the stage of development of the science concerned.

In one science and at one stage, particular weight may attach to **a single unexpectedly successful prediction**: as when the wave theory of light led to the totally unexpected discovery that a perfectly circular obstacle placed in front of a point source of light produces a circular shadow having a bright spot at its centre. Even within a single science such as physics, indeed, **scientists are not faced at every stage by problems and judgments of a single, uniform type**. Instead, the historical evolution of physics - down the centuries from Nicole Oresme, Galileo, and Newton to Maxwell, Rutherford, and Heisenberg - has generated an entire genealogy of varied problems.

So, within the more complex framework of a developing rational enterprise, the philosopher's task is no longer to **impose** any single or simple criterion of intellectual choice upon scientific judgments of all kinds. Rather, his task is to recognize **how** the rational considerations and criteria of validity relevant to particular judgments **vary** with the **theoretical problem situations** that provide their historical contexts.

The Justification of Scientific Statements

The **most powerful statements** in science are those with the **widest applicability**. For example, **Newton's Third Law** is a powerful statement because it applies to **every action, anywhere, and at any time**.

But it is not possible for scientists to have tested every incidence of an action, and found a reaction. **How is it**, then, that they can assert that the Third Law is in some sense **true**? They have, of course, tested many, many actions, and in each one have been able to find the corresponding reaction. But can we be sure that the next time we test the Third Law, it will be found to hold true?

1. Induction

One solution to this problem is to rely on the notion of **induction**.

Inductive reasoning maintains that **if a situation holds in all observed cases, then the situation holds in all such cases**.

So, after completing a series of experiments that support the Third Law, one is justified in maintaining that the Law holds in all cases.

Explaining **why induction is true** has been somewhat **problematic**.

One cannot use deduction, the usual process of moving logically from premise to conclusion, because there is simply no syllogism that will allow such a move. No matter how many times 17th Century biologists observed white **swans**, and in how many different locations, **there is no deductive path** that can lead them to the conclusion that **all swans are white**. This is just as well, since, as it turned out, that conclusion would have been wrong. Similarly, it is at least possible that **an observation will be done tomorrow that shows an occasion in which an action is not accompanied by a reaction**;

The same is true of any scientific law.

One answer has been to conceive of a different form of rational argument, one that does not relying on deduction. Whereas deduction allows one to formulate a specific truth from a general truth (all crows are black; this is crow; therefore this is black), induction somehow allows one to formulate a general truth from some series of specific observations (this is a crow and it is black; that is a crow and it is black; therefore all crows are black; this is a **numeration**).

The **problem of induction** is one of considerable debate and moment in the philosophy of science: is induction indeed justified, and if so, how?

Here induction is justified by induction:

We have seen that induction has been true; This is an induction; So it is true!

But,

This is begging the question !

This is circular reasoning!

This is assuming the truth of the thing needing proof!

(We may discuss **Russell's view and Popper's view**)

2. Falsifiability

Another way to use logic to justify scientific statements, first formally discussed by **Karl Popper**, is **falsifiability**.

Falsification's aim is to re-introduce deductive reasoning into the debate. It is not possible to deduce a general statement from a series of specific ones, but it is possible for one specific statement to prove that a general statement is false. Finding a **black swan** (only **ONE** swan!) might be sufficient to show that the general statement 'all swans are white' is false.

Falsifiability neatly avoids the problem of induction, because it does not make use of inductive reasoning. However, it introduces its own difficulties. When an apparent falsification occurs, **it is always possible to introduce an addition to a theory that will render it unfalsified**.

So, for instance, ornithologists might have simply argued that the large black bird found in Australia was not a member of the genus Cygnus, but of some other, or perhaps some new, genus.

3. Coherentism

Induction and Falsification both attempt to justify scientific statements by **reference to other specific scientific statements**.

Both must avoid the **problem of the criterion**, in which any justification must in turn be justified, resulting in an infinite regress.

The **regress argument** has been used to justify one way out of the infinite regress, **foundationalism**.

Foundationalism claims that **there are some basic statements that do not require justification**.

Both induction and falsification are forms of foundationalism in that they rely on **basic statements** that derive directly from **observations**.

