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Logical Empiricism, 1922–1970

LOGICAL EMPIRICISM was the dominant movement in twentieth-century philosophy of science until about 1965. During its prime in the 1950s it dominated the field so totally that philosophers regarded it as identical with philosophy of science itself. Its basic definitions and distinctions were regarded as self-evident, and anyone who questioned them was contemptuously ignored as simply not a philosopher of science. After about 1958, it was increasingly on the defensive against newer movements, and by 1970 all the innovation was occurring in the newer movements. By 1980 it had almost completely disappeared from philosophy of science convention programs in the United States. The general approach or mood continues among some philosophers of science such as Ayer (1987), but the specific doctrines are no longer asserted, except in social sciences textbooks.

The movement declined and disappeared because its internal difficulties were insurmountable. It was not overthrown; it slowed, stopped, and died out. Yet it remains important historically, in part because of its positive achievements and in part because all later movements defined themselves in opposition to one or more of its doctrines. In philosophy of science after 1950, it was the thesis; all other movements were antitheses. They were all, in part, negations. As Popper observed, if the logical empiricists were positivists, then he was a negativist (1962, p. 229). However, each movement negated a different set of doctrines, so they disagreed with one another as well as with the received philosophy. Stockman (1983) contrasts three of these antipositivist movements. Of course, logical empiricism too began as a negation and it too must be understood as an attempt to correct "old errors." By 1950 the negative program of logical empiricism had been pretty well completed and people thought of it as simply positive philosophy of science, so we must go back to the 1920s to recover the original meaning of this movement.

I begin with 1922 because this was the year in which the Vienna

Circle began to form around the newly appointed Professor Schlick at the university. The earliest members included Carnap, Neurath, Feigl, Waismann, Gödel, and Kraft (Kraft, 1968, pp. 1–2). Other early members were Gustav Bergmann and Philipp Frank (B. Smith, 1987, p. 36). Popper distinguished himself from the Vienna Circle but was regarded by them as part of the movement; he was a dissenting member. Some writers regard the Vienna Circle as the original logical empiricists (or logical positivists), while others regard the Circle as the predecessors of true logical empiricism. In either case, the movement soon spread to include such people as Reichenbach and Hempel in Berlin and Ayer in London.

The Vienna Circle made a sharp distinction between science and metaphysics, identifying themselves with science and against metaphysics. By *science* they meant primarily physics, and especially Einstein's theory of relativity. They also meant mathematics, which was an essential part of physics and astronomy. Some of the Vienna Circle were mathematicians or physicists, and all the others had studied physics and especially relativity theory or some other science (Carnap, 1963, p. 21). Carnap's thesis was titled "Raum" (space), and Schlick's thesis was on the physics of light, under Max Planck (Schlick, 1938, p. viii).

By *metaphysics* they did not mean study of the general characteristics of the real world. They meant the study of a reality beyond appearance, a supersensible reality. They meant primarily Kant, with his thing-in-itself; Bradley, with his Absolute; Spinoza; Schopenhauer, with his Will and Idea, and the like. Schlick begins his *Problems of Philosophy* (1933–34) with the "pernicious" appearance-reality distinction and devotes several chapters to theories about ultimate reality, the thing-in-itself, the essence of things, the external world. His *Allgemeine Erkenntnislehre* (1925) also devotes several chapters to the *Ding an sich*, appearance and reality, and the like.

The science-metaphysics distinction provided two tasks for the Vienna Circle: explain what science is, and expose the errors of metaphysics. They called the first task an "explication" or "rational reconstruction" of science. There was no doubt that physics exists and does provide knowledge. The task of the scientific philosopher was to clarify just how physics does this. Science seeks truth; philosophy seeks clarity (Schlick, 1933–34, pp. 48–49). Each needs the other; both tasks go hand in hand (pp. 51–53). The explication of a science includes stating the general principles any science has to follow to get knowledge (Schlick, 1925, p. 7), and also the logical or mathematical structure of some particular science. Thus for Carnap the philosophy of a science is a syntactical analysis of the language of that science (1935, p. 88 and *passim*).

