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Logical Reconstructionist Philosophy of Science

Percy Williams Bridgman (1882–1961) was a physicist, a Nobel prize winner, who conducted pioneering investigations of the properties of matter under high pressures. His experimental determinations included the electrical and thermal properties of various substances at pressures as high as 100,000 atmospheres. In 1939 he closed his high-pressure laboratory at Harvard to visitors from totalitarian countries, an act that produced controversy within the academic community. Bridgman championed a methodological orientation known as operationalism, in which emphasis is placed on operations performed to assign values to scientific concepts.

Carl Hempel (1905–97) was a German-born philosopher who studied at Göttingen, Heidelberg, and Berlin. Hempel was a member of the Berlin group that supported the aims and viewpoint of the Vienna Circle in the early 1930s. He went to the United States in 1937 and taught at Yale and Princeton. Hempel wrote important essays on the logic of scientific explanation and the structure of theories, a number of which essays are included in *Aspects of Scientific Explanation* (1965).

Ernest Nagel (1901–87) was born in Czechoslovakia, went to the United States in 1911, and has spent nearly all his academic career as Professor of Philosophy at

Columbia. Nagel was one of the first American philosophers to take sympathetic account of the work of the Vienna Circle. His book *The Structure of Science* (1960) contains incisive analyses of the logic of scientific explanation, nomic universality, causality, and the structure and cognitive status of theories.

A Hierarchy of Language Levels

After the Second World War, philosophy of science emerged as a distinct academic discipline, complete with graduate programmes and a periodical literature. This professionalization occurred, in part, because philosophers of science believed that there were achievements to be won and that science would benefit from them.

Post-war philosophy of science was an attempt to implement a programme suggested by Norman Campbell. In *Foundations of Science* (1919),[1] Campbell noted that recent studies of the foundations of mathematics by Hilbert, Peano, and others had clarified the nature of axiomatic systems. This development was of some importance to the practice of mathematics. Campbell suggested that a study of the "foundations" of empirical science would be of similar value to the practice of science. The "foundations" Campbell discussed include the nature of measurement and the structure of scientific theories.*

Philosophers of science who sought to develop their discipline as an analogue of foundation studies in mathematics accepted Reichenbach's distinction between the context of scientific discovery and the context of justification.[2] They agreed that the proper domain of philosophy of science is the context of justification. In addition they sought to reformulate scientific laws and theories in the patterns of formal logic, so that questions about explanation and confirmation could be dealt with as problems in applied logic.

The great achievement of logical reconstructionism was a new understanding of the language of science. The language of science comprises a hierarchy of levels, with statements that record instrument readings at the base, and theories at the apex.

Logical reconstructionist philosophers of science drew several important conclusions about the nature of this hierarchy:

1. Each level is an "interpretation" of the level below;
2. The predictive power of statements increases from base to apex:
3. The principal division within the language of science is between an "observational level"—the bottom three levels of the hierarchy—and a

* Campbell's position on the structure of theories is discussed in Ch. 9, pp. *121–127*.

"theoretical level"—the top level of the hierarchy. The observational level contains statements about "observables" such as 'pressure' and 'temperature'; the theoretical level contains statements about "non-observables" such as 'genes' and 'quarks';

4. Statements of the observational level provide a test-basis for statements of the theoretical level.

Language Levels in Science

Level	*Content*	*For example*
Theories	Deductive systems in which laws are theorems	Kinetic molecular theory
Laws	Invariant (or statistical) relations among scientific concepts	Boyle's Law ('$P \propto 1/V$')
Values of concepts	Statements that assign values to scientific concepts	'$P = 2.0$ atm.' '$V = 1.5$ lit.'
Primary experimental data	Statements about pointer readings, menisci, counter clicks, *et al.*	'Pointer p is on 3.5.'

Operationalism

In analyses dating from 1927, P. W. Bridgman emphasized that every bona fide scientific concept must be linked to instrumental procedures that determine its values.[3] Bridgman was impressed by Einstein's discussion of the concept of simultaneity.

Einstein had analysed the operations involved in judging that two events are simultaneous. He noted that a determination of simultaneity presupposes a transfer of information by means of some signal from the events in question to an observer. But the transfer of information from one point to another takes a finite period of time. Thus, in the case that the events in question occur on systems which are moving with respect to one another, judgements of simultaneity depend on the relative motions of the systems and the observer. Given a particular set of motions, observer Lynx on system 1 may judge that event x on system 1 and event y on system 2 are simultaneous. Observer Hawk on system 2 may judge otherwise. And there is no preferred standpoint from which to determine that Lynx is correct and Hawk incorrect, or vice versa. Einstein concluded that simultaneity is a relation between two or more events and an observer, and is not an objective relation between events.