The way in which basic statements are derived from observation complicates the problem.

Observation is a cognitive act; that is, it relies on our existing understanding – our set of beliefs. An observation of a **transit of Venus** requires a huge range of auxiliary beliefs, such as those that describe the optics of telescopes, the mechanics of the telescope mount, and an understanding of celestial mechanics.

Prima facie, the observation does not appear to be 'basic'.

Coherentism offers an alternative by claiming that statements can be justified by their being a **part of a coherent system**.

In the case of science, the system is usually taken to be the complete set of beliefs of an individual or of the community of scientists.

W. V. Quine argued for a Coherentist approach to science. An observation of a transit of Venus is justified by its being coherent with our beliefs about optics, telescope mounts and celestial mechanics. Where this observation is at odds with one of these auxiliary beliefs, an adjustment in the system will be required to remove the contradiction.

Conceptual change and the development of science

The problem of **conceptual change** is one of those aspects of the scientific enterprise (along with formal structure of scientific theory) that have dominated **recent debate** in the philosophy of science. It is clear that the philosophical problems to which these two aspects give rise are correlative and complementary - the one being static, the other being dynamic.

The crucial question it poses is: "**What is a concept?**"

In the heyday of Logical Empiricism, that question had largely been disregarded.

Following the example of **Frege, the Viennese Positivists** had condemned any tendency to regard the philosophy of science as concerned with scientific thinking - which was in their view a matter for psychologists - and had restricted themselves to the **formal analysis of scientific arguments**.

This preoccupation with logic was also reflected in their view of concepts.

To interpret a concept such as force as referring either to a feeling of effort or to a mental image could lead, they argued, only to confusion.

Instead, **the philosopher must equate concepts with the terms and variables appearing in the propositional systems of science and define them, in part by reference to their roles in the formal structures of those propositional systems - thus fixing their systematic import - and in part by reference to the specific events and phenomena they are used to explain - thus fixing their empirical import.**

In the 1920s and 1930s, accordingly, all substantive philosophical questions about the concepts of science were simply translated into logical or linguistic questions about the **formal roles and empirical references of technical terms and mathematical variables**.

Once the philosophy of science is approached more historically, however, those substantive questions must be faced afresh in their own right. Rival scientific theories will now be distinguished not merely as so many alternative formal systems, based on different primitive terms and axioms, but also as **alternative ways of organizing the knowledge of nature**, based on different explanatory techniques and modes of representation.

The distinctive features of different scientific concepts will lie, as a result, not in their respective formal roles and empirical references but in the styles of explanatory procedure involved in their application.

Those procedures may be of many different kinds: e.g., physical conservation calculations, optical-ray diagrams, functional analyses, taxonomic classifications, historico-evolutionary reconstructions, or dynamical axiom systems.

The philosopher can describe the conceptual organization of the resulting explanations in terms neither of intuitive models nor of mathematical formulas and variables taken alone: what he must now consider is the entire pattern of theoretical interpretation - models, mathematics, and all.

Viewed from this alternative standpoint, the philosophy of science will begin by identifying the different styles of explanation characteristic of different sciences or of different stages in a given science and will recognize how those differences in explanatory style reflect the characteristic problems of different scientific fields and periods.

So considered, empirical generalizations and descriptive classifications will serve to organize the empirical data of science in a preliminary way; but serious theoretical interpretation can begin only after that point.

The central philosophical task now is to analyze, clearly and explicitly,

1. **the standards by appeal to which scientists have to decide whether or not some interpretation is legitimate, justified, and conclusively established and**
2. **the considerations that justify giving up one currently accepted interpretation in favour of an alternative, novel one.**

The **first** of these questions is one that the **Logical Empiricists** set out to answer in their own manner. They treated the empirical data and the theoretical principles of science as being connected by purely logical relations and attempted to define the required standards in terms of a **formal theory of confirmation, corroboration, or falsification.**

The **second** question is one that they never seriously tackled.

Instead, they assumed that one could, first, work out a quantitative index of acceptability for individual theories taken separately and, afterward, use this as a scale for measuring and comparing the merits of rival theoretical interpretations.

