

*Some Major Issues and Developments in  
the Philosophy of Science of Logical Empiricism*

ABOUT twenty-five years ago a small group of philosophically minded scientists and scientifically trained philosophers in Vienna formulated their declaration of independence from traditional philosophy. The pamphlet *Wissenschaftliche Weltauffassung: Der Wiener Kreis* (1929) contained the first succinct statement of the outlook which soon after became known as "logical positivism." In the first flush of enthusiasm we Viennese felt we had attained a philosophy to end all philosophies. Schlick spoke of a "Wende der Philosophie" (a decisive turning point of philosophy). Neurath and Frank declared "school philosophy" as obsolete and even suggested that our outlook drop the word "philosophy" altogether, and replace it by "Einheitswissenschaft" or by "scientific empiricism." The notable impact of Alfred Ayer's first book in England, and my own efforts toward a propagation of Logical Positivism in the United States during the early thirties, and then the immigration of Carnap, Frank, von Mises, Reichenbach, Hempel and Bergmann created a powerful movement, but it elicited also sharp opposition and criticism. Through the discussions within the movement and its own production and progressive work, as well as in response to the

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criticisms that were leveled against it from without, many important changes and modifications have occurred and are still occurring.

The movement resembles the philosophical development of Bertrand Russell, who in many ways is the prime progenitor of logical empiricism, in that the vitality of our outlook is based upon, and demonstrated by, its flexibility and capacity for growth and adaptation. There is nothing dogmatic or ritualistic in our movement. It is not a religion. Quite to the contrary, it is a reaction against and an emancipation from the bondage of metaphysical dogma and speculation. The spirit of the Enlightenment of the eighteenth century and of the *Encyclopedistes* has been revived and been brought to bear upon the foundations of the scientific outlook of the twentieth century. In this it was immensely aided by the tools of modern logic and of logical analysis.

As I see it, logical empiricism has made its most important and constructive contributions in the logic and methodology of the sciences. The foundations of logic, mathematics, physics, biology, psychology and of the social sciences have been penetrated in an unprecedented manner by the work of Schlick, Carnap, Reichenbach, Wittgenstein, Frank, von Mises, F. Kaufmann, Hempel, Woodger, Brunswik, Zilsel, Popper, Nagel, Kaplan, Braithwaite, Pap and many others. It would take many hours merely to outline the most important achievements in these fields. I have reluctantly decided to restrict this paper to a few selected points of basic and general philosophical interest.

As the movement of logical empiricism attained its world-wide scope, we may clearly discern three major trends differing amongst each other more in their method of procedure than in their basic outlook. There is first the trend exemplified most typically by the work of Philipp Frank (and to some extent also by the earlier work of Neurath and von Mises), which combines informal *logical analyses* of the sciences with a vivid

American or Italian operationism, the Significs Movement of the Netherlands, the contributions of the Polish logicians, or of the various trends in British analytic philosophy. (Recent excellent studies, (30) and (28), have given a well-rounded account of the history of logical empiricism.) Instead, I propose to deal with just a few fundamental points in the epistemology and methodology of the empirical sciences. This enables me to sketch, at least in outline, some of the more important changes and developments in the outlook of logical empiricism. I also attempt to reply to some criticisms which have been made against our position. The present report will unavoidably reflect my own interests and predilections. But this will be compensated by the fact that my own views have developed partly through repeated personal discussions in various periods of the last thirty years with most of the leaders of logical empiricism and of related movements.

awareness of *psychological* and *social-cultural* factors operating in the selection of problems and in the acceptance or rejection of hypotheses and which contribute to the shaping of certain styles of scientific theorizing. In a sense, this is a genuine sequel to the work of Ernst Mach. There is, secondly, the trend characterized variously as "analytic philosophy," "therapeutic positivism," or "casuistic logical analysis," originally introduced by G. E. Moore in England and most strikingly developed and modified by Wittgenstein. Nowadays Cambridge and Oxford, the second even more strongly than the first, are among the chief centers of this type of philosophizing. Here we find the Socratic method applied with extreme subtlety to the peculiarities (ambiguities and vaguenesses, strata and open horizons, implicit rules) of *natural languages*. To be sure, extreme preoccupation with this approach has led to some excesses which were stigmatized as "futilitarian" (the phrase is Gustav Bergmann's). Nevertheless, this informal but often very brilliant method is fundamentally not as different as it may appear from either the older (first mentioned) positivistic-pragmatist approach, or the more rigorous logical reconstruction method. This last and third method is best exemplified in the work of Carnap and Reichenbach, and in that of their students. It is also pursued in the work of Woodger and Braithwaite as well as in the work of Tarski, Mehlberg and others among the Polish logicians and methodologists.

Awareness of meaning and of logical structure can indeed be attained by the informal analyses of the Wittgensteinian type as well as by the special construction of artificial languages in which the cognitively significant features of the natural or the scientific languages are subjected to a more rigorous and systematic scrutiny. It was Wittgenstein's basic insight that the meaning of terms must be found in the rules of their usage. Pointing up this usage in various contexts, eliminating ambiguities and other pitfalls of ordinary language, will always remain at least the first and indispensable step in the business of clarification. And whether, beyond this, strict and systematic explication in terms of a constructed language is necessary or advisable clearly depends on the nature of the problem concerned and on the degree of precision desired in the logical penetration of its structure. It seems fairly obvious that the clarification of the logic of science is greatly aided by the construction of syntactical and semantical metalanguages. As practiced by Carnap, this method of analysis is much more flexible than is realized

by its opponents, especially those who support the British natural-language approach. Carnap has pointed out repeatedly\* that exact rational reconstruction or explication by means of artificial languages (or language-schemata) may be achieved in various alternative manners, and that the respective advantages and disadvantages of these alternatives have to be weighed against one another. Perhaps only the total set of useful alternative reconstructions can give us anything like a satisfactory analysis of meaning. This corresponds closely to the current British point of view (inspired by the later work of Wittgenstein) with its attention to families of concepts, strata of language, "slippery slopes," tacit presuppositions, and so on.

One of the controversial issues, which I am confident may be resolved by proper attention to the distinction between natural and constructed languages, concerns the division of all statements into *analytic* and *synthetic* ones. As is well known, this dichotomy forms one of the cornerstones of modern logical empiricism, just as it did already—in a different nomenclature—in the classical empiricism of Hume. At least, according to the view held in common by Schlick, Carnap, Reichenbach and many others, this provides the basis for an adequate account of the difference between the formal sciences (logic and pure mathematics) and the factual or empirical sciences. One of the greatest logicians of our time (I shall for special reasons leave his name unmentioned) shocked me considerably when in a conversation many years ago he branded the sharp distinction between analytic and synthetic as the metaphysical prejudice in logical empiricism. And only a few years ago another outstanding logician, W. V. Quine (of Harvard University) published an article, "Two Dogmas of Empiricism," (35)† contending that our distinction is an indefensible dogma. Similar criticisms of the analytic-synthetic dichotomy have been made by various British writers.

I shall present my reply to Quine's (and related) criticisms in the extremely compressed form of a few theses and very brief arguments. I shall be concerned to point out in which regards the logical empiricists may maintain the status quo and in which other regards important changes of their outlook have become imperative.

1. *The sharp distinction between analytic and synthetic statements is not only fruitful but indispensable.*

\* See (8), especially p. 204; also (9), especially p. 40.

† See also, however, the incisive critique by Benson Mates (33).

No matter what a more sophisticated syntactical, semantical, or pragmatic analysis may finally contribute toward a definitive clarification and characterization of the difference between purely logical truths and factual truths; no matter whether we formulate the distinction in terms of analytic and synthetic, or in terms of true (or false) by virtue of presupposed definitions, designation rules, etc.—the distinction itself is indispensable, if for nothing else, then certainly for the sake of mere clarity of thought. The most obvious and most convincing case can be made out for statements which depend for their truth (*logical truth* in this case) upon presupposed definitions of the purely explicit, abbreviatory, or notational kind. The statement, "If this room is 60 feet long, then it has a length of 20 yards," may serve as an obvious and trivial example. More generally, metrical scales which differ only in the conventional choice of a unit (as, e.g., kilogram vs. pound; Fahrenheit vs. Centigrade; etc., etc.) are analytically equivalent, or logically translatable into one another.