Bridgman declared that it is the operations by which values are assigned that give empirical significance to a scientific concept. He noted that operational definitions link concepts to primary experimental data *via* the schema

$$(x)\ [Ox \supset (Cx \equiv Rx)]^{*}$$

Given an operational definition, and the appropriate primary experimental data, one can deduce a value for the concept. Consider a case in which the presence of an electrically charged body is determined by operations with an electroscope:

$$\begin{array}{ll} & (x)\ [Nx \supset (Ex \equiv Dx)] \\ & Na \\ & \underline{Da} \\ \therefore & Ea \end{array}$$

where $Nx = x$ is a case in which an object is brought into proximity to a neutral electroscope.
$Ex = x$ is a case in which the object is electrically charged, and
$Dx = x$ is a case in which the leaves of the electroscope diverge.

Since Na and Da are primary experimental data, this deductive argument enables the scientist to mount, as it were, from primary experimental data—the level of the "directly observed"—to the level of scientific concepts, viz.,

Language Level		*For example*
Statements that Assign Values to Scientific Concepts	↑	Ea
Operational Schema		$(x)\ [Nx \supset (Ex \equiv Dx)]$
Primary Experimental Data		Na, Da

Bridgman insisted that if no operational definition can be specified for a concept, then the concept has no empirical significance and is to be excluded from science. Such was the fate of "absolute simultaneity", and Bridgman recommended similar exclusion for Newton's "Absolute Space" and Clifford's speculation that, as the solar system moves through space, both measuring instruments and the dimensions of objects measured contract at the same rate.[4]

But although Bridgman insisted that links be established between statements about theoretical terms and the observational language in which the results of measurement are recorded, he acknowledged that the links may be complex indeed. One of Bridgman's examples is the concept of stress within a deformed elastic body. Stress cannot be measured directly, but it can be calculated by means of a mathematical theory from measurements made on the surface of the body. Thus, for the concept stress, the operations performed include "paper and pencil" operations. No matter. Given the formal

* 'For all cases, if operations O are performed, then concept C applies if, and only if, results R occur.'

relationship between 'stress' and 'strain', and the results of instrumental operations performed on the surface of the body, a value of stress follows deductively. This suffices to qualify stress as a permissible concept from the operationalist standpoint.

In his post-war writings, Bridgman emphasized two limitations of operational analysis.[5] One limitation is that it is not possible to specify all the circumstances present when an operation is performed. A compromise must be effected between the requirement of inter-subjective repeatability and the desirability of a full elaboration of conditions under which an operation is performed.

Scientists have antecedent beliefs about which factors are relevant to the determination of the values of a quantity, and they proceed on the assumption that it is safe to ignore numerous "irrelevant" factors in the repetition of a given type of operation to measure that quantity. For example, scientists perform operations with manometers to determine the pressure of gases without taking into account the intensity of illumination in the room or the extent of sunspot activity. Bridgman observed that the exclusion from consideration of certain factors can be justified only by experience, and cautioned that an extension of operations into new areas of experience may require taking into consideration factors previously ignored.

A second limitation of operational analysis is the necessity to accept some unanalysed operations. For practical reasons, the analysis of operations in terms of more basic operations cannot proceed indefinitely. For example, the concept "heavier than" may be analysed in terms of operations with a beam balance. These operations may in turn be analysed further by specifying methods for constructing and calibrating balances. But provided that standard precautions about parallax are observed, scientists assume that determination of the position of the pointer on the balance scale is an operation that does not call for further analysis.

Operations performed to measure "local time" and "local length" are accepted as unanalysed operations in both classical physics and relativity physics. The "local time" of an event is its coincidence with the position of a hand on a clock. The "local length" of a body is the coincidence of its extremities with a properly calibrated, rigid rod in those cases in which there is no motion of the body relative to the rod.

Of course, the determination of coincidences in the above manner cannot guarantee that the instrument involved is functioning properly as a balance or a clock, or that the rod is a proper measure of length. Moreover, one may accept certain unanalysed kinds of coincidence-determination without committing oneself to the inflexible position that these kinds of coincidence-determination are unanalysable. Bridgman emphasized that although it is

unanalysed a particular set of operations is subject to review as our experience becomes more extensive. He noted that our experience to date has been such that no difficulties for physical theory have arisen from accepting the above coincidence-determinations as unanalysed. But he insisted that it always is possible to give a more detailed analysis of operations.[6] Thus, according to Bridgman, those currently accepted unanalysed coincidence-determinations provide for theoretical statements only a provisional anchor in the observational language.

The Deductive Pattern of Explanation

Operational schemata relate statements about scientific concepts to primary experimental data. At the next higher level, the orthodox programme is to specify the logical relations between scientific concepts and laws. The programme may be implemented from either end. Given a statement of the value of a scientific concept, one may seek to explain this fact by referring to some law. And given a law, one may seek confirming evidence among statements of the values of scientific concepts.