By now, however, it is evident that, when biophysicists, say, abandon one theoretical approach in favour of another - as being more fruitful from the standpoint of biophysics - the considerations that lead them to do so are by no means analyzable in formal terms alone.

On the contrary, the ability of a biochemist, say, to judge whether or not such a change in approach will effectively help to solve his theoretical problems is one of the most severe assessments of his substantive grasp of what biochemistry is about.

In this way**, the shift of attention from the propositions of science to its concepts** is making philosophers more aware of the extent to which theoretical understanding involves the reinterpretation of empirical results, not merely their formal transformation.

Similarly, the problem of conceptual change is raising questions about the processes by which theoretical interpretations succeed one another and about the procedures of conceptual judgment that are applied in the rational development of a science. These questions are currently under active discussion, and several lines of attack are being considered, none of which has finally established itself.

At one extreme, there are some who still regard **theoretical concepts and principles as organized into compact, logical systems** and who attempt to **define the alternative standpoints of different sciences as the consequences of different basic premises or presuppositions**.

Having adopted this systematic approach, the investigator then discovers that conceptual change at a fundamental level finds adequate scope only through the replacement of one complete formal system by another, distinct and separate successor system.

As a result, **fundamental theoretical change is, in this view, intelligible only as the outcome of thoroughgoing intellectual revolutions, in which one entire theoretical system - axioms, principles, criteria of relevance, standards of judgment, and all - is swept aside in favour of another.**

Alternatively, **there are those who**

distinguish two different kinds of fundamental principles in a science (marking off the basic theoretical assertions such as "Matter consists of atoms combining into molecules" from its methodological maxims and standards of judgment, such as "All physical phenomena are to be explained in mechanical terms") and who recognize fundamental conceptual changes in the science as legitimate, just so long as they respect the methodological maxims that are definitive of the science in question.

In this second view, **conceptual changes** of any depth in the intellectual substance of a science **will continue to be intelligible**, provided only that the new views are still governed by the established program and framework concepts of the science in question.

There will then be **revolutions** in science only when some entire intellectual approach is discredited or when some entirely new science is created, with its own complete system of interpretation.

At the other extreme, **there are some who** doubt whether any **sharp distinction** can be drawn between **substantive theoretical assertions and maxims of methodological procedure** and, who argue that all aspects of a natural science are alike open to historical reconsideration and modification.

The more specific the theoretical doctrines and concepts being considered, the more risky they will be, and the more readily they will be modified or abandoned or both.

From this third point of view, however, it is questionable whether any change is ever as discontinuous or revolutionary as the two former views imply.

Whichever alternative is adopted, one point must be kept in mind:

the moment that problems about the changing theoretical organization of science begin to be treated in an authentically developmental manner, philosophical inquiries are given a quite new direction.

This step compels one to view all questions about the logical structure and propositional systems of science against a **broader historical background**. In this new context the **natural sciences are seen not as static formal structures but as rational enterprises characterized by certain typical intellectual procedures or movements.**

Scientific discovery and the rationality of scientific thought

In analyzing the natural sciences for philosophical purposes **as historically developing enterprises**, the question "**What is it that makes the sciences rational**?" is raised in a new form:

Do the intellectual procedures that scientists actually employ to investigate and explain natural phenomena have definite and objective intellectual merits that make their adoption rationally prudent, wise, and obligatory?

In answering this question, philosophical opinion has tended to polarize in recent years toward **two extreme positions**:

On the one hand, a formalist or positivist extreme,

On the other, a romantic or irrationalist one.

Given their mathematical inspiration and preoccupations, both the Viennese Empiricists and their successors in Britain and the United States have interpreted the **rationality** of scientific procedures as depending solely on the **formal validity, or logicality, of scientific arguments**.

In their view, questions of rationality can be raised about the scientist's work only at the final stage in his inquiries. Only then, they declare, will there be anything about science that is capable of being criticized in logical or philosophical terms.

It is therefore a commonplace of recent Empiricist analysis in the philosophy of science that one must distinguish at the very outset between **discovery and justification**.

The term **discovery** refers to all the stages in a scientific inquiry preceding the formulation of the new explanatory arguments that are its final outcome.

The term **justification** refers, by contrast, to the demonstration that the formal validity or explanatory power of those arguments justifies the scientist in accepting their conclusions as scientifically validated or established.

