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THEORIES OF SCIENTIFIC METHOD

Models for the Physico-Mathematical Sciences

Nancy Cartwright, Stathis Psillos, and Hasok Chang

Scientific methods divide into two broad categories: inductive and deductive. Inductive methods arrive at theories by generalizing from what is known to happen in particular cases; deductive methods, by derivation from first principles. Behind this primitive categorization lie deep philosophical oppositions. The first principles central to deductivist accounts are generally taken to be, as Aristotle described, "first known to nature" but not "first known to us." Do the first principles have a more basic ontological status than the regularities achieved by inductive generalization – are they in some sense "more true" or "more real"? Or are they, in stark opposition, not truths at all, at least for a human science, because always beyond the reach of human knowledge?

Deductivists are inclined to take the first view. Some do so because they think that first principles are exact and eternal truths that represent hidden structures lying behind the veil of shifting appearances; others, because they see first principles as general claims that unify large numbers of disparate phenomena into one scheme, and they take unifying power to be a sign of fundamental truth.[1] Empiricists, who take experience as the measure of what science should maintain about the world, are suspicious of first principles, especially when they are very abstract and far removed from immediate experience. They generally insist on induction as the gatekeeper for what can be taken for true in science.

Deductivists reply that the kinds of claims we can arrive at by generalizing in this way rarely, if ever, have the kind of precision and exceptionlessness that we require of exact science; nor are the concepts that can be directly tested in experience clear and unambiguous. For that we need knowledge that is expressed explicitly in a formal theory using mathematical representations and theoretical concepts not taken from experience. Those who maintain the centrality of implicit knowledge, who argue that experiment and model

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[1] For defense of the importance of unification, cf. P. Kitcher, "Explanatory Unification," *Philosophy of Science*, 48 (1981), 507–31.

building have a life of their own only loosely related to formal theory, or who aim for the pragmatic virtues of success in the mastery of nature in contrast to an exact and unambiguous representation of it, look more favorably on induction as the guide to scientific truth.

The banners of inductivism and deductivism also mark the divide between the great traditional doctrines about the source of scientific knowledge: empiricism and rationalism. From an inductivist point of view, the trouble with first principles is in the kind of representations they generally involve. The first principles of our contemporary physico-mathematical sciences are generally expressed in very abstract mathematical structures using newly introduced concepts that are characterized primarily by their mathematical features and by their relationships to other theoretical concepts. If these were representations taken from experience, inductivists would have little hesitation in accepting a set of first principles from which a variety of known phenomena can be deduced. For induction and deduction in this case are just inverse processes. When the representations are beyond the reach of experience, though, how shall we come to accept them? Empiricists will say that we should not. But rationalists maintain that our capacity for thought and reason provide independent reasons. Our clear and distinct ideas are, as René Descartes maintained, the sure guide to truth; or, as Albert Einstein and a number of late-twentieth-century mathematical physicists urge, the particular kind of simplicity, elegance, and symmetry that certain mathematical theories display gives them a purchase on truth.

These deeper questions, which drive a wedge between deductivism and inductivism, remain at the core of investigation about the nature of the physico-mathematical sciences. They will be grouped under five headings below: I. Mathematics, Science, and Nature; II. Realism, Unity, and Completeness; III. Positivism; IV. From Evidence to Theory; V. Experimental Traditions. It is usual in philosophy to find that the principal arguments that matter to current debates have a long tradition, and this is no less true in theorizing about science than about other topics. Thus, an account of contemporary thought about scientific method for the physico-mathematical sciences necessarily involves discussion of a number of far older doctrines.

MATHEMATICS, SCIENCE, AND NATURE

How do the claims of mathematics relate to the physico-mathematical sciences? There are three different kinds of answers:

Aristotelianism[2]

Quantities and other features studied by mathematics occur in the objects of perception. The truths of mathematics are true of these perceptible

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[2] Cf. Aristotle, *Metaphysics* μ–3.

quantities and features, which are further constrained by the principles of physics. Thus, Aristotle can explain how demonstrations from one science apply to another: The theorems of the first science are literally about the things studied in the second science. The triangle of optics, for instance, is a perceptible object and as such has properties like color and motion. In geometry, however, we take away from consideration what is perceptible (by a process of *aphairesis* or abstraction) and consider the triangle merely "*qua* triangle." The triangle thus considered is still the perceptible object before us (and need not be in the mind), but it is an object of thought.