Along similar lines we may say that any sentence, which upon logical analysis reveals itself as a substitution instance of a logical truth, is analytical or tautological. If I say that the weather will either stay the same or change, I have not managed to express an empirical (or synthetic) statement. Clearly we have here a substitution instance of the law of the excluded middle. In Quine's own felicitous formulation, the descriptive terms in such sentences occur only "vacuously."

Besides purely explicit definitions, ordinary contextual definitions (definitions in use) and recursive definitions furnish the bases of undeniably analytic statements. Definitions of these three kinds may be construed either as rules of synonymy (or equivalence), or else as analytic statements in either a syntactical or a semantical metalanguage. Given the context of pure syntax or semantics and given a full-fledged specification of the object-language on hand, definitions may in the second alternative be construed as analytic statements in the respective metalanguage.

I cannot here argue (which, I think, would be laboring the obvious) that the sharp dichotomy of the analytic and the synthetic is indispensable for the logic and methodology of science in that it, and only it, enables us to explicate the difference between necessary and problematic inference, in other words between *deduction* and *induction*.

2. *The revolt against the dualism of the analytic and the synthetic*

rests on a confusion of the logical analysis of (artificially fixed) languages with the historical investigation of (growing, shifting natural) languages.

Quine's protestations to the contrary notwithstanding, I am yet to be convinced that this is not the main source of his erroneous repudiation of the fundamental distinction. Even in the historical or descriptively semantic study of languages, we cannot possibly do without specifying the meanings of terms, since they depend upon culture, period, speaker, writer, listener, reader, practical context, etc. Granting all the ambiguities, relativities, vaguenesses and shifts of meaning as they occur in the natural languages,\* we cannot fail to notice the rule-governed aspects of linguistic behavior that establish synonymies of terms and consequently the analyticity of certain statements. Once the historical type of investigation is supplanted by the logico-analytical type of reconstruction, we deliberately decide upon certain rules and definitions. This may be done informally by the usual techniques of the Socratic (casuistic, dialectical) procedure or by the systematic elaboration of a language in terms of its vocabulary and its syntactic, semantic, and pragmatic rules. No one will deny the artificial and sometimes even procrustean character, especially of the latter procedure. But, if the simple illustrations given under my first thesis are remembered, could it be seriously maintained that we know what we mean by our assertions unless—at least implicitly and in the given context—the game of language is played according to specifiable and definite rules?

Intelligible and responsible communication is impossible without adherence to rules of inference which presuppose, at least, contextually and temporarily fixed meanings, i.e., synonymies and logical equivalences. Must I tell the logicians that their business would be of no use if these conditions could not be fulfilled?

3. *It must be admitted that many empiricists have traditionally tended to misclassify certain rather basic synthetic statements as "really analytic." But reference to this mistake does not justify the repudiation of the basic distinction.*

Anxious to avoid the taboo of the synthetic a priori, empiricists have frequently declared that such fundamental statements as those of the transitivity of temporal succession, or of the principle of causality (sometimes even some of the general laws of physics) are analytic—i.e., dis-

\* Copiously illustrated in a series of articles by Friedrich Waismann (49).

guised definitions, conventions, ultimately tautologies. This must be recognized as a mistake to be charged against those empiricists who commit it. Logically, the mistake boils down to a confusion of what is customarily, but perhaps somewhat misleadingly called "implicit definition" (or "definition by postulates") with explicit definition. The mistake occurs when, e.g., it is said, "we would not call an event A 'earlier' than B; and B 'earlier' than C unless A is also earlier than C"; this is said in (false) analogy to "we would not call Z a 'vertebrate' unless it had a spinal column"; or: "we would not call U an 'uncle' unless it (he) were of the male sex." The applicability of a certain formal structure such as transitivity to a set of empirically given facts is clearly a matter for synthetic statements. And although it might be said that Newtonian time is analytically transitive, or that Euclidean equilateral triangles are analytically equiangular, it cannot be claimed that the transitivity of experienced (or physically measured) time is a matter of analytic truth. Nor can it be said that it is logically true that a physical triangle empirically determined as equilateral must therefore also be equiangular. Similarly any formulations which express the principle of causality as an analytic truth simply do not render adequately its ordinarily understood meaning, be that the homely "same causes, same effects," or any of the more sophisticated formulations of nineteenth-century field physics. This meaning can be construed only by means of a synthetic statement. Surely, there are statements which are true by virtue of the meaning of certain terms they contain, and are nevertheless not analytic but genuinely synthetic. But in this case the meaning of the terms in question is not specifiable by explicit definition.

Wherever the meaning of scientific terms is determined by postulates (implicit definitions), statements formulating such meaning may be said to be both synthetic and necessary. But "necessary" does not mean the same as "a priori" in the Kantian sense. "Necessity" here amounts to no more than causal connection as formulated in syntactical P-transformation rules (45) or else in a modal logic. This is perfectly compatible with the empiricist outlook, although it does represent a certain emendation of Hume's analysis of causality. But since in scientific research we can never be sure of the laws of nature, and thus really never know where to apply the accent of modal connection, the problem of induction remains precisely where Hume (or better, perhaps, Reichenbach) left it. All this, however, does not in the least tend to blur the distinc-

tion of the analytic and the synthetic. Quite to the contrary, a sharp differentiation between the analytic and the synthetic is needed.

4. The fact that a given string of signs (a sentence, a formula, etc.) may be the vehicle for either an analytic or a synthetic statement neither blurs the distinction nor introduces a neutral third category of statements. The neutral third thing is nothing more than the series of signs.

This elementary insight which many of us are at pains to hammer home to our freshman and sophomore students scarcely needs elaboration here. But since several important writers in recent decades have espoused the notion of *functional analyticity* or of a *pragmatic a priori*, it may be useful to scrutinize these ideas, especially inasmuch as they are applied to the logical reconstruction of scientific theorizing.

Is Newton's second law a definition of force; or is it a law of nature that enables us to predict the acceleration of given mass under the influence of a given force? Or is it neither the one nor the other? Is the first law of thermodynamics a definition of "energy" (kinetic and/or potential) or is it a genuine law that tells us that certain measurable quantities are invariant with respect to a whole class of transformations? Is the constancy of the speed of light a matter of definition or of empirical fact? I shall not pause to restate the excellent criticisms (by Mach, Planck, Enriques, Schlick, Reichenbach, or Kraft) that have exposed the weakness of the more exaggerated aspects of Poincaré's, Dingler's, and Eddington's versions of conventionalism. My own tentative resolution of the problem, however, differs from those of the radical empiricists. As I view the matter, I urge that we must recognize the essentially network-like character of scientific theories. A given formula or sentence which somehow relates two or more concepts in the network may be construed as either analytic (definitional) or synthetic (factual) depending on the interpretation of the rest of the system. Since scientists, as a matter of obvious record, shift from one interpretation to another, depending on the context of inquiry (often without being fully aware of this), the only adequate reconstruction would be in terms of a whole set of alternatives. This of course makes the meaning of given symbols systematically ambiguous, but in each single one of the alternative reconstructions the meaning of the symbols is completely fixed. In a sense I am merely applying the Socratic method on the level of the methodology of scientific theorizing. If a physicist tells me—or better still, if he reveals by his very procedures—that he uses Newton's formula,  $F =$

$M \cdot A$ , as a means of prediction or explanation, then I know that he must have independent operational access to the three magnitudes  $M$ ,  $A$ , and  $F$ . (In that case either Newton's third "law"—here misnamed a "law"—or perhaps Hooke's "law"—equally misnamed—will serve as a definition of force.) Similarly, I would question the customary conventionalistic interpretation of the constancy of  $c$ . Surely we cannot interpret both the constancy of  $c$  and statements about simultaneity (of spatially distant events) as factual assertions. But a closer analysis of the inductive implications of the results of Michelson and Morley, and especially of de Sitter's observations, suggests that the invariance\* of  $c$  can be empirically justified, even without recourse to any determination of simultaneity at a distance.