In this view, **the rational concerns of the philosopher of science are restricted solely to this final phase of justification.**

All questions about the earlier stages - i.e., about discovery - are matters of mere psychology, not of serious philosophy.

As one widely accepted epigram expresses it, "There is no logic of discovery"; and this distinction - given the equation of rationality within logicality - seemingly invalidates all questions about the rationality of the preliminary steps by which a scientist arrives at a discovery.

At the opposite extreme, there are those, such as **Michael Polanyi**, a Hungarian-born scientist and philosopher, and **Arthur Koestler**, a novelist and journalist, who emphasize the parts played by **intuition, guesswork, and chance in scientific investigation**, citing these as evidence that theoretical achievement calls into play an intellectual creativity superior to mere rationality. According to this **anti-Positivist argument**, the modern scientist is a **sleepwalker** whose creative insight guides him to intellectual destinations that he could never clearly see or state beforehand.

In each of these extreme cases, however, the **initial equation of rationality with logicality** demands closer examination. Certainly, the activity of investigation and discovery can be examined with advantage from a psychological point of view. **Yet the possibility of such psychological inquiries does not obviously prove, entirely by itself, that procedures of intellectual investigation in science and mathematics are essentially nonrational**. Chance, for instance, may help to bring relevant material to a scientist's attention. But **chance** - as has often been remarked - **favours the prepared mind**.

Similarly, in the case of creative intuition and the rest: once again, the man with the best trained mind can afford to give the freest rein to his intellectual imagination because he will be best qualified to appraise the rational context of his current problems and to recognize significant clues, promising new lines of analysis, or possible answers to his questions, as they come to mind.

To find a **middle way between formalism and irrationalism**, it is necessary to look more closely at the nature of the problems of scientific inquiry.

If the improvement of scientific concepts and theories depends on the development of more powerful explanatory procedures, the **philosophical analysis of discovery** then requires that one show:

1. **what is essentially involved in devising such procedures,**
2. **testing them out, and**
3. **determining the range of their application.**

This problem must be dealt with, furthermore,

Not by a formal analysis of the resulting arguments alone

But first and foremost by establishing

what tasks any novel explanatory procedure in science can be required to perform,

what demands its performance can properly be asked to satisfy, and so

what intellectual goals a scientist is expected to be aiming at in all the phases of his investigations.

Posed in these alternative terms, **the problem of scientific rationality becomes a problem of showing how conceptual changes in science result in the introduction of novel ideas, which are - in a phrase coined by Mach as early as 1910 - "better adapted, both to the facts and to one another"**.

It is rational for **older scientific theories** to be displaced by **newer ones** that are **functionally superior**; **and the task for philosophers of science is to demonstrate explicitly in what such functional adaptedness consists.**

At the present time, many younger philosophers of science are actively analyzing the nature of the problems of science in these terms. Significantly, most of these men have had their own primary training within the natural sciences proper rather than in formal logic or pure mathematics, for the task requires a much more detailed analysis of the processes of intellectual innovation than has been customary hitherto.

In place of the simple dichotomy between discovery and justification, for instance, it calls for a subdivision of the innovation process into a more complex sequence of distinct stages; and at each stage both rational and causal considerations are relevant.

Thus, at the initial stage in any inquiry, a scientist must decide **which** among all of the philosophically conceivable variants from the current repertory of explanatory methods are to be taken seriously at all; **which**, that is, are genuine possibilities.

This preliminary sorting of initially plausible from implausible innovations must be dealt with - and dealt with in the most rational manner possible - long before any question of justification arises.

This initial sorting procedure is one about which scientists themselves also speak eloquently.

Far from deciding what novel suggestions are genuinely possible or plausible on a purely psychological basis or by the exercise of some mysterious, nonrational intuition, **scientists will commonly explain their reasons for accepting one set of conceptual variants rather than another** as deserving serious consideration. At the same time, such microanalyses of scientific innovation must certainly leave room for causal as well as rational questions.