This doctrine allows Aristotelians to be inductivists. The properties described in the first principles of the mathematical sciences literally occur in the perceptible world. Yet it dramatically limits the scope of these principles. How many real triangles are there in the universe, and how does our mathematics apply where there may be none at all, for example, in the study of rainbows? The same problem arises for the principles of the sciences themselves. Theories in physics are often about objects that do not exist in perceptible reality, such as point masses and point charges. Yet these are the very theories that we use to study the orbits of the planets and electric circuits. The easy answer is that the perceptible objects are "near enough" to being true point masses or true triangles for it not to matter. But what counts as near enough, and how are corrections to be made and justified? These are the central issues in the current debate among methodologists over "idealization" and "de-idealization."[3]

Pythagoreanism

Many modern physicists and philosophers (Albert Einstein being a notable example) maintain, with the early Pythagoreans, that nature is "essentially" mathematical. Behind the phenomena are hidden structures and quantities. These are governed by the principles of mathematics, plus, in current-day versions, further empirical principles of the kind we develop in the physico-mathematical sciences. Some think that these hidden structures are "more real" than what appears to human perception. This is not only because they are supposed to be responsible for what we see around us but, more importantly, because the principles bespeak a kind of necessity and order that many feel reality must possess. Certain kinds of highly abstract principles in modern physics are thought to share with those of mathematics this special necessity of thought.

Pythagoreanism is a natural companion to rationalism. In the first place, if a principle has certain kinds of special mathematical features – for example, if the principle is covariant or it exhibits certain abstract symmetries – that is supposed to give us reason to believe in it beyond any empirical evidence. In the second, many principles do not concern quantities that are measurable

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[3] Cf. the series *Idealization* I–VIII in *Poznan Studies in the Philosophy of the Sciences and the Humanities,* ed. J. Brzezinski and L. Nowak, etc. (Amsterdam: Rodopi, 1990–7).

in any reasonable sense. For instance, much of modern physics studies quantities whose values are not defined at real space-time points but instead in hyperspaces. Pythagoreans are inclined to take these spaces as real. It is also typical of Pythagoreans to discuss properties that are defined relative to mathematical objects as if they were true of reality, even when it is difficult to identify a measurable correlate of that feature in the thing represented by the mathematical object. (For example, what feature must an observable have when the operator that represents it in quantum mechanics is invertible?) Current work in the formal theory of measurement develops precise characterizations of relationships between mathematical representations on the one hand and measurable quantities and their physical features on the other, thus providing a rigorous framework within which these intuitive issues can be formulated and debated.[4]

Instrumentalism and Conventionalism

The French philosopher, historian, and physicist Pierre Duhem (1861–1916) was opposed to Pythagoreanism. Nature, Duhem thought, is purely qualitative. What we confront in the laboratory, just as much as in everyday life, is a more or less warm gas, Duhem taught.[5] Quantity terms, such as "temperature" (which are generally applied through the use of instruments), serve as merely *symbolic* representations for collections of qualitative facts about the gas and its interactions. This approach makes Duhem an instrumentalist both about the role of mathematics in describing the world and about the role of the theoretical principles of the physico-mathematical sciences: These serve not as literal descriptions but, rather, as efficient instruments for systematization and prediction. The methods for coming to an acceptance or use of the theoretical principles of physics, then, will clearly not be inductive. Duhem advocated instead the widely endorsed hypothetico-deductive method. He noted, however, that the method is, by itself, of no help in confirming hypotheses, a fact which lends fuel to instrumentalist doctrines (see the section "From Evidence to Theory"). Duhem's arguments still stand at the center of debate about the role of mathematics in science.

Alternative to the pure instrumentalism of Duhem is the conventionalism of his contemporary, Henri Poincaré (1854–1912), whose work on the foundations of geometry raised the question "Is physical space Euclidean?" Poincaré took this question to be meaningless: One can make physical space possess *any* geometry one likes, provided that one makes suitable adjustments to one's physical theories. To show this, Poincaré described a possible world in which the underlying geometry is indeed Euclidean, but due to the existence of a strange physics, its inhabitants conclude that the geometry of their world is non-Euclidean. There are then two empirically equivalent theories

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[4] See, for instance, D. H. Krantz, R. D. Luce, P. Suppes, and A. Tversky, *Foundations of Measurement* (New York: Academic Press, 1971).

[5] P. Duhem, *Aim and Structure of Physical Theory* (New York: Atheneum, 1962).

to describe this world: Euclidean geometry plus strange physics versus non-Euclidean geometry plus usual physics. Whatever geometry the inhabitants of the world choose, it is not dictated by their empirical findings. Consequently, Poincaré called the axioms of Euclidean geometry "conventions."

Poincaré's conventionalism included the principles of mechanics as well.[6] They cannot be demonstrated independently of experience, and they are not, he argued, generalizations of experimental facts. For the idealized systems to which they apply are not to be found in nature. Nor can they be submitted to rigorous testing, since they can always be saved from refutation by some sort of corrective move, as in the case of Euclidean geometry.