If these conclusions are correct, then one must seriously doubt that "functional analyticity" or the "pragmatic a priori" as applied to specific scientific sentences (formulae) constitutes a genuine third category of statements. If anywhere at all, then these terms had better be used for the very frame-conventions† of the total cognitive enterprise. Parenthetically, I might suggest that some of those frame-conventions might be explicated as P-formation rules of the language of science. For example, it seems plausible that the three-dimensional character of physical space (in ordinary contexts an unquestioned but by no means unquestionable presupposition of science) could be construed as a syntactical feature that determines the form of singular descriptions in science. The rule that a functor like temperature is to be used as a prefix for a triple of coordinates  $x, y, z$ ; or for a quadruple of coordinates  $x, y, z, t$ ; reflects a certain basic feature of reality and may thus be construed as a synthetic characterization of our world.

Be that as it may, I have been concerned in the present context merely to point out that the idea of the systematic interchangeability of the analytic and the synthetic character of formulae in theoretical networks may furnish a more adequate reconstruction than *functional analyticity*. According to the interpretation here proposed, *functional analyticity* is a hybrid notion which, when properly analyzed, comes down to either ordinary analyticity (definitional truth), or else to synthetic factuality assumed with the customary inductive confidence and credit bestowed upon well-confirmed hypotheses.

\* I.e., the equality of the speed of light in its motion toward and from a reflector.

† Such as the principle of induction; or the realistic outlook. See (18).

5. *Independent testability of logically independent postulates of a scientific theory is a practical requirement of the actual methodology of the empirical sciences.*

It is significant that Pierre Duhem's view of the impossibility of crucial experiments has been reiterated and re-emphasized primarily by mathematicians and logicians (such as Poincaré and Quine). Indeed, from a purely formal point of view it must be admitted that adjustments in any part of the theoretical network may result in a better empirical "fit." But, this does scant justice to the actual practice of science. Moreover, it presupposes the shifting of interpretation from one alternative to another. The successive confirmation of logically independent hypotheses is, after all, one of the most striking features of the scientific enterprise. The search for independent, even if indirect, evidence, the search for converging lines of independent evidence, is the major concern of all the experimental ingenuity of modern science.

Permit me to remark somewhat *ad hominem* that logicians would look askance at any scientific theory which presented itself with the claim, "Take me or leave me, but you can't pick out any parts." If I recall correctly, that is precisely what Freud once said about psychoanalytic theory and its "monolithic" character. But Freud himself has repeatedly modified parts of his theory—presumably on the basis of clinical evidence. Science would be in a sorry condition if its theories could not be stated in terms of logically independent postulates. It is precisely for the sake of systematic examination through empirical testing that we must unravel the knowledge claims of a theory into a maximal number of independently confirmable postulates. For example, only after disentangling the various components in the principles of special relativity can we say which experiments confirm which laws. The experiments of Michelson and Morley (Trouton and Noble) confirm one component, the observations on double stars by de Sitter another; and for the confirmation of auxiliary hypotheses, the measurements of aberration and the experiment of Fizeau are equally indispensable.

A view that maintains that the whole body of a scientific theory (if not of all science) confronts experience and that modifications may be required in any part of the system if it does not "fit" obscures dangerously what is of the greatest importance for the progress of science: the successive testing and securing of parts of science—at least in the sense of an approximation. Naturally, no part can be considered as established

with finality; but this insight, which impresses the pure logician, should not blind him to the recognition of the method of successive confirmation. It is curious that it should be the logicians who first undermine their own enterprise by a denial of the sharp distinction between the empirical and the logical (because here they are struck with the vagueness of ordinary discourse and of scientific methodology), and then proceed to blur the methodology of science beyond recognition (because from a purely formal point of view a theory is in principle adjustable in any one of its parts). I suggest that if our logical analyses and reconstructions of the scientific enterprise are thus far unable to do justice to its most important and indispensable criteria (of confirmability and of confirmation), we must look for better analyses and reconstructions instead of giving a distorted picture of scientific method.

6. *There is no convincing instance of the alleged need to change the laws of logic in order to achieve better conformity of theory and fact.*

I realize fully that this is a highly controversial and, besides, a very large issue. So permit me here merely to register my serious doubts concerning the role of alternative (three-valued, or many-valued) "logics" as a solvent of scientific problems. Naturally, I expect to be called "conservative," if not "reactionary"; I expect to be reminded of the fate of those who considered Euclidean geometry as unique and as eternally true (or at least of those who were convinced that it will always be more economical to use Euclidean geometry for physical space). I cannot here specify the reasons why I consider the two-valued logic the only one that deserves to be considered a theory of deduction. Nor can I here furnish convincing arguments that neither von Neumann's nor Reichenbach's alterations of logic are required or even helpful in the axiomatization of quantum mechanics. I shall only say that the yes-or-no character of the logic, the only one that can rightfully be claimed to provide the canons of deductive inference, stems from the most basic feature of language: the semantic rules which govern the univocality of the application of terms to items or aspects of experience.

I turn now to one of the most fundamental and notoriously controversial issues: *the empiricist criterion of meaning*. It is not surprising that this has aroused so much dispute. Occam's razor is apt to mobilize the castration complex of the metaphysicians. But psychoanalytic jokes aside, there are, as it were, three blades to Occam's razor, and careful distinctions between them will help toward a more adequate under-

standing of the import of the meaning-criterion. I shall again proceed by means of concise theses and their elucidation and justification.

1. *The criterion of factual meaningfulness must be distinguished from the principle of formal simplicity and from the principle of inductive simplicity (or factual parsimony).*

Whenever the factual content of two cognitive systems is invariant relative to purely analytic transformations, preference of one system of conceptual representation over another can be justified only on the basis of a principle of formal simplicity or of logico-mathematical expediency. The preference for the kinematics of the heliocentric over against that of the geocentric system is a case in point. If, however, two theories differ in factual content, if the assumptions made in the one theory are more numerous than in the other, then a decision may be made in favor of the factually simpler one. This is merely part of the policy of normal inductive or hypothetico-deductive procedures. The excess cut away by this (second) blade of Occam's razor consists in (presumably) superfluous but meaningful hypotheses. This is the principle of parsimony, the methodological core of Newton's first *regula philosophandi*.

The empiricist criterion of factual meaningfulness, in contrast to the two principles of simplicity just mentioned, is directed exclusively against hypotheses which are absolutely proof against disproof. Such unconfirmability-in-principle may be due to the employment of concepts which are completely isolated from the observation basis of science; this in turn may result from assumptions or stipulations especially made in order to protect some favored hypothesis from refutation. The history of scientific thought contains many examples of this sort. An hypothesis which in its original conception had confirmable consequences is rendered absolutely unconfirmable by special provisions. Absolute space and time, the phlogiston, the caloric, the ether, vital forces or entelechies, etc., at least in the final stages of their most desperate defense, were equipped with properties which made even indirect or incomplete confirmation (or disconfirmation) a logical (and not just a practical) impossibility.

2. *In the more liberal formulation of the meaning criterion, considerable progress has been made beyond the narrow position of the earlier logical positivism.*

Direct and complete verifiability—if not explicitly demanded—seemed implied by the position as taken by Carnap, Wittgenstein, and Schlick up to about 1930. But, as Reichenbach had then urged already, a much

more liberal meaning criterion is needed in order to do justice to the inferential (inductive) character of practically all our knowledge claims. Carnap, in his epoch-making article, "Testability and Meaning" (6), and Reichenbach, in his important book, *Experience and Prediction* (36), have explicitly formulated the broader new meaning criterion. Further modifications and refinements have been under discussion ever since. Paul Marhenke (32), C. G. Hempel (26), and others have expressed serious doubts as to the adequacy of all thus far suggested, or even as to any conceivable formulations of the meaning criterion.