During certain periods in the historical development of science, for instance, scientists have disregarded novel possibilities that later turned out to hold a key to the solution of crucial theoretical difficulties. Looking back at such periods, it is possible to reconstruct with care the rational considerations that might have been advanced at the time to explain this neglect; but even so, one is occasionally forced to conclude that the men involved were prejudiced against those possibilities by factors external to their sciences; e.g., by influences originating in the wider social, cultural, or political framework of their time.

Thus, Newton was particularly afraid that **his theory of material particles** might be accused of **supporting Epicureanism, whereas Darwin concealed his private speculations about the cerebral basis of mental activities because of public objections to Materialism**.

Scientific investigators working in different fields, or at different times, apparently face theoretical difficulties of quite different kinds.

One must therefore begin by studying the specific needs and tasks of each particular science, at one or another stage in its evolution, separately - seeking to recognize, in each individual case, the particular intellectual demands to be met by any new concept or theory if it is to be successful.

Eventually, the accumulated results of specific microanalyses may bring the investigator to a point at which he can again afford to generalize about all of the assorted theoretical problems confronting, say, physics and about the broader intellectual demands to be met by successful theoretical changes in a variety of scientific situations.

The unity of science (the project of unification of sciences)

As one notable illustration of the tug-of-war between logical and pragmatic issues in the philosophy of science, the "unity of science" movement may be cited.

Under the vigorous leadership of **Otto Neurath** this movement represented the high point in the ambitions of Viennese Positivism between World Wars I and II; for the general philosophical aims that motivated the search for a unified science are in striking contrast with the specific problem-solving considerations that lead working physicists to unify or integrate their theoretical concepts and explanatory procedures in actual scientific practice.

Aside from the primary test of **predictive success**, the Positivists of the Vienna Circle also did allow - on their own terms - for the further theoretical virtues of **coherence and comprehensiveness**.

Their **logico-mathematical approach** to the propositional structure of scientific theories, however, led them to interpret this demand for coherent and comprehensive theories in a formal sense.

On their interpretation, a totally unified body of scientific ideas would be a

- **comprehensive,**
- **quasi-Euclidean system of scientific theorems,**
- **based on a single set of**
    - **general axioms,**
    - **postulates, and**
    - **primitive propositions and**
- **applicable to natural phenomena of all kinds**.

Given sufficiently all-embracing empirical generalizations as the starting points of such a unified science, it would then be possible, in their view, to deduce particular statements about all the phenomena covered by the varied special sciences unified within its axiomatic scope.

Taking the **symbolic logic of Russell and Whitehead** as their **formal core**, philosophical advocates of the unity of science then set out to construct, on a single axiomatic pattern, a fully comprehensive account of nature capable of explaining all natural phenomena whatsoever.

At first glance, this ambition seemed legitimate, but once again the Empiricist program subsequently encountered unforeseen entanglements.

The reasons for this situation were not merely the discovery that the theoretical ideas employed within different branches of a science (e.g., of mathematical physics) are more resistant to conceptual integration than had originally been hoped (**the task of constructing a self-consistent relativistic theory of quantum electrodynamics, for instance, is one that still defeats the physicists**);

But, what is worse, it has now become apparent that several well-founded and properly respected branches of scientific theory do not lend themselves to exposition in a formal mathematical manner at all. Any satisfactory theory of **organic evolution**, for instance, has an irreducibly **historical dimension**; and there is no possibility of putting historical zoology on the sort of predictive basis that Empiricists have demanded, still less of incorporating it into Neurath's larger unified axiom system.

Faced with this particular example, indeed, one distinguished Empiricist philosopher, **Carl Hempel**, has drawn a somewhat extreme conclusion, viz., that **the theory of natural selection is not really an explanation of organic evolution at all - not even a bad one - but is merely an elaborate redescription of the historical episodes concerned**.

If, on the other hand, the demand for integration or unification is considered as a practical problem of methodology, it will then be found that the scientists are facing problems of a different and more pragmatic kind.

The science of **physiology** poses an interesting example because, within this field, the problem of reductionism - i.e., of whether all phenomena whatsoever can be reduced to physico-chemical terms alone - has repeatedly drawn active debate.

To sum up:

In the **methodological drive toward the unification of the sciences**, as in the earlier phases of discovery and validation, the intellectual temptation to generalize prematurely exposes the philosopher to certain real dangers.