So, Poincaréan conventions are held true, but their truth can be established neither a priori nor a posteriori. Are they then held true merely by definition? Poincaré repeatedly stressed that it is experience that "suggests," or "serves as the basis for," or "gives birth to" the principles of mechanics, although experience can never establish them conclusively. Nevertheless, like Duhem and unlike either the Aristotelians or the Pythagoreans, for Poincaré and other conventionalists the principles of geometry and the principles of physics serve as symbolic representations of nature, rather than literally true descriptions (see the next section).

REALISM, UNITY, AND COMPLETENESS

These are among the most keenly debated topics of our day. One impetus for the current debates comes from the recent efforts in the history of science and in the sociology of scientific knowledge to situate the sciences in their material and political setting. This work reminds us that science is a social enterprise and thus will draw on the same kinds of resources and be subject to the same kinds of influences as other human endeavors. Issues about the social nature of knowledge production, though, do not in general make special challenges for the physico-mathematical sciences beyond those that face any knowledge-seeking enterprise and, hence, will not be focused on here.

For many, knowledge claims in the physico-mathematical sciences do face special challenges on other grounds: (1) The entities described are generally unobservable. (2) The relevant features are possibly unmeasurable. (3) The mathematical descriptions are abstract; they often lack visual and tangible correlates, and thus, many argue with Lord Kelvin and James Clerk Maxwell, we cannot have confidence in our understanding of them.[7] (4) The theories

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[6] Cf. H. Poincaré, *La Science et L'Hypothèse* (Paris: Flammarion, 1902).

[7] See C. Smith and M. N. Wise, *Energy and Empire: A Biographical Study of Lord Kelvin* (Cambridge: Cambridge University Press, 1989), and J. C. Maxwell, "Address to the Mathematical and Physical Section of the British Association," in *The Scientific Papers of James Clerk Maxwell,* ed. W. D. Niven, 2: 215–29; *Treatise on Electricity and Magnetism,* vol. 2, chap. 5.

often seem appropriate as descriptions only of a world of mathematical objects and not of the concrete things around us. These challenges lie at the core of the "realism debate."

On a *realist* account, a theory purports to tell a literally true story as to how the world is. As such, it describes a world populated by a host of unobservable entities and quantities. Instrumentalist accounts do not take the story literally. They aim to show that all observable phenomena can be embedded in the theory, which is then usually understood as an uninterpreted abstract logico-mathematical framework. Currently another view has been gaining ground.[8] One may, with the realist, take the story told by the theory literally: The theory describes how the world might be. Yet, one can at the same time suspend one's judgment as to the truth of the story. The main argument for this position is that belief in the truth of the theoretical story is not required for the successful use of the theory. One can simply believe that the theory is empirically adequate, that is, that it saves all observable phenomena. It should be noted that "empirically adequate" here is to be taken in a strong sense; if we are to act on the theory, it seems we must expect it to be correct not only in its descriptions of what has happened but also about what will happen under various policies we may institute.

Realists argue that the best explanation of the predictive successes of a theory is that the theory is true. According to the *inference to the best explanation,* when confronted with a set of phenomena, one should weigh potential explanatory hypotheses and accept the best among them as correct, where "bestness" is gauged by some favored set of virtues. The virtues usually cited range from very general ones, such as simplicity, generality, and fruitfulness, to very subject-specific ones, such as gauge invariance (thought to be important for contemporary field theory), or the satisfaction of Mach's principle (for theories of space and time), or the exhibition of certain symmetries (now taken to be a sine qua non in fundamental particle theories).

Opponents of realism urge that the history of physics is replete with theories that were once accepted but turned out to be false and have been abandoned.[9] Think, for instance, of the nineteenth-century ether theories, both in electromagnetism and in optics, of the caloric theory of heat, of the circular inertia theories, and of the crystalline spheres astronomy. If the history of science is the wasteland of aborted best explanations, then current best theories themselves may well take the route to this wasteland in due course.

Realists offer two lines of defense, which work in tandem. On the one hand, the list of past theories that were abandoned might not after all be very big, or very representative. If, for instance, we take a more stringent account of empirical success – for example, we insist that theories yield novel predictions – then it is no longer clear that so many past abandoned

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[8] See especially B. C. van Fraassen, *The Scientific Image* (Oxford: Clarendon Press, 1980).

[9] Cf. L. Laudan, "A Confutation of Convergent Realism," *Philosophy of Science,* 48 (1981), 19–49.

theories were genuinely successful. In this case, the history of science would not after all give so much reason to expect that those of our contemporary theories that meet these stringent standards will in their turn be abandoned.