I admit that an all-around satisfactory and fully precise explication is difficult, but I am confident that confirmability-in-principle (for statements) or logical connectibility with the terms of a suitably chosen observation basis (for concepts) is the explicandum of at least a necessary condition for factual meaningfulness. Understood in this way, the meaning criterion still provides a sharp delimitation between sense and nonsense. The suggestion that meaningfulness is a matter of degree seems to me to rest on a confusion between meaningfulness and degree of specification of meaning. There are many concepts at the growing edge of scientific theorizing whose meaning may be only very incompletely determined or only very sketchily outlined. In this regard there is indeed no sharp line of demarcation between science and inductive metaphysics (or even "natural" theology). The objections to inductive metaphysics or theology should be formulated in terms of the principle of parsimony (inductive simplicity, Occam's razor—second blade) rather than in terms of the meaning criterion (third blade). The meaning criterion applies exclusively to transcendent metaphysics or theology, i.e., to doctrines which are immune against tests of even the most indirect sort.

3. *The criterion of factual meaningfulness is to be understood as a proposal and not as a proposition.*

Hence the criterion does not fall under its own jurisdiction. Its adoption can be justified only practically; it cannot be validated, except in the trivial sense that languages of a certain syntactical-semantic-pragmatic structure can analytically be shown to exclude the asking and the answering of empirically unanswerable questions. The criterion is still a powerful instrument for discriminating between sentences with factual meaning and sentences which have only purely formal meaning (such as tautologies or contradictions) and/or those which carry exclusively emotive (i.e. pictorial, emotional, or motivative) appeals. Whether one

considers the distinction thus achieved as the delimitation of science from metaphysics seems to me mainly an issue of terminology.

4. Adoption of the more liberal meaning criterion permits the abandonment of phenomenalism and of radical operationism in favor of a genuinely critical empirical realism.

The Aufbau or logical-construction phase of the early work of Carnap is now completely superseded by an epistemology and philosophy of science which has greater affinity with critical realism than with phenomenalist positivism. This new phase may be regarded as a semiotically more sophisticated and logically more secure form of the empirical realism which Schlick had so forcefully expounded in his *Allgemeine Erkenntnislehre* of 1918 (second edition, 1925).<sup>\*</sup> Knowledge, both on the level of common sense and on that of science, is now being regarded as a network of concepts and propositions tied only in a few places to the data of immediate experience, and for the rest a matter of "free construction." The construction, however, is not that of the Aufbau, which was inspired by Russell's doctrine of abstraction and his hierarchy of types. It is rather the free but tentative positing of a language, some of whose particulars and universals correspond to data and features of direct experience, the vast remainder of which, however, designate unobservables. Einstein, in repeated pronouncements (see especially 15), embraced precisely this point of view after the abandonment of his earlier phenomenalist or more radically empiricist position which he owed largely to Hume and Mach. To be sure, phenomenologists have always tried to distinguish their position from subjective idealism, but the result was never quite convincing. It was a hypocritical realism essentially designed to counteract the speculative excesses of the transcendent forms of realism. I contend that this hypocritical realism of phenomenalism must be supplanted by a hypercritical realism (see especially 19, 20, 21, and 44).

5. The new empirical realism insists on the distinction of the evidential basis and the factual reference of knowledge claims.

<sup>\*</sup> Credit should here be given also to the independent contributions to a similar semiotic-realistic epistemology by Richard Gättschenberger. His book, *Symbola* (G. Braun, Karlsruhe, 1920), which was completed in 1915, contains striking anticipations of the ideas of Schlick, Wittgenstein and Carnap. The work of the German epistemologists, Günther Jacoby and Curt Weinschenk, and of some of the American critical realists, especially of A. O. Lovejoy and Roy W. Sellars, deserves mention in the same connection. The semiotic aspects of the reality problem had, however, been largely neglected by this group of thinkers.

The traditional doctrine of radical empiricism, positivism, operationism, and phenomenalism identified the meaning of a statement variously with its verifying evidence, the method of its verification, the observational or experimental operations of verification, or with the verifying data of immediate experience. In keeping with the terminology common to Wittgenstein and the point of view of semantics, these ideas were often formulated also in the slogan, "The meaning of a statement consists in its truth conditions." But in typically phenomenalist manner, "truth conditions" was invariably understood in the sense of "verifying data."

Critical realists have consistently objected to this myopic identification. It is perfectly clear that existential hypotheses (involving theoretical constructs, *illata*) cannot be logically translated into statements about evidential data. Obvious examples are as follows: statements about past events; hypotheses of modern physics regarding such unobservables as electro-magnetic fields, atomic and subatomic particles, photons, etc. Positivist-phenomenalist attempts at "reduction" of these concepts must now be regarded as complete failures. According to the network analysis of scientific concepts and laws, the verifying evidence is to be viewed as causally related to the evidenced "theoretical" entities. If this be metaphysics, make the most of it! But surely it is not the sort of metaphysics that will generate unanswerable problems or unsolvable riddles of the universe. The realism here suggested allows for hypotheses only if they are at least indirectly confirmable, and for theoretical constructs only if they are part of the network which connects them with terms designating data of direct observation.

6. The meaning of theoretical constructs is best explicated in terms of their locus in the nomological network, i.e., by means of postulates.

This insight was clearly formulated by Schlick, especially in his doctrine of implicit definition (as formulated especially in the 1925 edition of the *Allgemeine Erkenntnislehre*). Strangely enough, neither Schlick himself nor Carnap utilized this insight in the subsequent development of the late twenties and the early thirties. As I interpret it, Schlick was overwhelmed by the then predominantly phenomenalist tendencies of Carnap's and Wittgenstein's outlook. But Carnap (and perhaps even Wittgenstein—though I am still not quite clear about his later development) abandoned phenomenalism by stages and came to embrace an epistemology of decisively realistic outlook. In "Testability and Mean-

ing," and the particular form of physicalism advocated there, the constructs of science were analyzed in terms of their introduction through reduction sentences. "Electric charge," "magnetic strength," "elasticity," and countless other dispositional concepts of physics, chemistry, biology, psychology, and the social sciences were analyzed by means of test condition  $\rightarrow$  test result conditionals. But more recently it became increasingly clear that the conditionals cannot be construed as material implications. Subjunctive and contrary-to-fact conditionals demand interpretations in terms of some sort of modal (causal) implication.

With this insight the pivotal place of laws in the formation and in the very meaning of concepts was realized. Wilfrid Sellars (43, 45, 46, 47) more prominently than anyone else, at this crucial juncture in the history of ideas, emphasized and demonstrated convincingly that "concepts involve laws and are inconceivable without them." This is the idea which Schlick had rather informally and casually anticipated by realizing the role of postulates in the determination of the meaning of concepts. Positivists of an earlier period were blinded by what they conceived as the power of explicit and of ostensive definitions. But explicit definitions presuppose meanings, i.e., the meanings of the definienda. And the so-called ostensive definitions are not definitions at all, but at best a way of learning to associate certain labels with certain items or aspects of experience. To be sure, if our language is to have extralinguistic reference, some terms must be related by an ostensive step to the data of experience. But this does not suffice as an analysis of the meaning of terms. Wittgenstein and his disciples have for a long time insisted that the meaning of terms consists in the rules according to which they are used. If we decide to fix these rules (at least temporarily) in order to study whatever *definite* meanings there may be, and if we codify these rules in some formal manner, then we discern immediately that it is the interrelations of the symbols in a language that are essential to meaning. These rules of usage may be represented as rules of inference in a syntactical metalanguage. The illusion that the meaning of concepts can be distilled by mere abstraction out of the raw material of experience or direct acquaintance is thereby effectively removed. Wilfrid Sellars' incisive critique of "concept empiricism" (i.e., the doctrine according to which the meaning of concepts is ultimately reducible to the elements of direct acquaintance) applies, as he has amply and emphatically argued, not only to the concepts of scientific theories but

also to the concepts of common sense and everyday life. Through adjustments and readjustments the terms of ordinary language have come to be used according to rules, which—fully articulated—amount to implicit definitions. This is epistemologically of the greatest significance in that the customary sharp distinction between observation language and theoretical language is thereby called into serious question, if not entirely eliminated. The predicates and relations even of natural language attain their meanings through a process reminiscent of Darwinian selection, i.e., by a process which is ultimately biopsychological in character. Various networks of concepts (or laws) are subjected to trial, and those which "fit" best are adopted—until further notice.