In a widely influential paper published in 1948, Carl Hempel and Paul Oppenheim addressed the problem of scientific explanation.[7] Commenting on an oarsman's observation that his oar is 'bent'. Hempel and Oppenheim suggested that

> the question 'Why does the phenomenon happen?' is construed as meaning 'according to what general laws, and by virtue of what antecedent conditions does the phenomenon occur?'[8]

The deductive pattern of explanation of a phenomenon takes the following form:

$$\begin{array}{ll} L_1, L_2, \ldots L_k & \text{General Laws} \\ \underline{C_1, C_2, \ldots C_r} & \text{Statements of Antecedent Conditions} \\ \therefore E & \text{Description of Phenomenon} \end{array}$$

In the case of the oarsman's observation, the general laws are the law of refraction and the law that water is optically more dense than air. The antecedent conditions are that the oar is straight and that it is immersed in water at a particular angle.

Hempel and Oppenheim made the important logical point that statements about a phenomenon cannot be deduced from general laws alone. It is necessary to include a premiss about the conditions under which the phenomenon which the laws are believed to hold and those initial conditions that are realized prior to, or at the same time as, the phenomenon to be explained. For instance, a deductive explanation of the expansion of a heated balloon might take the following form:

$\left(\frac{V_2}{V_1} = \frac{T_2}{T_1}\right)_{m,\, P = k}$	Gay-Lussac's Law
Mass and pressure are constant.	Boundary Conditions
$T_2 = 2T_1$	"Initial" Conditions
$\therefore\ V_2 = 2V_1$	

Certain of Darwin's explanations of observed biogeographical distributions appear to have the same form. Michael Ghiselin noted that Darwin formulated multiply-conditional explanations for such distributions. The "law" cited—if indeed it is a law—is that

> *if* there are variations, *if* these are inherited, *if* one variant is more suited to some task than another, and *if* the success in accomplishing that task affects the ability of the organisms to survive in whatever happens to be their environment, *then* natural selection will produce an evolutionary change.[9]

For instance, Darwin gave a multiply-conditional explanation for the dominance on an offshore island of a particular species of finch. The argument has the form

If 1 and 2 and 3 and . . . then C
1 and 2 and 3 and . . .
∴ C

where

1. There was an initial dispersion of mainland finches to the island.
2. Geographical barriers ensure reproductive isolation on the island.
3. The island has a distinctive habitat *H* that differs from the habitat of the mainland.
4. There exists variation within the initial mainland population.
5. Those finches in *H* that possess trait *T** are better suited to the performance of task *K* than are finches that lack *T**.
6. Success at *K* affects positively its possessor's likelihood to survive and reproduce.
7. *T** is transmitted genetically.

C. Finches with *T** become dominant in *H*.

To qualify as a successful application of the Hempel and Oppenheim Deductive Pattern, two conditions must be fulfilled: 1) the conditional premise must be a genuine law, and 2) statements 1 through 7 about initial conditions and boundary conditions must be true.

In the course of their discussion of the deductive pattern of explanation, Hempel and Oppenheim were careful to indicate that many bona fide scientific explanations do not fit the deductive pattern. This is the case for many explanations based on statistical laws.[10] An example given by Hempel in a subsequent essay is:

A high percentage of patients with streptococcus infections recover within 24 hours after being given penicillin.
Jones had a streptococcus infection and was given penicillin.
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Jones recovered from streptococcus infection within 24 hours of receiving penicillin.[11]

This explanatory argument does not have deductive force. Rather, the premisses provide only strong inductive support for the conclusion.*

Hempel thus acknowledged that subsumption under general laws may be achieved either deductively or inductively. He consistently maintained, however, that every acceptable scientific explanation involves deductive *or* inductive subsumption of an explanandum under general laws.

Nomic *v.* Accidental Generalizations

On the orthodox view, a successful scientific explanation subsumes its explanandum under general laws. But how can we be sure, in a particular case, that the premisses do include laws? We accept the following argument as a scientific explanation of a green flame test result:

All barium-affected flames are green.
This is a barium-affected flame.
∴ This flame is green.

But we deny explanatory power to the following argument:

All the coins now in my pocket contain copper.
This is a coin now in my pocket.
∴ This coin contains copper.

* A double line between premisses and conclusion is used to indicate that the argument is an inductive argument.

The two arguments have the same form. However, the former argument subsumes its explanandum under a bona fide law, whereas the latter argument subsumes its explanandum under a "merely accidental" generalization.