In practice, the case for unifying the theories and concepts of two or more sciences has to be considered afresh in every instance, and **it can rarely be decided in advance whether or not such a unification will achieve anything useful for the sciences**.

Instead, one has to analyze the practical demands of the current problems in the different fields and see how far those requirements can be met by developing a unified explanatory treatment for all of the special sciences in question.

The integration of theoretical concepts achieved in the process will not consist solely in the formal running together of different propositional systems: more typically, it will require the development of a whole new pattern of theoretical interpretation. And, though it may be possible, in certain cases, to expound the resulting theory in axiomatic form, it must be established, in each case separately, whether or not this can be done.

In this sense, conceptual and methodological unification represents a genuine movement in the development of scientific thought; **but the logical form** of the unified science towards which the philosopher is working is not something that he can lay down definitively before the event.

From philosophical analysis to scientific practice

The arguments about these rival ontological and epistemological views cannot be safely left or judged without first looking more closely at the complex relationship between the **general analytical interests of philosophers** and the more **specific intellectual concerns of working scientists themselves**.

For the degree to which each view about the reality of scientific entities and facts can carry conviction depends substantially on what branches of science are at issue. As the focus of philosophical attention has shifted historically from one scientific terrain to another, so, too, have the relative degrees of plausibility of these rival positions varied.

Since the 1920s, for instance, there has been a marked **revival of philosophical discussion among scientists working in several specialized fields** - particularly, among physicists concerned with the structure and development of quantum mechanics.

In epistemic terms, the **statistical character of quantum-mechanical explanations has prompted some fundamental questions about the status and limitations of human knowledge**.

Clearly, the extent and accuracy of human knowledge about nature are limited by the modes of operation of scientific instruments.

Is it not also possible, however, **that the significance of this statistical character lies at a deeper level**?

Perhaps the relevant objective relationships and states of affairs in nature itself are governed intrinsically by a merely probabilistic causality and so are essentially indeterminate;

Or,

Is there a point to be reached on the microphysical level at which any such **distinction between subjective human knowledge and the objective state of affairs has finally broken down**?

The **ontal implications of quantum mechanics** have been as puzzling as the **epistemic**.

For example, in the **psychology of perception** and related fields, the extension of understanding in recent years at last has permitted the framing of authentically empirical questions about perception and cognition, which lend themselves to direct investigation instead of being restricted to general a priori speculations. The result has been a theoretical debate, the final outcome of which will have profound effects on both philosophical epistemology and natural science.

In areas of this debate where even **Mach** was content to pose entirely general questions, in the philosophical tradition of **David Hume**, about the role of sense impressions as the raw material of all cognition and perception whatsoever, it is now clear that many preliminary differences and complexities must be unravelled before one can hope to recognize the truly operative questions in this field.

Thus, at the present time, the investigations of some physiologists, psychologists, and cyberneticists are bringing man's sensory and cognitive activities within the scope of natural science while at the same time preserving a feeling for the more **general philosophical problems and insights of such philosophers as Locke and Leibniz, Hume and Kant, Helmholtz and Mach**.

So long as philosophical discussion is confined within the limits of an **artificial, ideal language or propositional system**, it is possible, perhaps, to continue posing purely abstract, general dilemmas about, say, **theoretical entities or confirmation theory**.

But the bearing of such formal dilemmas on the actual content of contemporary scientific thought is becoming increasingly unclear. In debating the ontal status of theoretical entities, for instance, the question must at some stage be faced whether that phrase is intended to cover such notions as **gene or pi-meson, momentum or superego, social class or economic market**. (Certainly**, not all of these terms have identical characters and functions**.)

In debating the epistemic status of scientific theories, likewise, it must be made clear whether one has in mind, say, the **mathematical schema of quantum-mechanical field theory, the populational analysis of natural selection, the microstructures and mechanisms of molecular biology, the developmental sequences of cognitive psychology, the labour theory of economic value, the general regularities of terrestrial meteorology, or what**. (Once again, **not all of these theories have identical kinds of status or implications**.)

Philosophical doctrines and approaches that carry great conviction when applied to the theories and ideas of one science may - not surprisingly - lose all of their plausibility when extended to other fields.