On the other hand, realists can point to what in theories is not abandoned. For instance, despite the radical changes in interpretation, successor theories often retain much of the mathematical structure of their predecessors. This gives rise to a realist position much in sympathy with the Pythagoreanism discussed in the first section. According to "structural realism," theories can successfully represent the *mathematical structure* of the world, although they tend to be wrong in their claims about the entities and properties that populate it.[10] The challenge currently facing structural realism is to defend the distinction between *how an entity is structured* and *what this entity is.* In general, realists nowadays are at work to find ways to identify those theoretical constituents of abandoned scientific theories that contributed essentially to their successes, separate these from others that were "idle," and demonstrate that the components that made essential contributions to the theory's success were those that were retained in subsequent theories of the same domain. The aim is to find exactly what it is most reasonable to be a scientific realist about.

Closely connected with, but distinct from, realism are questions about the *unity* – or unifiability – of the sciences and about the *completeness* of physics. It is often thought that if the theories of physics are true, they must fix the behavior of all other features of the material universe. Thus, unity of the sciences is secured via the reducibility of all the rest to physics. Opposition views maintain that basic theories in physics may be true, or approximately so, yet not *complete:* They tell accurate stories about the quantities and structures in their domains, but they do not determine the behavior of features studied in other disciplines, including other branches of physics.[11] Whether reductions of one kind or another are possible "in principle," there has over the last decade been a strong movement that stresses the need for *pluralism* and interdisciplinary cooperation in practice.[12]

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[10] Cf. J. Worrall, "Structural Realism: The Best of Both Worlds?" *Dialectica,* 43 (1989), 99–124; P. Kitcher, *The Advancement of Science* (Oxford: Oxford University Press, 1993); S. Psillos, "Scientific Realism and the 'Pessimistic Induction,'" *Philosophy of Science,* 63 (1996), 306–14.

[11] For classic loci of these opposing views, see P. Oppenheim and H. Putnam, "Unity of Science as a Working Hypothesis," in *Concepts, Theories and the Mind-Body Problem,* ed. H. Feigl, M. Scriven, and G. Maxwell (Minneapolis: University of Minnesota Press, 1958), pp. 3–36; and J. Fodor, "Special Sciences, or the Disunity of Science as a Working Hypothesis," *Synthese,* 28 (1974), 77–115. For contemporary opposition to doctrines of unity, see J. Dupré, *The Disorder of Things: Metaphysical Foundations of the Disunity of Science* (Cambridge, Mass.: Harvard University Press, 1993); for arguments against completeness, see N. Cartwright, *The Dappled World* (Cambridge: Cambridge University Press, 1999).

[12] Cf. S. D. Mitchell, L. Daston, G. Gigerenzer, N. Sesardic, and P. Sloep, "The Why's and How's of Interdisciplinarity," in *Human by Nature: Between Biology and the Social Sciences,* ed. P. Weingart et al. (Mahwah, N.J.: Erlbaum Press, 1997), pp. 103–50, and S. D. Mitchell, "Integrative Pluralism," *Biology and Philosophy,* forthcoming.

POSITIVISM

All varieties of positivism insist that *positive knowledge* should be the determinant of what practices and claims are accepted in science. Differences arise over two issues: (a) What is positive knowledge? and (b) What are the principles of determination? We shall focus on the Vienna Circle here since most of the positivist legacy in current Anglo-American thinking about science has been inherited through it.[13] The Vienna Circle offered special forms of the two dominant kinds of answers to both questions.

Members of the Circle met in Vienna from 1925 until the group was broken up by Nazi oppression in 1935. Their ideas were influenced by the new physics, particularly Einstein's theory of relativity. A number of Circle members, especially Otto Neurath (1882–1945) and Edgar Zilsel (1891–1944) and to a lesser extent Rudolf Carnap (1891–1970), were politically active and held strong socialist views. In general, they saw their belief in socialism and their advocacy of a scientific style of philosophy as closely allied. (Neurath, for instance, embraced a scientifically interpreted version of Marxist materialism.)

What is positive knowledge? It is knowledge of what can be really known, where "what can be really known" is what happens in the real world. But how shall we characterize the kinds of things that happen in the real world? This problem arises as much for the physicalism and philosophical naturalism of the 1990s as it did for earlier positivists. Physicalism maintains that all true descriptions of the world are fixed by the physical descriptions true of it – the main target of concern being mental states and emotions and the features and norms of social groups. Its companion, philosophical naturalism, urges that philosophy has no special subject matter other than what is already studied in science. But what constitutes a physical description, or the proper subject matter of science?