Orthodox theorists accepted Hume's position on scientific laws. R. B. Braithwaite, for instance, declared that

> I agree with the principal part of Hume's thesis—the part asserting that universals of law are objectively just universals of fact, and that in nature there is no extra element of necessary connexion.[12]

Braithwaite noted, however, that there are difficulties in a Humean analysis of law. One difficulty is that the Humean analysis blurs the distinction between lawlike universals and accidental universals.*

Suppose that two similar pendulum clocks are arranged to be 90° out-of-phase so that the ticks of the two clocks are in constant sequential conjunction. If scientific laws were *nothing but* statements of constant conjunction, then the following statement would be a law:

'For all x, if x is a tick of clock 1, then, x is a tick followed by a tick of clock 2.'

Now suppose that the pendulums of the two clocks were arrested. Does the "law" support the contrary-to-fact conditional 'If clock 1 were to tick, then this tick would be followed by a tick of clock 2'? Presumably not.

"Genuine scientific laws", on the other hand, do support contrary-to-fact conditionals. That 'All barium-affected flames are green' does support the claim that 'if that flame were a barium-affected flame, then it would be green.'

Moreover, a number of important scientific laws seem not to be about constant conjunctions at all since they refer to idealized situations that do not exist. The Ideal Gas Law is a law of this type. Even though there are no gases in which the molecules have zero extension and zero intermolecular force fields, if there were such a gas, then its pressure, volume, and temperature would be related as

$$\frac{PV}{T} = \text{constant.}$$

There is, then, a prima facie difference between lawlike universals and accidental universals. Lawlike universals support contrary-to-fact conditionals; accidental universals do not. But what does "support" mean in this context?

According to Braithwaite, this "support" results from the deductive relationship of the lawlike universal to higher-level generalizations. He suggested that a universal conditional h is lawlike if h

* Hume himself was uneasy about this distinction. See Ch. 9, pp. 94–6.

> occurs in an established deductive system as a deduction from higher-level hypotheses which are supported by empirical evidence which is not direct evidence for h itself.[13]

The barium-flame-colour generalization is a deductive consequence of the postulates of atomic theory. And there is extensive confirming evidence for these postulates (over and above the colour of barium-affected flames). No such deductive relationship is known for the generalization about the two clocks.

Ernest Nagel likewise defended a Humean position on scientific laws. He maintained that lawlike generalizations can be distinguished from accidental generalizations without reference to modal notions like "necessity" and "possibility". Nagel listed four characteristics of lawlike universals:[14]

1. A universal does not acquire lawlike status solely in virtue of being vacuously true. If there are no Martians, then it is true to say that 'All Martians are green.' But truth acquired in this manner does not confer lawlike status on a statement.

   There are vacuously true laws, of course. But their status as laws is determined by their logical relationship to other laws in a scientific theory.
2. The scope of predication of a lawlike universal is not known to be closed to further augmentation. The scope of predication of an accidental universal, by contrast, often is known to be closed. A case in point is 'All the coins now in my pocket contain copper.'
3. Lawlike universals do not restrict to specific regions of space or time the individuals which satisfy the antecedent and consequent conditions.
4. Lawlike universals often receive indirect support from evidence which directly supports other laws in the same scientific deductive system. For instance, if laws L_1, L_2, and L_3 are jointly derivable within an interpreted axiom system, then evidence which directly supports L_2 and L_3 provides indirect support for L_1. For example, since Boyle's Law, Charles's Law, and Graham's Law of Diffusion all are deductive consequences within the kinetic theory of gases, Boyle's Law is indirectly confirmed by evidence that confirms Charles's Law or Graham's Law. Accidental universals, by contrast, do not receive this kind of indirect support.

The Confirmation of Scientific Hypotheses

Hempel suggested in 1945 that there are three phases in the evaluation of a scientific hypothesis:[15]

1. Accumulating observation reports which state the results of observations or experiments;

2. Ascertaining whether these observation reports confirm, disconfirm, or are neutral toward, the hypothesis; and
3. Deciding whether to accept, reject, or suspend judgement on the hypothesis in the light of this confirming or disconfirming evidence.

Hempel outlined a programme of research for the second and third of these phases. Phase 2 is the problem of confirmation. Hempel maintained that this is a problem in applied logic. Both observation reports and hypotheses are sentences, and relations between sentences may be expressed in the categories of formal logic. What needs to be done is to formulate a definition of '*o* confirms *H*' in terms of logical concepts such as consistency and entailment. Armed with a suitable definition, the philosopher of science would then be able to decide whether a particular observation report confirms a hypothesis.