7. *The new (semiotic) empirical realism requires a revision of the thesis of the unity of science and allows for a monistic (double language) interpretation of the relation of the mental to the physical.*

Carnap's views on the unity of science dated from his constructionistic periods—first phenomenalistic, later physicalistic. It was a thesis of the reducibility of the language of all sciences to a unitary basis (experiential or physicalistic, respectively). But even during the early period of the physicalistic reducibility view, there was the belief that in some way all statements of the sciences were nothing more than convenient *façons de parler* about the data of actual and possible observations. I know, from many conversations with him in recent years, that Carnap has now completely abandoned this last remnant of phenomenalistic positivism. Of course, he is still an empiricist in that he insists that the concepts and statements of the factual sciences must be related to concepts designating data of experience. The thesis of the unity of science, if it is to be readapted, must now be formulated as a program of unification rather than as an achievement of unity. The unification of the sciences is progressing most auspiciously on the level of scientific theory. Many originally separate disciplines within physics and chemistry were welded into coherent conceptual structures through the development of the theory of relativity and of the theories of atomic structure and quantum dynamics. Similarly, the progress of biophysics, biochemistry, psychophysiology, etc., especially aided by such interdisciplinary developments as cybernetics, promises higher degrees of coherence for the future. These developments, together with the results of the epistemology sketched earlier, enhance the plausibility of a new monistic solution of the old puzzle regarding the relations of the mental and the physical. While

the evidential bases of introspective, behavioral, and neurophysiological studies of human organisms are disjoint, it is quite plausible that some of the concepts formed on these respective bases refer to one and the same set of events. "Mind" and "body" may thus be revealed as hopelessly clumsy and singularly unenlightening categories of an earlier epoch in the history of thought.\*

8. *Two quite different forms of logical reconstruction of empirical science, each with its respective advantages and disadvantages, must be distinguished.*

The idea of a unitary nomological net is, as was just pointed out, the projection of an ideal science which can at present be sketched only in a very incomplete outline. Nevertheless, such a schematic reconstruction of the network of concepts and laws can be helpful and illuminating. Only in such an idealized schema are the concepts designating observables connected with the concepts designating unobservables by lawful connections. (Whether these lawful connections are conceived as essentially deterministic or, as present-day physics strongly suggests, as statistical is an important question in its own right, but not vital for the point of the present remarks. Only the future advance of science can show whether something like the determinism of classical physics will be justifiable on a more fundamental level of experimental or theoretical analysis. If the question is raised in the "absolute" sense—i.e., as to the deterministic or statistical character of the "groundlevel" of nature—it is bound to be unanswerable. It reveals itself a pseudo problem because of the complete lack of criteria which would identify the "absolute groundlevel." The question makes sense only when the level of analysis is specified by reference to the variables occurring in the laws and descriptions on that level.)

While the ideal aim of a unitary science is reflected in the sort of reconstruction just suggested, the actual progress of *unfinished, growing* science is more adequately reconstructed by an analysis which sharply distinguishes between theoretical and observation language. The customary reconstruction (5, 13, 27, 2) in terms of partially interpreted postulate systems makes use of the idea of coordinating definitions, or semantic designation-rules (Campbell's "dictionary") which connect the

\* See my article (25). Another, more elaborate, essay on "The 'Mental' and the 'Physical,'" together with critical discussions by some of my collaborators in the Center is being prepared for a later volume of our publications.

(undefined primitive or defined complex) concepts of the erstwhile uninterpreted postulate system with the concepts of the observation language. The meanings of the observation terms are either taken for granted as they are found in the natural language of common life, or else introduced on the basis of such meanings by explicit or by conditional (operational, reduction-sentence) definitions. The one-to-one correspondence, implied in the idea of coordinating definitions and especially encouraged by the well-known philosophical accounts of physical geometry, is however quite problematic. No matter whether the observation terms are concepts of the physicalistic thing-language or of a phenomenal language, there is at best some sort of probabilistic or confirmatory relation between them and corresponding terms of the theoretical language. This is due, in part, to the usually rather complex causal (or statistical) relations between the theoretically posited magnitudes and the indicator variables of the instruments of observation or measurement. In practically all these situations—from astrophysics to the dynamic psychology of personality—there are obfuscating or "nuisance" variables which interfere with the direct assessment of the theoretical laws (relating basic variables). Then there is also the complexity and/or the (possibly only) statistical character of the relations between the physical situations (stimulus-patterns) of the extradermal world, the central processes, and the ensuing responses of the observer. As long as the obfuscating variables in these domains are neither adequately known, nor sufficiently controllable, the confirmation of theories proceeds implicitly (here pointed out explicitly) with such promissory notes as this: "We are going to account for these anomalies, irregularities, or apparent exceptions at some later date when we know more about all this."

I have used the phrase "promissory note" on previous occasions mainly to emphasize the sketchy character theoretical concepts often have when first introduced. Witness, for example, the progressive enrichment of the concepts of atomic physics during the last century; or, in psychophysiology, the transition from rather barren outline-concepts like memory trace to more fully specified neurophysiological structures and processes. But there are also those other promissory notes at the confirmation base of theories. Once sufficient evidence encourages us to postulate theoretical assumptions (nomological or existential), we disregard baffling anomalies in other domains of evidence and thus feel justified in postponing their explanation as "disturbances" until experi-

mental research has succeeded in disentangling them. An adequate logic of the confirmation of theories will therefore have to differ radically from the logic of primary (simple enumerative) induction. In what follows I shall discuss the contributions of Carnap and Reichenbach toward a theory of inductive probability. I shall begin with a logical-empiricist critique of the widely current view which asserts that scientific method rests upon unconfirmable presuppositions.\*

1. *There are no philosophical postulates of science, i.e., that the scientific method can be explicated and justified without metaphysical presuppositions about the order or structure of nature.*

My positivistic or logical empiricist background, I must admit, may have made me somewhat allergic to the term "metaphysics." I realize full well that there are uses of this word that cover quite respectable or at least semirespectable endeavors. If "metaphysics" designates the examination and explication of the basic concepts, methods, and assumptions of the quest for knowledge, then, surely, logical empiricists are metaphysicians—only they prefer to label this sort of study "logical analysis," "theory of knowledge," or "philosophy of science." The term "metaphysics" is sometimes used also for the highly extrapolative—and in this sense precariously speculative—pursuits of the synthesis of a well-rounded world view. "Inductive metaphysics" is perhaps a good label for this type of endeavor. But metaphysics in this sense is continuous with science. It is merely the most venturesome part of scientific theorizing. Modern cosmological theories in physics and astronomy furnish a good example of this sort of speculation. Generally, it seems that scientists are much more competent than professional philosophers to judge to what extent, if any, hypotheses of this more sweeping sort are warranted by the empirical evidence on hand.

I shall refrain from discussing other meanings of the badly ambiguous term "metaphysics," such as the alleged intuitive or dialectical methods of arriving at ultimate truths concerning the nature of existence.

The two senses in which the term "metaphysics" covers enterprises that seem objectionable to the logical empiricist are of course (1) transcendent, i.e., in principle untestable, assertions, and (2) the belief in factual truths that could be validated a priori, i.e., in complete independence of the data of observation.

\* This last part of the present paper is reprinted, with minor alterations, from (24).

The history of modern and recent physics provides a long series of devastating object lessons in both respects. From Galileo down to our days of the theory of relativity and of quantum mechanics, physicists have become poignantly aware of the futility of untestable assumptions and of the questionability of erstwhile unquestioned a priori postulates or presuppositions. In an impressive sequence of incisive changes the very postulates of physical theory have undergone profound revisions. The impact of these developments upon philosophy has quite generally tended to diminish, if not to abolish, the traditional prepossessions of Rationalism. Little, if anything, is left of the *verités éternelles* of Leibniz or of the Kantian synthetic a priori and other principles of this sort, formerly considered as self-evident, indubitable, or as the preconditions for the very possibility of scientific knowledge.