The positivism of the Vienna Circle took a double stand: a materialist "metaphysics" and a "verificationist" epistemology. Their materialism dictated either that all there is is what physics studies ("physics-ism"), or that what there is is what occurs in space and time ("physicalism"). Their verificationism dictated that what is really true is what can be verified in experience. By taking these stands, they aimed to rule out from the realm of positive knowledge both religion and Hegelian idealism. Religion was attacked for its mystical characters and moral injunctions; Hegelian idealism, for its philosophical obscurities, its realm of pure ideas, and its teleological account of the history of humanity; and both, for their contempt for the physico-mathematical sciences.

Both of these stands were motivated by the positivists' aim to answer the question of what can be really known. The central epistemic problem

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[13] For a general discussion of the logical positivists, see T. Uebel, ed., *Rediscovering the Forgotten Vienna Circle: Austrian Studies on Otto Neurath and the Vienna Circle* (Dordrecht: Kluwer, 1991).

is whether knowledge is conceived of as private or as public. Traditional empiricism assumes that all one can really be sure of are facts about one's own experience. Thus, Ernst Mach's (1838–1916) defense of a positivist reading of physics is titled *The Analysis of Sensations.* Following John Locke, George Berkeley, and David Hume, it also assumes that the only concepts that can be meaningfully spoken of should be built out of sensory experience. Notoriously, Hume (1711–1776) used this restriction to undermine the concept of causality, the concept of one thing's making another happen in contrast to that of mere regular association. Many modern positivists continue this attack. They insist that physics has no place for causality. This is not just because causality is not part of our observable experience but also because of the "theory-dominated" assumption that physics knowledge equals physics equations (an assumption that excludes knowledge of how things work) and that physics equations record mere association. Concerns about causality in physics have become prominent recently, both because of the possibility of nonlocal causal influences in quantum mechanics raised by J. S. Bell's work on the Einstein-Podolsky-Rosen experiment and because of a renewed interest in how physics is put to work to intervene in the world.[14]

On the side of the private view of knowledge is the claim that our individual experiences are the only plausible candidates for nonanalytic knowledge of which we can be certain; and if we do not found our scientific claims in something of which we can be reasonably certain, we have no genuine claim to knowledge at all. The entire edifice of modern knowledge, even in physics and other exact sciences, may be a chimera. Opposed to this is the view that knowledge is necessarily a public, cooperative enterprise to which a great number of persons must contribute and of which a single person can possess only a minuscule part. This claim, which is clearly closer to science as we see it practiced, is one of the central tenets of studies in the sociology of knowledge of the 1980s and 1990s. The public view of knowledge can also count on its side the private-language argument, in establishing that the idea of private knowledge does not make sense.[15]

FROM EVIDENCE TO THEORY

What are the principles that allow us to deduce higher-level knowledge from lower? Rudolf Carnap first proposed an *Aufbau* – a way to construct new knowledge from some given positive base methodically, whether the base is

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[14] J. S. Bell, *Speakable and Unspeakable in Quantum Mechanics* (Cambridge: Cambridge University Press, 1987); M. S. Morgan and M. C. Morrison, eds., *Models as Mediators* (Cambridge: Cambridge University Press, 1999).

[15] L. Wittgenstein, *Philosophical Investigations,* 3d ed. (New York: Macmillan, 1958); see also S. A. Kripke, *Wittgenstein on Rules and Private Language* (Oxford: Blackwell, 1982).

private or public.[16] But many believe that scientific knowledge clearly goes far beyond a mere reassemblage of what is given in the positive base. Carnap himself later offered a theory of confirmation to show how and to what degree evidence can make further scientific hypotheses probable, and the hunt for a viable theory of confirmation is still on.[17] The problem is to find something that can fix the probability. Carnap took the probabilistic relation between evidence and hypotheses to be a logical one; hence, "inductive logic." One of the troubles with inductive logics, from Carnap till now, is that they require that the evidence and hypotheses be expressed in a formal language. Some view the requirement of formality as an advantage, since knowledge claims must be both exact and explicit to count as genuinely scientific. Others claim, however, that it places undue constraints on the expressive power of science; in addition, the probability assignments that emerge tend to be highly sensitive to the choice of language.

One major approach to confirmation is the hypothetico-deductive method. Scientific claims are put forward as hypotheses from which are deduced empirical consequences that can be compared with experimental results. Clearly this requires that both the hypotheses and the evidence be described formally enough for deduction to be possible. The most telling objection to the hypothetico-deductive method is the so-called Duhem-Quine problem: Scientific theories never imply testable empirical consequences on their own but only when conjoined with a (usually elaborate) network of auxiliary assumptions. If the empirical consequences are not borne out, one of the premises must be rejected, but nothing in the logic of the matter decides whether it is the theory or an auxiliary that should go.