Qualitative Confirmation: The Raven Paradox

Hempel pointed out in 1945 that 'qualitative confirmation' is a paradoxical notion.[16] Consider the relationship between the hypothesis 'all ravens are black' and statements that record evidence. Our intuitions are that a black raven provides support for the hypothesis whereas an orange raven would refute the hypothesis. So far, so good. But the following propositions are all logically equivalent:

(1) $(x)\ (Rx \supset Bx)$
(2) $(x)\ (\sim Rx \vee Bx)$
(3) $(\sim Bx \supset \sim Rx)$

It seems plausible to hold that if an observation report confirms a generalization, then it also confirms every sentence logically equivalent to it. But a black shoe $(\sim Ra \cdot Ba)$ confirms (2),* and a white glove $(\sim Ra \cdot \sim Ba)$ confirms (3). If an Equivalence Condition is accepted, then the raven hypothesis is confirmed by both the black shoe and the white glove. This a paradoxical result. It suggests that it would be appropriate to practice ornithology indoors without even studying birds.

Hempel emphasized that the "Raven Paradox" results when four principles are affirmed. These principles are:

1. The Principle of Instance Confirmation (Nicod's Criterion).[17]

* Since (2) states that, 'given anything in the universe, either it is not a raven or it is black', it is appropriate to take a specific black non-raven to be an "instance" of (2).

2. Equivalence Condition.*
3. The assumption that many important scientific laws are universal conditionals properly symbolized '$(x)\ (Ax \supset Bx)$'.
4. Our intuitions about what should count as confirming instances.

To dissolve the paradox, it is necessary to reject one or more of the four principles.

Hempel maintained that the Principle of Instance Confirmation and the Equivalence Condition are deeply embedded in scientific practice, and that many important scientific laws are represented correctly as universal conditionals. His own position on the Raven Paradox was that we are misguided by our intuitions. In the first place, we judge wrongly that 'All ravens are black' is exclusively "about" ravens. But this is not the case. It is, rather, "about" all the objects in the universe. It asserts that 'given anything in the universe, if it is a raven, then it is black'. An equivalent formulation is $(x)\ [\sim Rx \vee Bx]$, which asserts that 'given anything in the universe, either it is not a raven, or it is black'.

A second reason why our intuitions about confirmation often are wrong is that we tacitly appeal to our background knowledge when judging whether an evidence statement confirms a generalization. For instance, we know that there are many more non-black objects than ravens. And we also know that our chance of finding a disconfirming case—$Ra \cdot \sim Ba$—is greater if we examine ravens for colour than if we examine non-black objects for "ravenhood". Since the risk of falsification is greater if we focus on the class of ravens, we regard a case in which a raven has passed the test—$Ra \cdot Ba$—as a confirming case. On the other hand, we are not impressed when a non-black object passes the test—$\sim Ba \cdot \sim Ra$.

But suppose we knew that there were just ten objects in the universe, that nine of the ten were ravens, and that only one of the ten was not black. If this were our background knowledge then our intuitions about confirmation would be different. We would seek confirming evidence for 'All ravens are black' by examining the one non-black object for ravenhood.

Hempel concluded that the relationship between generalizations and their confirming instances is not paradoxical to the properly educated intuition. If one keeps in mind the logical form of a universal generalization, and if one

* "Statement—forms *p* and *q* are logically equivalent—$p \equiv q$—if, and only if

p	q	p ≡ q
T	T	T
T	F	F
F	T	F
F	F	T

excludes background knowledge about relative class-sizes, then there is no paradox. Hempel insisted that statements about black ravens, statements about black shoes, and statements about white gloves all count as confirming evidence for 'All ravens are black.'[18]

Carnap On Quantitative Confirmation

Rudolf Carnap maintained that the prospects of a theory of qualitative confirmation were unpromising. He sought, instead, to formulate a theory to measure the *degree* of confirmation afforded hypothesis *H* by evidence *e*. Carnap's project was to:

1. Specify the structure and vocabulary of an artificial language within which '$c(H,e) = k$' can be defined;*
2. Enlist the resources of the mathematical theory of probability to assign values to *k*; and
3. Argue that the calculated values are consistent with our intuitions about confirmation.[19]

Unfortunately, the '*c*-functions' developed by Carnap assign the value '$c = 0$' to those universal conditionals for which infinitely many substitution instances are possible. This is counter-intuitive. We believe, for example, that the degree of confirmation of the law of gravitational attraction on the evidence is considerably greater than zero.

Carnap acknowledged this. But he insisted that when a scientist uses a universal generalization, she need not commit herself to the truth of the generalization over a large number of instances. It suffices that the generalization hold true in the next instance. Carnap was able to show that this "next-instance confirmation" of a universal generalization approaches 1 as sample size increases, provided that there are no refuting instances in the sample.[20] Opinions were divided on the appropriateness of this shift of emphasis from 'confirmation' to 'next-instance confirmation'.

* The ingredients of the artificial language include;
1. truth-functional connectives and quantifiers,
2. individual constants that name individuals,
3. primitive predicates that are finite in number, co-ordinate, and logically independent of one another, and
4. rules of sentence formation and deductive inference.