The discovery of the non-Euclidean geometries more than a hundred years ago, and especially their utilization in present-day physical and astronomical theories, shattered the rationalistic and absolutistic conceptions of space and time. The electrodynamic, quantum, and wave-mechanical theories of matter have transformed the time-honored idea of substance beyond recognition, if they have not indeed made it completely obsolete. In this connection the ancient dogma of continuity, one of Leibniz' basic truths of pure reason, had to give way to the conception of the discontinuous interaction between matter and radiation. Connected with the transition to discontinuity was the even more fundamental critique of the deterministic conception of causality. The principles of sufficient (or of insufficient!) reason, as well as the closely related symmetry principles used in many demonstrations of classical and statistical physical theory, have long been exposed as only speciously a priori. Ever since the critique by Ernst Mach of Archimedes' proof of the law of the lever, and Richard von Mises' critique of the principle of indifference (e.g., in Keynes' theory of probability), we have recognized that the correspondence of physical with geometrical symmetries is a matter of empirical fact. Indeed, one would wish to know what the rationalists have to say on the geometrical relation of electric and magnetic field vectors, for this basic asymmetry in our universe should certainly shatter anyone's faith in a priori discernible laws of nature.

But to continue with more recent revolutions in our concept of nature, neither the idea of particles that remain self-identical and indestructible nor the idea of the strict predictability of micro-processes

can be maintained any longer with confidence. A large mass of experimental evidence militates relentlessly against practically each and every one of the alleged truths of pure reason held inviolable for so long by countless philosophers. Adding insult to injury, recent physical theories (introducing advanced potentials) seem to upset completely all classical notions concerning the temporal structure of causal relations, and recent cosmological theories have called into doubt even the dictum *ex nihilo nihil fit*—and blithely assume the possibility of spontaneous generation of matter, along with assorted doctrines of expanding or oscillating finite but unbounded universes.

Even if some of these developments need not be taken too seriously, their total trend and significance is indisputable. The mere fact that some of the allegedly indubitable first principles have been called into doubt, indicates that they cannot be indispensable presuppositions of science. The great conservation principles (of energy and of momentum), for example, were for a while (around 1924) under suspicion of being merely statistical macro-laws—along with so many other such manifestations of the “law of large numbers.” But the detailed studies of the Compton effect reassured the physicists that these basic laws still hold good in a strictly deterministic fashion, even for micro-processes. I shall not tire my readers with the narration of further instances. The *a priori* in physical theory is either of the analytic, purely definitional sort or it is nonexistent. Of course, if one wishes to speak of a *relative* or *pragmatic a priori*—in the sense of C. I. Lewis, Victor Lenzen, Arthur Pap, or of Wilfrid Sellars—this is another matter. This sort of *a priori* is different from the classical rationalistic one (especially from the Kantian) in that it is ultimately under the jurisdiction of experience. This conception of the *a priori* connotes only universality and necessity, the latter in the sense in which even synthetic propositions can be true by virtue of the meaning of the terms they contain. I shall return to this later.

The upshot of our discussion thus far is simply this: Any proposed assertion concerning the order and structure of our universe, no matter how fundamental its role or pervasive its scope, must be regarded as tentative and may be held only until further notice; such notice being given by data of experience which may conceivably motivate us to modify, if not to abandon, the assumption at issue. In other words, any assertion regarding nature, if it is to be scientifically meaningful, must in principle be confirmable or disconfirmable.

2. Assumptions about the uniformity of nature are neither necessary nor effective for the justification of inductive probability.

In an interesting recent article (4) A. W. Burks argues the necessity of unconfirmable presuppositions for the very confirmation of any scientific laws (be they causal or statistical) and of any scientific hypothesis or theory. Along lines fairly familiar since J. S. Mill and Jevons, and elaborated in various ways in more recent decades by Edgar Zilsel, W. E. Johnson, C. D. Broad, J. M. Keynes, J. Nicod, Bertrand Russell, and others, he asks what assumptions regarding the uniformity of nature are required in order to account for the probability of inductive generalizations. Burks claims he does not intend to furnish a justification of the inductive leap. This, he recognizes (3), is an insoluble problem precisely because—at least in its traditional setting—any proposed solution involves either a self-contradiction or a *petitio principii*. This has of course been known ever since Hume's incisive critique of causality and induction. If we attempt to transform induction into deduction, we require premises whose validity cannot be anything but inductive. And if we try to demonstrate the (certain or even only the probable) success of inductive inference on the basis of its success to date, then we assume the very principle we propose to prove.

What, then, is the function of the sort of presuppositions that Burks deems indispensable? He tells us that they are required in order to explain induction. This seems to mean that these presuppositions are needed as premises from which the assumed or actual success of the inductive methods can be deduced. Unfortunately Burks gives no more than a hint as to precisely what presuppositions he has in mind. He suggests that they are of the type of Keynes' principle of limited variety, or, more generally, that they are assumptions concerning the uniformity of nature.

I fail to see the philosophical importance of any attempt in this direction. If it were the success of human adaptive learning and theorizing behavior that was to be accounted for, I would be the first to admit that this is a genuinely meaningful question—but surely a question of science, not of philosophy. This question can indeed be answered. And the answer is clearly along the lines of the biology and psychology of knowledge. It is the same sort of question that can be asked on a more lowly level in regard to the learning and generalizing behavior of that pet of our psychologists, the white rat. Given the rat's equipment of

learning capacities, how complicated a maze will it be able to master, in how many trials, under what conditions of previous training, etc.? While it is a long way from the orientation of rats in a maze to the intellectual adaptations (if I may be forgiven the irreverent comparison) of the Newtons, Maxwells, and Einsteins in their theoretical constructions of the physical universe, the nature of the problem is the same: What type and degree of uniformity must the universe possess in order to be successfully predictable by means of the inductive and hypothetico-deductive procedures of modern science? I think the answer to this question is very obvious. The universe must have precisely the type and degree of uniformity which the successfully confirmed laws and theories ascribe to it (or rather, to some of its aspects).

Burks, however, does not propose this sort of conception of presuppositions or premises for the explanation of induction. He states explicitly and emphatically that the presuppositions of induction are in principle unconfirmable. They result in the choice of a definition of initial probabilities (or of a concept of degree of confirmation) which, once adopted, bestows certainty upon its own presuppositions, and a probability of zero to any alternative presupposition. This trivial and tautological confirmation of the presuppositions of all inductive inferences whose probabilities are determined by the respective choice of a definition of degree of confirmation can indeed not be regarded as a genuine appraisal on the basis of empirical evidence. The three inductive methods which Burks outlines are according to his claim altogether immune to the testimony of experience. Yet, Burks indicates, in a world of completely random character one method (Mr. "Dagger's"), would yield results superior to those of the standard or normal procedure of inductive extrapolation (Mr. "Star's" method; best adapted to a uniform world). The third method, finally (i.e., Mr. "Diamond's"), would serve most effectively in a universe of such diabolically perverse structure that what for the normal method is the most probable, would in that universe be the least probable predictions or hypotheses (and vice versa).

Without wishing to go into technical details of Burks' comparison of the three methods, I should like to raise a more fundamental question: What is the point of the presuppositional analysis if only an omniscient being could decide which presupposition actually applies to this universe of ours in which limited human beings grope for reliable knowledge? If finite, limited empirical evidence has no relevance for the preference

of one presupposition as against another and if limited empirical evidence is the only sort of evidence that human beings will ever be able to marshal, then what, in Burks' opinion, justifies us in clinging to the standard method and in considering the other two as "perverse"?

Of the many traditionally proposed solutions of the problem of induction, which one is relevant or helpful in deciding this issue? The psychologistic, or "animal faith," doctrine of Hume and Santayana merely restates what the human animal as a matter of stark propensities and of habituation believes so strongly anyway. It furnishes no objective reason for a preference. The Kantian theory of knowledge in its anthropological version yields an equally psychologistic, but implausible doctrine of forms inevitably impressed upon the contents of raw experience—and thus depends for its plausibility and effectiveness on the assumption of the constancy of the forms of pure reason. But this is clearly an inductive assumption and thus ineffective in resolving our problem. The presuppositional interpretation of Kant's critique is of course the one to which Burks (along with many other modern thinkers) appeals but it does not enable him to ground the preference for the standard inductive method. Probabilistic solutions of the problem of induction, such as that of D. C. Williams are equally unavailable because they too endorse the standard method by presupposing (D. C. Williams' protestations to the contrary notwithstanding) uniformity. The common-sense school of philosophy (mostly the disciples of Moore and Wittgenstein) reminds us that the very meaning of "rationality" and "reasonableness" includes besides formal consistency especially the normal method of profiting from experience, i.e., standard induction. But since no reasons are given why this standard method should be dignified with the honorific label "reasonable," we are, despite the helpful reminder, left in the dark as far as our problem is concerned.