Thus, an Empiricist analysis may apply quite straightforwardly to meteorology, yet entirely misrepresent the structure and implications of electromagnetic theory; while, in return, a Neo-Kantian account of theoretical physics may lack any direct relevance, say, to ideas about animal behaviour.

Today as in classical Athens, **analytical clarification** in the philosophy of science goes, in this respect, hand in hand with **methodological refinements** in the sciences themselves.

In retrospect, the methodological insights of **Aristotle the marine biologist** and of **Plato the theoretical astrophysicist** can be seen to have been complementary, rather than incompatible.

Similarly, today, the philosopher must look at rival positions in the philosophy of science not merely as **contradictory answers** to technical questions within philosophy itself but equally as **complementary contributions** to the methodological improvement of theoretical understanding over the whole varied range of different scientific fields.

Interrelationships of science and other fields of the culture

This survey has been concerned, almost exclusively, with philosophical problems and arguments about the **sciences regarded as sources of theoretical knowledge**. In putting **Realism against Instrumentalism, mechanistic ideas against organicist ones, divine knowledge against human fallibility, or Platonic Ideas against Aristotelian essences**, the philosopher is in each case concerned with the **intellectual status, implications, and validity of certain general scientific concepts, methods, or entities.**

To confine oneself entirely to these intellectual aspects, however, would mean accepting a total abstraction of theory from practice and of scientific ideas from their behavioral expression. Thus, along with the present-day shift of emphasis from the physical to the human and social sciences, one finds that all such abstract approaches are coming once again under criticism, as over-intellectualizing the nature and implications of science.

Some of these attacks come from the **neo-Marxist** direction and reflect a traditional Marxian insistence on the **unity of theory and action**.

Analogous criticisms, however, are also coming from men with very different intellectual loyalties - e.g., from the urban sociologist **Lewis Mumford** and from **many contemporary Existentialists**.

In conclusion, therefore, a concise discussion is here given of some of the views about the relations between science and the rest of culture; i.e., about the **relevance of scientific knowledge to other spheres of experience and concern and, conversely, about the significance of broader, practical considerations for man's understanding of scientific theory itself.**

The **variety of these views** has always been very great.

Their exponents have ranged all the way:

From those who, like the energeticist **Wilhelm Ostwald** and the evolutionist **Julian Huxley** - both of whom rooted ethics in nature - present **scientific ideas and procedures as rational panaceas for intellectual and practical problems of all kinds.**

To those who, like **Pierre Duhem** and **Carl von Weizsäcker**, physicist and philosopher of nature, both of whom are theists, deliberately **limit the claims of science so as to preserve a freedom of manoeuvre for ethics, for example, or theology.**

Whatever one's general philosophical position with respect to the reality of scientific knowledge and entities may be, however, **there are other more practical questions to be faced**, questions about the **specific implications of different scientific ideas and beliefs for parallel fields of human action and experience**.

On this point, one particular theme unites a wide range of radical critics of science, including both **Lewis Mumford**, U.S. social critic**, and the Existentialists**.

Just as the Christian Dane **Søren Kierkegaard** condemned Kant's universalized system of ethics for ignoring the individuality of actual ethical problems and decisions,

So today there is a widespread reaction against any tendency to treat social or practical decisions as technical matters, which can be left to the judgment of scientific or technological experts.

The general methods of technology may, indeed, represent practical applications of the theoretical understanding arrived at by science.

But all individual decisions about putting those general techniques to use - e.g., in constructing an airport or power station - must be made not by appealing to any general formula or rule of thumb but by balancing a whole range of diverse considerations - economic and aesthetic, environmental and human, as well as merely technical.

According to another contemporary critique, the **theoretical points of view** adopted in **natural science** are **general and abstract**, but the **practical demands of sociopolitical action** and **of individual action, are concrete and particular**; and, by itself, this contrast places an immediate restriction on the existential relevance of scientific ideas and engineering techniques.