The Structure of Scientific Theories

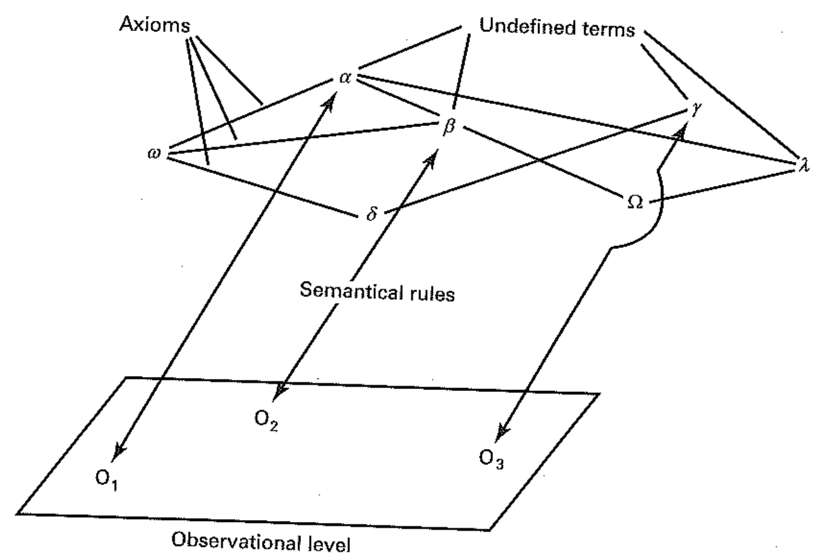
Post-war analyses of the structure of theories were based on Campbell's distinction between an axiom system and its application to experience.* Rudolf Carnap restated the "hypothesis-plus-dictionary" view of scientific theories in an influential essay published in the *International Encyclopedia of Unified Science* in 1939. He declared that

> any physical theory, and likewise the whole of physics, can . . . be presented in the form of an interpreted system, consisting of a specific calculus (axiom system) and a system of semantical rules for its interpretation.[21]

This claim was repeated by Philipp Frank and Carl Hempel in subsequent essays in the same encyclopedia.[22]

Hempel's version of the hypothesis-plus-dictionary view bears some resemblance to safety-nets used for the protection of trapeze artists. The axiom system is a net supported from below by rods anchored at the observational level of scientific language.[23]

Following Campbell, Hempel observed that it is not necessary that every knot in the net have a point of support among the statements of the observational level. This being the case, the question naturally arises, under what



Hempel's "Safety-Net" Image of Theories

* Campbell's view of theories is discussed above, pp. 121–7.

conditions is the net securely anchored? How can it be known whether there is a sufficient number of links of adequate strength between the net and the plane of observation? The strength of the anchoring relation is greatest for "mathematical theories" in which each term of the calculus is assigned a semantical rule. Physical geometry is an example of a theory of this type. Each of the terms of the calculus—'point', 'line', 'congruence' . . . —is correlated with physical operations. At the other extreme, one could imagine a "mechanical theory" whose calculus was linked to observables by a single semantical rule. Would such a 'theory' be empirically significant?

Hempel suggested that a satisfactory answer could be given to this question if there were available an adequate theory of confirmation. According to Hempel, an adequate theory of confirmation would contain rules such that, for every theorem (*T*) and every sentence of the observation language reporting evidence (*E*), the rules confer a specific degree of confirmation on *T* with respect to *E*. A theory to which confirmation rules applied in this way would qualify as empirically significant. The semantical rules of such a theory would be of sufficient strength to anchor its calculus. However, Hempel conceded that no theory of confirmation presently available was adequate for the indicated purpose.[24] Consequently, his proposal (in 1952) to measure the adequacy of the empirical interpretation of calculi by a theory of confirmation had the status of a programme for future enquiry.

Theoretical terms for which there are no dictionary entries nonetheless are assumed to be empirically significant. R. B. Braithwaite suggested that empirical significance is conferred upwards from statements about observables to axioms.[25] In the quantum theory, for instance, it is theorems about electron charge densities, scattering distributions, and the like, that confer empirical significance upon the 'ψ-function'. Noretta Koertge noted that the logical reconstructionist position is that empirical meaning seeps upwards *via* "capillary action" from the soil of the observational level of scientific language.[26]

Theory Replacement: Growth by Incorporation

It was the orthodox position that to explain a phenomenon is to show that its description follows logically (usually deductively) from laws and statements of antecedent conditions. Similarly, to explain a law is to show that it follows logically from other laws.[27]

Applied to the history of science, this concern with a logical reconstruction of the relation between laws was reflected in an emphasis on "growth by incorporation". Ernest Nagel observed that

> the phenomenon of a relatively autonomous theory becoming absorbed by, or reduced to, some other more inclusive theory is an undeniable and recurrent feature of the history of modern science.[28]

Nagel distinguished two types of reduction. The first type is homogeneous reduction, in which a law subsequently is incorporated into a theory which utilizes "substantially the same" concepts that occur in the law. He suggested that the "absorption" of Galileo's law of falling bodies into Newtonian mechanics is a reduction of this type.[29] According to Nagel, Galileo's law has been reduced to, and is explained by, the principles of Newtonian mechanics.