It should scarcely be necessary to explode the illusion that the presupposition of uniformity is an ultimate postulate. "Postulate" in modern science, especially in mathematics, means an assumption which serves as a premise for deduction. In contradistinction to the term "axiom," it does not carry the traditional connotation of self-evident truth or indubitability. In keeping with its etymology, a postulate is a demand, a requirement. But the mere fact that we require or demand uniformity does not make it sure or even only likely that nature will be good enough to conform with such demands.

3. Carnap's and Reichenbach's theories of induction involve pragmatic vindication rather than theoretical justification.

I shall forgo the discussion of intuitionistic, metaphysical, and other clearly ineffective approaches to the problem of induction. Instead I shall more fruitfully turn to a brief discussion and comparison of two outstanding contributions by logical empiricists, i.e., those of Carnap and Reichenbach. Since it would take much more space than is available here, I shall have to assume some familiarity with the basic features of these two theories. It is clear that the normal method of induction (or at least something very close to it) is here absorbed in Carnap's definition of a logical concept of probability. Like J. M. Keynes' concept of probability, Carnap's degree of confirmation is relational in that it determines the degree to which some given evidence supports an inductive conclusion. Reichenbach, though an avowed proponent of the frequency conception of probability, defines a concept of weight for singular predictive inferences as well as for the probability of hypotheses. Since the supporting evidence in Carnap's conception consists in observed frequency ratios, and since Reichenbach's concept of weight is similarly defined in terms of the relative frequency of a certain type of event in a properly chosen reference class, these two theories of probability are perhaps not as irreconcilably different as might appear at first glance. It is true, Carnap's concept of null-confirmation (i.e., a degree of probability on the basis of no evidence whatever) allows for a qualified use of a principle of indifference and thus seems objectionable to Reichenbach, the staunch empiricist. But Carnap's "a priori" probability (if I may label it in this perhaps somewhat misleading manner) is rendered fairly innocuous by the qualification that in his theory the reliability of assertions of degrees of confirmation on scant evidence is extremely low. This corresponds to the absence of appraised posits in the so-called "primitive induction" according to Reichenbach's theory. And this means that numerically specified weights for predictions and hypotheses can be justified only after certain basic frequency ratios have been posited without assignable weight. Furthermore, Reichenbach, when faced with the notorious problem of how to confirm or disconfirm statements regarding limits of relative frequency has to resort either to a problematic finitization, or to the utilization of a probability-logic which bears a vague though suggestive resemblance to a theory of degree of confirmation. Add to this that Carnap's degree of confirmation,

according to his own theory, is equivalent to an estimate of relative frequency; and that the two rival theories are, of course, isomorphic to the extent that they both yield the customary axioms and theorems of the calculus of probabilities—and one begins to wonder whether Carnap and Reichenbach are as far apart from one another as is generally believed.

Perhaps an even more convincing, and for our present purposes extremely important, common feature of the two theories is the justification of induction. According to Carnap there is a large class, in fact a continuum, of inductive rules (or what is tantamount: of definitions of degree of confirmation), of which Reichenbach's rule is an element, which all share the following significant feature: If the world has some degree of order at all, predictions made according to any one of the inductive rules will in the long run (strictly speaking, in the limit) not only converge with the others, but can also be shown (deductively!) to be the only type of predictions that utilize evidence methodically and are capable of anticipating that order of nature. In some of my early papers (16, 17) I had been groping for this sort of solution of the problem of induction and I think I came fairly close to a tenable formulation in the paper of 1934. But with genuine appreciation I credit the late Hans Reichenbach (see especially 37) with the independent discovery and the more elaborate presentation of this solution.

Let me now show in some detail that this solution implies that there are no ultimate factual presuppositions of science. The search for invariant first principles may be humanly understandable, but in the light of the history of science (as I briefly sketched it in the beginning of this paper) it must be considered as misguided. The inductive and hypothetico-deductive methods of science do not logically presuppose any premises concerning the uniformity of nature. The invariant character of the basic rules of scientific method hinges exclusively upon the aims of scientific inquiry. In order to take the air of dogmatism out of my formulation, I shall state it in the form of a doubly conditional proposition: If it is the goal of scientific investigation to discover lawful relationships and thereby to render the observed phenomena maximally predictable, then the methods of (normal) inductive generalization and of theory construction will attain this goal, provided it is attainable at all, i.e., if nature is at all uniform in some sense, some aspect, and to some degree.

Hume has shown once and for all that no matter what type or degree of order our observations may have confirmed thus far, it is logically conceivable that this type or degree of order may not prevail in the as yet unexplored domains of nature. These domains may be the future, or distant regions of space, or untested ranges of the magnitudes that figure in the functionally formulated quantitative laws of nature. A survey of the conceivable types and degrees of order (uniformity, dependence, independence) is a matter for purely formal analysis in modern logic and mathematics. A study of the criteria of confirmation for the empirical actuality of any one of these types and degrees of order is a task for philosophical analysis and for the theory of probability. Despite the present controversies in the theory of probability, it seems safe to say that there is one basic rule of procedure for the extrapolation from the known to the inferred portions of the universe: "Generalize on the basis of as broad a background of experience as can be secured, and in accordance with the principles of factual simplicity." If there is an order of nature, not too complicated or too deeply hidden, to be discoverable by limited human beings, then this rule of procedure will yield successful predictions. This last statement is of course a tautology. But I think it is illuminating nevertheless. It shows that, while it is impossible to validate any over-all assumptions concerning the uniformity of nature, the procedure of induction which posits specific uniformities can be pragmatically justified or "vindicated" (18). If the degree of uniformity in our world were much lower than it actually is, the rule of induction would still be a helpful tool in the anticipation of the as-yet-unknown. Even in a universe so completely chaotic that all specific generalizations were invariably fruitless, there would yet be one very modest generalization which would hold: namely, the expectation that the chaos will continue. And is this not also an induction?

The principle of induction when interpreted as a rule of procedure, and not as a postulate regarding the order of nature, is of course not subject to confirmation or disconfirmation. Its adoption depends merely on our purpose: to discover and generalize, tentatively and self-correctively, whatever type or degree of uniformity the accumulating observational evidence suggests. Perhaps the dim recognition of the implicit functioning of this most basic rule of all empirical knowledge led so many philosophers to mistake it for a genuinely synthetic truth concerning the universe that could be known on the basis of pure reason.

Waiving important technical questions which are still highly controversial, I venture to state rather succinctly the following tentative conclusions:

A. No matter how strong or how weak the uniformities are in our world, the procedure of normal induction is the only methodical procedure of which we can prove deductively that it can disclose such uniformities.

B. Our world, as we have come to appraise it through the guidance of the principle of induction, seems to contain a vast amount of independencies (nonuniformity, disorder) in addition to a limited amount of dependencies (uniformity, order of various types, such as the causal and the statistical). This inductively established very general assumption serves indeed as a presupposition—or, if you will, as a postulate—for all types of more special causal and statistical research. But this postulate is "philosophical" only in the somewhat regrettable sense that it is terribly vague and abstract. It is certainly not metaphysical in the sense of being either untestable or a priori (i.e., it is not independent of the jurisdiction of experience).

C. While differing in technical points, as well as in the specific values of probabilities based on relatively small amounts of evidence, such inductive methods as R. A. Fisher's (of maximum likelihood), Reichenbach's (rule of induction), and Carnap's (definition of degree of confirmation  $c^*$ ) yield in the long run equal results and are perhaps merely different versions of one and the same basic idea.