But even if the empirical consequences of a theory *T* are borne out, does this provide support for *T?* To infer *T* from *E* and "*T implies E*" is to commit the fallacy of affirming the consequent. This problem is known as the "problem of underdetermination of theory by evidence": that *T* determines *E* does not imply that only *T* does so; any number of hypotheses contradictory to *T* may do so as well. This bears on the realist claim that it is rational to infer to the best explanation. If all we require to say that *T* explains *E* is that *T* imply *E,* then the ability of a theory to explain the evidence does not logically provide any reason to believe in that theory over any of the indefinite number of other theories (most unknown and unarticulated) that do so as well. The problem of underdetermination was the reason that Karl Popper (1902–1994) insisted that theories can never be confirmed, but can only be shown to be false.[18] But the Duhem-Quine problem remains, for it obviously affects attempts to falsify single hypotheses as much as attempts to confirm them.

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[16] R. Carnap, *Der Logische Aufbau der Welt* (Berlin: Weltkreis, 1928), translated as *The Logical Structure of the World* (Berkeley: University of California Press, 1967).

[17] R. Carnap, *Logical Foundations of Probability* (Chicago: University of Chicago Press, 1950).

[18] Karl R. Popper, *The Logic of Scientific Discovery* (London: Hutchinson, 1959).

The basic assumption of the hypothetico-deductive method – that theories should be judged by their testable consequences – no longer seems sacrosanct in contemporary physics. Many of the new developments in high theory are justified more by the mathematical niceties they exhibit than by the positive consequences they imply. String theory is the central example of the 1990s, with some physicists and philosophers suggesting that mathematics is the new laboratory site for physics.[19] This is, however, still a slogan and not a developed methodological or epistemological position. Other equally notable philosophers and physicists oppose this dramatic departure from even the weakest requirements of empiricism. Does the existence of a flourishing physics community pursuing this mathematics-based style of theory development provide on-the-ground evidence against the epistemological and ontological arguments that support empiricism? Or do the positivist arguments show that these new theories will have to make a real contribution to positive knowledge before they can be adopted? Debate at this time is at a standoff.

There are two further main contemporary theories of confirmation. The first is bootstrapping; the second, Bayesian conditionalization. Bootstrapping is the one that on the face of it looks closest to what happens in contemporary physics.[20] In a bootstrap, the role of antecedently accepted old knowledge looms large in confirmation. The inference to a new hypothesis is reconstructed as a deduction from the evidence plus the background information. Thus, the question "Why do the data cited count as evidence for the hypothesis?" has a trivial answer – because, given what we know, the data logically imply the hypothesis. The method is dependent on our willingness to take the requisite background information as known, and on our justification for doing so. How well justified are the kinds of premises generally used in bootstrap confirmations? A cautious inductivist who wishes to stay as close to the facts as possible may be wary, since the premises almost always include assumptions far stronger and far more general than the hypothesis to be confirmed. For example, in order to infer the charge of "the" electron in an experiment designed to provide new levels of precision, we will assume that all electrons have the same charge.

On the Bayesian account of confirmation, the probabilistic relation between evidence for a theoretical hypothesis and the hypothesis itself is not seen as a logical relation, as with Carnap, but rather as a subjective estimate. Nevertheless, the axioms of probability place severe constraints on the estimates. The probability of a hypothesis H, in the light of some evidence e, is given by Bayes's theorem:

$$\mathrm{prob}(H/e) = \frac{\mathrm{prob}(e/H)\,\mathrm{prob}(H)}{\mathrm{prob}(e)}$$

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[19] Cf. P. Galison's discussion "Mirror Symmetry: Persons, Objects, Values," in *Growing Explanations: Historical Reflections on the Sciences of Complexity,* ed. N. Wise, in preparation.

[20] C. Glymour, *Theory and Evidence* (Princeton, N.J.: Princeton University Press, 1980).

Bayesians take the degree of belief in a hypothesis H to be the subjective estimate of its probability (*prob(H)*). But they insist that it should be revised in accord with Bayes's formula as evidence accumulates. In recent years, the Bayesian approach has been extended to cover a large number of issues, including the Duhem-Quine problem, the problem of underdetermination, and questions of why and when experiments should be repeated.[21]

Although Bayesianism is gaining currency, not only among philosophers but also among statisticians, both specific Bayesian recommendations and the general approach are highly controversial.[22] The most general criticism is that too much is left to subjectivity: New probability assessments of hypotheses depend on original subjective assessments, both on the prior degree of belief in a hypothesis (*prob (H)*) and on the likelihood of the evidence given the hypothesis (*prob(e/H)*). Realists in particular would prefer to find some way to maintain that the degree to which a piece of evidence confirms a hypothesis is an objective matter.