A second, more interesting, type of reduction is the deductive subsumption of a law by a theory that lacks some of the concepts in which the law is expressed. Frequently, the law subsumed refers to macroscopic properties of objects and the reducing theory refers to the micro-structure of the objects. An example to which Nagel devoted some attention is the reduction of classical thermodynamics to statistical mechanics.[30] There occur in the laws of classical thermodynamics concepts which are not included among the concepts of statistical mechanics. Among these concepts are "temperature" and "entropy". Maxwell and Boltzmann, nevertheless, succeeded in deducing the laws of classical thermodynamics from premisses which include statistical laws about the motions of molecules.

Reflecting on this typical case of heterogeneous reduction, Nagel sought to uncover the necessary and sufficient conditions for the reduction of one branch of science to another. He cautioned that conditions for reduction can be formulated only for branches of science that have been formalized. One requirement for formalization is that the meanings of the terms which occur in the theories in question are fixed by rules of usage appropriate to each discipline. Given that this is the case, and that the relations of logical dependence within each theory have been stated, the following are necessary conditions for the reduction of T_2 to T_1.[31]

Formal Conditions for Reduction

1. Connectability: for each term which occurs in T_2 but not in T_1, there is a connecting statement which links the term with the theoretical terms of T_1.
2. Derivability: the experimental laws of T_2 are deductive consequences of the theoretical assumptions of T_1.

Non-Formal Conditions for Reduction

3. Empirical Support: the theoretical assumptions of T_1 are supported b

4. Fertility: the theoretical assumptions of T_1 are suggestive of further development of T_2.

Progress by Incorporation

Successful reduction is incorporation. One theory is absorbed into a second theory which has a broader scope. This suggests that progress in science is much like the creation of an expanding nest of Chinese boxes.

In essays written in the 1920s and subsequently, Niels Bohr championed this view of scientific progress. He maintained that the Chinese-box view is a fruitful methodological application of the Correspondence Postulate.*

To apply the Correspondence Principle as a criterion of acceptability is to require of every candidate to succeed a theory *T* that (I) the new theory has a greater testable content than *T*, and (2) the new theory is in asymptotic agreement with *T* in the region for which *T* is well confirmed.

Joseph Agassi has expressed this methodological extension of the Correspondence Postulate as follows:

> there are two acknowledged methodological demands which can be made of any newly proposed theory: it should yield the theory it comes to replace as a consequence or as a first approximation and also as a special case. The first demand amounts to nothing more than the demand that the new theory explain the success which the preceding theory had. The second demand amounts to the requirement that the new theory be more general and independently testable.[33]

Notes

1 N. R. Campbell, *Foundations of Science* (New York: Dover Publications, 1957), 1–12.

2 Hans Reichenbach, *The Rise of Scientific Philosophy* (Berkeley, Calif.: University of California Press, 1951), 231. This distinction had been made earlier by John Herschel. Herschel's use of the distinction is discussed in Ch. 9, Sect. II of the present work.

* The Correspondence Postulate was an axiom of Bohr's theory of the hydrogen atom (1913). In order to account for the observed spectrum of hydrogen, Bohr suggested that the hydrogen electron can exist only in certain stable orbits, the angular momenta of which are given by $m\,v\,r = \frac{n\,h}{2\pi}$, where *m* is the mass of the electron, *v* is its velocity, *r* is the radius of its orbit, *h* is Planck's constant, and *n* is a positive integer. Transition from one stable orbit to another is accompanied by the emission or absorption of energy (e.g., the transition from $n = 3$ to $n = 2$ produces the first spectral line in the Balmer Series). The Correspondence Postulate stipulates that, in the limit as *n* approaches infinity and the electron no longer is bound to the nucleus, the electron obeys the laws of electrodynamics.

Encouraged by the success of his theory of the hydrogen atom, Bohr maintained that a generalized version of the Correspondence Postulate is a criterion of acceptability for quantum mechanical theories. According to Bohr, whatever the form of a theory of the quantum domain, it must be in asymptotic agreement with classical electrodynamics in the region for which the classical theory has proved adequate.[32]

3 P. W. Bridgman, *The Logic of Modern Physics* (New York: The MacMillan Company, 1927); *The Nature of Physical Theory* (Princeton, NJ: Princeton University Press, 1936).