Such scholars as **Thomas Huxley or Wilhelm Ostwald**, who viewed reality as essentially energy, might argue in general, abstract terms for interpreting ethical principles in evolutionary or thermodynamical terms if they pleased (so the critics continue); but such abstract speculative arguments have no bearing on the actual tasks of ethical decision and action. Here again, **every ethical choice involves a unique constellation of considerations and demands**; and this problem cannot be dealt with by appealing to any universal rule but must be appraised on an individual basis, as and when it arises.

Others take a more **positive approach toward the contribution of science to an understanding of human values**. Without necessarily claiming to transform ethics itself into a "**science**," they at any rate argue that the personal attitudes needed for effective work in science - adventurous skepticism and critical open-mindedness - have a wider relevance also to human conduct and social affairs.

On the one hand, there has recently been a revival of explicitly **anti-scientific views**, which had been more or less dormant since the time of **William Blake, Johann Wolfgang von Goethe, and their successors in the Romantic movement**.

Supporters of this anti-science position point to the central role of military technology in the financial support of 20th-century scientific research and dismiss the average scientist's plea that he is not responsible for the uses to which his ethically neutral discoveries are put, as pallid and insincere.

On the contrary (they argue), there is a long-standing and unholy alliance linking the collective institutions of the scientific and technological professions to the economic, industrial, and political powers.

Faced with the fruits of this historical union (they conclude), **it is time that scientists acknowledged their social responsibilities.** Perhaps man already knows too much for his own good and needs to digest the significance of his existing stock of knowledge much further before adding to it and so **widening yet again the gulf between theoretical knowledge and practical wisdom**.

On the other hand, there are those who recognize science as playing a crucial role in modern society, but who go on to draw the opposite conclusion. Rather than putting a stop to science (these men would argue), **its scope should be broadened**; that is, scholars should be studying and understanding better the **manner in which science serves as an element in the larger social order** - perhaps by developing more adequate analyses of the social structure or perhaps by a large-scale extension of the methods of operations research.

As compared with the controversies of earlier centuries, the **debate between science and religion** is curiously muted today.

In earlier times, the term **cosmology** **embraced not only the structure of the astronomical cosmos and the origins of the human species but also the religious significance of man's place in nature.**

Contemporary theologians, by contrast, see physics and biology as having much less bearing on man's religious attitudes and preoccupations than their predecessors had supposed that they had.

As a result, men's earlier ambition to construct a single, comprehensive worldview, embracing the essential truths of both science and religion, no longer plays the active part in intellectual life that it formerly did.

The only branches of science still capable of provoking vigorous theological debate, even now, are the human, rather than the natural sciences. The implications of **Freudian psychology** for the doctrine of grace and the use of psychedelic drugs for inducing quasi-mystical experiences are topics for live discussion today, in a way that evolution, astrophysics, and historical geology no longer are.

This change of focus has been accompanied by a change in ideas about the intrinsic limits of science.

It was formerly assumed that the boundaries between science and other aspects of human experience could be defined by marking off certain types of subject matter as essentially closed to scientific investigation.

To one generation, the heart of this forbidden territory was the mind; to another, it was life; to a third, the creation.

In this view, something in the essential nature of mental or vital activities, or in the origins of the present order of nature, made it impossible to treat these as phenomena open to study and explanation by the rational methods and intellectual procedures available to science. In fact, this view always had defects, from both the scientific and the theological points of view. To scientists, it seemed to impose an arbitrary restriction on their sphere of operations and so acted as a standing challenge and irritation. For theologians, it had the disadvantage of placing the essential claims of religion, so to speak, on a sandbank, where they risked being submerged in time by the rising tide of scientific knowledge.

So, by tacit consent, the essential limits of science are now defined in quite different terms. **These limits are now identified by recognizing that the character of scientific procedures themselves places restrictions on the relevance of their results.**

A scholar may choose to study whatever objects, systems, or processes he may please, but only certain of the questions that he asks about them will be answerable in the general, theoretical terms characteristic of science.

This change of approach may not have made the substantive problem - that of **delimiting the frontiers of science exactly at all points** - very much easier to deal with than it was before, but **it has one genuine merit**: it respects the crucial fact, to which attention has been drawn at several points in this present survey, that **the distinctive features of science lie not in the types of object and event to which the scientist has access but in the intellectual procedures that his investigations employ and so in the kinds of problem that lend themselves to a scientific solution.**

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