EXPERIMENTAL TRADITIONS

Nowadays it is common to complain about the "theory-dominated" approach in the history and philosophy of science. This domination by theory springs from the long-standing assumption, advocated at various periods in the history of the physico-mathematical sciences and widespread since World War II, that the ultimate aim of science is to produce satisfactory theories. One corollary of this assumption is that the primary purpose of observation and experimentation is to validate or test theories. Then the central issue becomes how well observations can ground theories. The doctrine that all observation is "theory-laden," developed during the 1960s and 1970s, gave observation an even weaker role by suggesting that observations could not be made at all unless they were framed by theories and not accepted unless they were validated by theories.[23]

Against this perspective, more recent work maintains that "experimentation has a life of its own," to borrow a now-famous slogan.[24] (In this chapter, we focus on experimentation, rather than observation in general, since a number of interesting issues come out more clearly when we consider explicitly experimental situations, involving conscious planning and contrivance

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[21] C. Howson and P. Urbach, *Scientific Reasoning: The Bayesian Approach* (La Salle, Ill.: Open Court, 1989).

[22] Cf. C. Glymour, "Why I Am Not a Bayesian," in *Theory and Evidence* (Princeton, N.J.: Princeton University Press, 1980), pp. 63–93, and D. Mayo, *Error and the Growth of Experimental Knowledge* (Chicago: University of Chicago Press, 1996).

[23] Cf. N. R. Hanson, *Patterns of Discovery* (Cambridge: Cambridge University Press, 1958); T. S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962; 2d ed. 1970); P. K. Feyerabend, *Against Method* (London: New Left Books, 1975).

[24] I. Hacking, *Representing and Intervening* (Cambridge: Cambridge University Press, 1983), p. 150.

on the part of the observers.) First of all, many argue that the purpose of experimentation is not confined to theory testing. Experiment may be an end in itself or, more likely, serve some other purposes than those of theoretical science, ranging from public entertainment to technological control; the contexts giving rise to these aims could be as grand as imperial world domination or as immediate as brewing.[25]

Whatever one thinks about the aim of experimentation, the question about validity must be addressed. How do we ensure that our observations are valid? Or, at least, how do we judge how valid our observations are? The relevant notion of validity will certainly depend on the aims of those who are making and using the observations, but the least common denominator is probably some weak sense of truth or correctness. This kind of notion of validity is contrary to radical relativism, but it does not involve any commitment to realism concerning theories.

Conscientious practitioners have long been clear about the extraordinary difficulty of achieving high-quality observations. In the context of a quantitative science, observation means measurement. Whenever an instrument is used, the question arises about the correctness of its design and functioning – something painfully clear to those who have tried to improve measurement techniques.

Strategies for achieving validity in observations can be classified into two broad groups: theory dominated and theory independent. Theory-dominated strategies attempt to give theoretical justifications of measurement methods. For instance, in a physiology laboratory, we trust that a nerve impulse is being recorded correctly because we trust the principles of physics underlying the design of the electrical equipment. This, however, only pushes the problem out of sight, as Duhem recognized clearly.[26] Any conscientious investigator must ask how the theoretical principles justifying the measurement method are themselves justified. By other measurements? And what shows that *those* measurements are valid?

These worries have fueled attempts to formulate theory-independent strategies for achieving validity in observations. Many positivistic philosophers made a retreat to sense-data, but even sense-data came to be seen as less than assuredly certain. Currently it does not seem plausible that theory ladenness in its most fundamental sense can be escaped, because any concepts used in the description of observations carry theoretical implications and expectations (and are therefore open to revision). More recently, many methodologists have sought to base validity on independent confirmation: It would be a highly unlikely coincidence for different methods to give the same results, unless the results were accurate reflections of reality. Although

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[25] For discussions of the various purposes and uses of experimentation, see D. Gooding, T. Pinch, and S. Schaffer, eds., *The Uses of Experiment* (Cambridge: Cambridge University Press, 1989), and M. N. Wise, ed., *The Values of Precision* (Princeton, N.J.: Princeton University Press, 1995).

[26] P. Duhem, *Aim and Structure of Physical Theory* (New York: Atheneum, 1962), part II, chap. 6.

intuitively persuasive and reflected widely in experimental practice, this line of argument fails to go beyond the pragmatic, as exhibited nicely in the inconclusive results of recent debates regarding the reality of invisible structures observed to be the same through different microscopes.[27]

In the remainder of this section we examine two of the more plausible attempts to eliminate theory dependence in measurements from the history of physics, one by Victor Regnault (1810–1878) and another by Percy Bridgman (1882–1961). Although virtually forgotten today, perhaps because he did not make significant theoretical contributions, Regnault was easily considered the best experimental physicist in all of Europe during his professional prime in the 1840s. His fame and authority were built on the extreme precision that he was able to achieve in many fields of physics, particularly in the study of thermal phenomena. In his vast output, we find very little explicit philosophizing, but some important aspects of his method can be gleaned from his practice.