4 Bridgman, *The Logic of Modern Physics*, 28–9.

5 Bridgman, *Reflections of a Physicist* (New York: Philosophical Library, 1950), 1–42; *The Way Things Are* (Cambridge, Mass.: Harvard University Press, 1959), Chapter 3.

6 Bridgman, *The Way Things Are*, 51.

7 Carl G. Hempel and Paul Oppenheim, 'Studies in the Logic of Explanation', *Phil. Sci.* 15 (1948), 135–75; repr. in Hempel, *Aspects of Scientific Explanation* (New York: Free Press, 1965), 245–95.

8 *Ibid.* 246.

9 Michael Ghiselin, *The Triumph of the Darwinian Method* (Berkeley: University of California Press, 1969), 65.

10 Hempel, *Aspects of Scientific Explanation*, 250–1.

11 Ibid. 582.

12 R. B. Braithwaite, *Scientific Explanation* (Cambridge: Cambridge University Press, 1953), 294.

13 Ibid. 302.

14 Ernest Nagel, *The Structure of Science* (New York: Harcourt, Brace, & World, 1961), 56–67.

15 Carl Hempel, 'Studies in the Logic of Confirmation', *Mind*, 54 (1945), 1–26; 97–121. Repr. in Hempel, *Aspects of Scientific Explanation* 3–46.

16 Ibid.

17 Jean Nicod, *Geometry and Induction* (London: Routledge & Kegan Paul, 1969), 189–90.

18 Hempel, 'Studies in the Logic of Confirmation', 18–20.

19 Rudolf Carnap, *Logical Foundations of Probability* (Chicago: University of Chicago Press, 1950).

20 Ibid. 572–3.

21 Carnap, 'Foundations of Logic and Mathematics' (1939), in *International Encyclopedia of Unified Science*, vol. i, pt. 1, ed. O. Neurath, R. Carnap, and C. Morris (Chicago: University of Chicago Press, 1955), 202.

22 Philipp Frank, 'Foundations of Physics,' in *International Encyclopedia of Unified Science*, vol. i, pt. 2, 429–30; Hempel, 'Fundamentals of Concept Formation in Empirical Science', in *International Encyclopedia of Unified Science*, vol. ii, no. 7, 32–9.

23 Hempel, 'Fundamentals of Concept Formation in Empirical Science', 29–39.

24 Ibid. 39.

25 Braithwaite, *Scientific Explanation*, 51–2, 88–93.

26 Noretta Koertge, 'For and Against Method', *Brit. J. Phil. Sci.* 23 (1972), 275.

27 Nagel, *The Structure of Science*, 33–42.

28 Ibid. 336–7.

29 Ibid. 339.

30 Nagel, *The Structure of Science*, 342–66; 'The Meaning of Reduction in the Natural Sciences', in *Readings in Philosophy of Science*, ed. P. Wiener (New York: Charles Scribner's Sons, 1953), 535–45.

31 Nagel, *The Structure of Science*, 345–66.

32 Niels Bohr, 'Atomic Theory and Mechanics' (1925), in *Atomic Theory and the Description of Nature* (Cambridge: Cambridge University Press, 1961), 35–9.
33 Joseph Agassi, 'Between Micro and Macro', *Brit. J. Phil. Sci.* 14 (1963), 26.

13

Orthodoxy under Attack

Paul Feyerabend (1924–98) received a Ph.D. from the University of Vienna and taught at the University of California. He was a self-professed "anarchist" who opposed the search for rules of theory-replacement and "rational reconstructions" of scientific progress. Feyerabend's position was that "anything goes" and that the mark of creativity in science is a proliferation of theories. Consistent with this orientation, his major work is titled *Against Method* (1975).

Nelson Goodman (1906–98) received a Ph.D. from Harvard and taught at the University of Pennsylvania, Brandeis, and Harvard. He made important contributions to inductive logic, epistemology, and the philosophy of art. He is the author of *The Structure of Appearance* (1951), *Fact, Fiction and Forecast* (1955), and *Languages of Art* (1968).

Stephen Toulmin (1922—) received a D. Phil from Oxford and has taught at the University of Leeds, Michigan State, the University of Chicago, and the University of California. He has written widely on topics in the history and philosophy of science, epistemology, and ethics. In recent work he has outlined a reconstruction of scientific growth in categories borrowed from the theory of organic evolution.

Herbert Feigl (1902–88) participated in the activities of the Vienna Circle (1924–30), as friend and associate of Schlick and Carnap. He came to the United States in 1930 to work with P. W. Bridgman. Feigl was appointed Professor of Philosophy at the University of Minnesota in 1940 and was instrumental in the founding and continuing success of the Minnesota Center for the Philosophy of Science. Feigl