D. This basic idea (again waiving controversial issues) may be formulated as follows: If we wish for a method of generalization or of individual predictive inference that utilizes evidence and yields (at least in the limit) unique results, then the "normal" method (or methods) of induction are quite distinct from any "perverse" ones. The "perverse" methods are either insensitive to the testimony of accumulating evidence—and therefore not self-corrective—or else they lack the uniqueness that is characteristic of the methods of simplest generalization or of maximum likelihood.

E. Confronted with the specific problem of determining the most adequate value for the limiting frequency of some statistical phenomenon (as, for example, in radioactive disintegration, quantum-transitions of various sorts, or in Mendelian inheritance, etc.), we may choose either straightforward generalization of the statistical ratios obtaining for speci-

fiable aspects under operationally identifiable conditions; or else construct a theory, an essential part of which must be assumptions regarding statistical distributions, in such a fashion that the observed data will have a maximum likelihood (according to a non-perverse definition of likelihood) in the light of the assumed theoretical model. This suggests that the "Star" method or something very close to it could be given an objectively justifiable preference.

F. In testing the correctness of a specific estimate of a limit of relative frequency, it would indeed be "perverse" to assume that the available samples are not representative. They may of course not be representative for all that. We may at any time, or even all the time, be the victims of what in the light of further evidence would appear as a "terribly improbable" run of bad luck. But to assume this to start with not only makes our estimates nonunique (it opens the floodgates to limitless possible hypotheses) but would amount to a complete defeatism. The method of science is that of a fallibilistic optimism. ("If at first you don't succeed, try, try again!") We must work with what we have and cautiously utilize every bit of uniformity that a broad domain of evidence suggests for generalization. This I accept as the most valuable core of the contribution to epistemology and the logic of science contained in the pragmatist philosophies of C. S. Peirce, John Dewey, and C. I. Lewis.

G. There is one very important issue which I wish to propose for further discussion (I have not yet attained a stable decision on it myself): Are scientific laws, and theories with lawlike assumptions, subject to appraisal in terms of any concept of inductive probability? It should be noted that unlimited generalizations from finite evidence receive invariably a degree of confirmation of zero, according to Carnap's theory of induction. Laws can therefore be appraised only on the basis of their instance-confirmation. This suggests that any assignment of inductive probabilities really presupposes a set of assumptions of lawfulness, certainly of the statistical type and, possibly, in addition, some of the deterministic type. These assumptions would then be *positis* in Reichenbach's sense, i.e., tentative assumptions to which we cannot assign any numerical or even comparative probabilities. This is in keeping with the prevalent view of theoretical physicists, according to which the basic premises of scientific theories may be accepted or rejected as they are more or less fruitful, convenient, simple, or expedient.

Probabilities can be determined only relative to a chosen frame of those basic theoretical assumptions. Far from being metaphysical (again in the sense of being exempt from the jurisdiction of experience) these basic *positis* are held, until further notice, that is to say as long as they provide the frame from which specific testable conclusions can be derived and successfully confirmed by the data of observation. It seems indeed questionable as to whether theories which introduce new magnitudes, as, for example, the electromagnetic field theory of Faraday and Maxwell, could be appraised as more or less probable in the light of experimental evidence. The introduction of new magnitudes semantically considered consists in an essential enrichment of the language of science; and it seems that probability estimates presuppose such a semantical frame but are not applicable to it, as it were, from the outside (19). Perhaps here is a point of genuine agreement between Burks and myself. Moreover, these lines of thought seem in accordance with the conception of natural law in terms of modal logic mentioned earlier and labeled "pragmatic a priori." At the risk of making a bad joke, this sort of view could be said to maintain the existence of synthetic a priori truths that are indistinguishable from analytic a posteriori truths (see 45, 46).

H. In short, the position I am inclined to favor is this: Instead of postulating any general principle of uniformity, I think it is philosophically more defensible to retain the sound core of that principle, either by absorbing it (*à la* Carnap) in a definition of *inductive probability* or even more explicitly by formulating it (*à la* Reichenbach) in terms of a rule of procedure, or as William Kneale very aptly put it, as a feature of the *policy* of induction. The tentative assumption of specific causal and statistical laws then is subject to confirmation in the sense of acceptance or rejection in the light of evidence. But these laws are not capable of probabilification—precisely because they function as the premises of all specific predictions and hypotheses whose probabilities can be ascertained only on the basis of those more fundamental *positis*.

By way of a brief summary I should like to say that there are postulates of science, but they are neither philosophical nor metaphysical in any fruitful sense of these words. The postulates of science are the basic assumptions which make up the scientific theories themselves.

The explication of the methods by which we confirm physical laws, hypotheses, or theories has been attempted in three ways: presupposi-

tionally, definitionally, and procedurally. Burks, in agreement with Kant, Mill, Keynes, and others finds the presuppositional approach illuminating. He feels that without reference to untestable presuppositions the other two approaches remain incomplete, arbitrary, or unjustified. Carnap's definitional and Reichenbach's procedural reconstruction of inductive probability, despite other important divergencies, do agree in the only essential point for our concern: the pragmatic justification of the inductive method. This justification can be derived from the most central purpose of the scientific enterprise: the achievement, through the utilization of empirical evidence, of a maximum of predictability among the observed phenomena. The rules of induction and of the hypothetico-deductive method of theoretical physics can be explicated as well as justified without reference to metaphysical assumptions.

Space unfortunately does not permit any discussion of the new developments in the work of Reichenbach and others on the philosophic foundations of quantum mechanics, on the problem of time, etc., or on recent developments in the moral philosophy of logical empiricism.\*

I have tried to convey my impression that the philosophy of science of logical empiricism, after twenty-five years of development, compares favorably with the earlier logical positivism, in that it is, firstly, more logical. It recognizes some of the erstwhile suppressed but nevertheless valid claims of "rationalism." Secondly, it is more positive, i.e., less negativistic. This is clearly evident in the abandonment of reductive phenomenalism and of ultra-operationism in favor of a more constructive "realism." Thirdly, logical empiricism today is more empirical, in that it refrains from ruling out by decree ontologies or cosmologies which do not harmonize with the preconceptions of classical positivism. Alternative and mutually supplementary logical reconstructions of the meaning of cognitive terms, statements, and theories have come increasingly to replace the dogmatic attempts at unique reconstructions. Logical empiricism has grown beyond its adolescent phase. It is rapidly maturing, it is coming of age. An enormous amount of important and difficult work is yet to be done in the logic and methodology of the sciences. The present outlook of logical empiricism holds high promise that these

\* A succinct survey of the work of Carnap and Reichenbach on probability, with a fairly complete list of the relevant literature, is contained in (24). See also (38). A book on "The Direction of Time" by Reichenbach will be published posthumously by the University of California Press in 1956. Moral philosophy is briefly discussed in (41) and in (23).

endeavors will be carried out successfully. Even if we cannot (and should not) claim to have arrived at a philosophy to end all philosophies, it may perhaps be acknowledged that the new enlightenment has made considerable advances.

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NOTE: It is gratifying to note a certain convergence of recent philosophical opinion in regard to the problem of the justification of inductive inference. From the point of view of the new common-sense (or common-language) approach, Paul Edwards, Max Black, P. F. Strawson, Frederick Will, and others have in various ways exposed the confusions that underlie some attempts to furnish a justification of induction. If the standard of rationality is identified with the logical cogency, which is characteristic exclusively of deductive inference, then indeed one is hopelessly entangled in a pseudo problem. But it is not enough to point to the (undeniable) fact that "rationality" as commonly understood includes, besides deductive consistency and conclusiveness, the patterns of normal induction. It has to be shown that the normal inductive procedure is reasonable, not merely by definition in the light of common usage, but that this definition itself is not merely a result of linguistic habituation or of an arbitrary fiat. This can be achieved, as I have tried to show (16), if the adoption of the rule of induction is made a matter of practical justification (vindication) rather than a matter of theoretical justification (validation). But it is important to note that vindication in this case does not require factual premises. We are dealing here with an extreme case of practical justification by means of purely tautological reasoning. This view, long ago formulated by Reichenbach and myself, seems essentially shared by Carnap (7), Kneale (29), C. I. Lewis (31, p. 325), P. F. Strawson (48), and J. O. Wisdom (52).

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