For Regnault, the search for truth came down to "replacing the axioms of the theoreticians with precise data."[28] For instance, others before him had made thermometers on the basis of the assumption that one knew the pattern of thermal expansion (usually assumed to be uniform) of some material or other. This was justified by an appeal to various theories, such as basic calorimetry (Brook Taylor, Joseph Black, Jean-André De Luc, Adair Crawford) or various versions of the caloric theory (John Dalton, Pierre-Simon Laplace). Regnault rejected this practice, arguing that it was impossible to verify theories about the thermal behavior of matter unless one already had a trusted thermometer.

How, then, did Regnault manage to design thermometers without assuming any prior knowledge of the thermal behavior of matter? He employed the criterion of "comparability," which required that all instruments of the same type give the same value in a given situation, if that type of instrument is to be trusted as correct. Regnault recognized comparability as a necessary, but not sufficient, condition for correctness. This recognition made Regnault ultimately pessimistic about guaranteeing the correctness of measurement methods, in contrast to the recent advocates of independent confirmation. However, a more pragmatic and positive reading of Regnault is possible. Although comparability did not guarantee correctness, it did give stability to experimental results. Regnault had little faith in the stability of anything founded on theory, having done much work himself to show that the simple and universal laws believed to govern the behavior of gases were mere approximations.[29]

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[27] I. Hacking, "Do We See Through a Microscope?" in *Images of Science*, ed. P. M. Churchland and C. A. Hooker (Chicago: University of Chicago Press, 1985), pp. 132–52, and B. C. van Fraassen's reply to Hacking in the same volume, pp. 297–300.

[28] J. B. Dumas, *Discours et éloges académiques* (Paris: Gauthier-Villars, 1885), 2: 194.

[29] V. Regnault, "Relations des expériences . . . pour déterminer les principales lois et les données numériques qui entrent dans le calcul des machines à vapeur," *Mémoires de l'Académie Royale des*

Regnault's inclination to eliminate theory from the foundations of measurement was shared by Percy Bridgman, American scientist-turned-philosopher and pioneer in experimental high-pressure physics. In one crucial way, Bridgman was more radical than Regnault. What came to be known as Bridgman's "operationalism" eliminated the thorny question of validity altogether, by defining concepts through measurement operations: "In general, we mean by any concept nothing more than a set of operations; *the concept is synonymous with the corresponding set of operations.*"[30] Then, at least in principle, any assertion that a measurement method is correct becomes tautologically true.

Bridgman's thought was stimulated by two major influences. One was his methodological interpretation of Albert Einstein's special theory of relativity, which to him taught the lesson that we will get into errors and meaningless talk unless we specify our concepts by reference to concrete measurement operations. When Einstein gave a precise definition of distant simultaneity by specifying precise operations for its determination, it became clear that observers in relative motion with respect to each other would disagree about which events were simultaneous with which. Bridgman argued that physicists would not have gotten into such errors if they had adopted the operational attitude from the start.

The other formative influence on Bridgman's philosophy was his own Nobel Prize–winning work in high-pressure physics, which emphasized to him how much at sea the scientist was in realms of new phenomena. His experience of creating and experimenting with pressures up to an estimated 400,000 atmospheres, where all previously known methods of measurement and many previously known regularities ceased to be applicable, supported his general assertion that "concepts . . . are undefined and meaningless in regions as yet untouched by experiment."[31]

Appraisals of Bridgman's thought on measurement have differed widely, but it would be fair to say that there has been a general acceptance of his insistence on specifying the concrete operations involved in measurement as much as possible. On the other hand, attempts to eliminate nonoperational concepts altogether from science (such as extreme behaviorism in psychology) are generally considered to have failed, as it is easily agreed that theoretical concepts are both useful and meaningful.[32] But the rejection of operationalism as a theory of meaning also implies the rejection of Bridgman's radical solution to the problem of the validity of measurement methods, which remains a subject of open debate.

Sciences de l'Institut de France, 21 (1847), 1–748; see p. 165 for a statement of the comparability requirement.

[30] P. W. Bridgman, *The Logic of Modern Physics* (New York: Macmillan, 1927), p. 5; emphasis original.

[31] Ibid., p. 7.

[32] C. G. Hempel, *Philosophy of Natural Science* (Englewood Cliffs, N.J.: Prentice Hall, 1966), chap. 7.