In order to explicate a science, one has to know the science, of course. "Philosophy is at home in all sciences, and I am convinced that one can only philosophize successfully from within science" (Schlick, 1925, p. 7)—especially natural science (p. 8). "All philosophical work is bound to be unproductive if it is not done in close cooperation with the special sciences" (Carnap, 1937, p. 332). "The philosopher who does not wish to fall into empty speculations must be master of the scientific mode of procedure" (Schlick, 1933–34, p. 52).

Metaphysical questions about ultimate reality can be exposed as pseudo-questions resulting from misuse of language (Waismann, in Schlick, 1938, pp. xxi–xxiii). Thus "Das Nichts nichtet" (the nothing nothings—Heidegger) gets its apparent profundity from the syntactic misuse of "nichts," adding a "das" to it (Kraft, 1968, p. 29). The question, "Do numbers exist or did we invent them?" is exposed as a pseudo-question when we talk instead about numerical expressions, words (Carnap, 1935, pp. 78–82). If we ask, "Do numerical expressions exist?" the puzzle has disappeared.

The most important antimetaphysical task was to recover mathematics for science. Kant had declared Euclidean geometry to be our a priori way of perceiving the world; it was a transcendental condition of all experience, a metaphysical truth about the world of appearance. Non-Euclidean geometries could be dismissed by Kantians as human inventions; but when the theory of relativity used Riemannian geometry, the picture changed. A human invention had fit the world better than an a priori truth. The Vienna people declared that all forms of mathematics, including that of Euclid, were human inventions for dealing with the world. They were languages, ways of expressing scientific hypotheses. Consequently, the way to clarify science was to study scientific languages, and especially the formal, mathematical one that physicists used. The study of language would also help in exposing metaphysical misuse of language.

Scientific languages had three aspects: syntax, semantics, and pragmatics. The syntax rules (L-rules) specified how to construct a sentence; for example, it had to have a subject and a predicate. Other rules of syntax told how to transform a sentence into an equivalent sentence, how to make deductions from sets of sentences, how to deduce predictions, boundary conditions, equilibrium conditions, and so forth, and also how to find inconsistencies. Philosophical clarification of a language through syntactical analysis would help the scientist avoid errors in these processes. The semantic rules, as they were later called, showed how to find the meaning of terms and sentences, and thereby reduce misunderstandings due to differences of meaning in scientific discussion. Pragmatics was concerned with how scientists actually work, but since this was practice, not logic, logicians could have nothing to say about it. Philosophical clarification was limited to syntax and semantics.

Formal languages can range from the most general to the most specific. The most general one would be a unified mathematical logic suitable for expressing and transforming any scientific sentence. The basic example for the Vienna Circle was Whitehead and Russell's *Principia Mathematica* (1915), which had just swept through the world of philosophy in 1920 and which had also inspired Wittgenstein (Toulmin, in Achinstein and Barker, 1969, pp. 26–27). Scientists could invent more particular languages to express their special theories. According to Carnap, anyone could make up a formal language with its own rules (1963, p. 55) and could change the rules when necessary for making changes in a scientific theory (1937, p. 318).

Between 1922 and 1930, the Vienna people developed a basic explication of science that carried out the above program in general terms. They thought of science, physics, as a set of sentences that were (1) true, (2) known to be true, (3) expressed or potentially expressible in a formal or artificial logical language, and (4) related to one another in some sort of structure.

1. There were two kinds of true sentences in science, analytic and synthetic. Analytic sentences are true by definition; they are tautologies saying nothing about the world. Examples: "2 + 2 = 4"; "All oculists are eye doctors"; "The total quantity of money in circulation is the sum of the money in circulation in each Federal Reserve district, each Thursday at 5:00 P.M." Carnap called this sort of truth L-truth. Such sentences are useful in science for making deductions, such as deducing the total quantity of money. That is, if (1) total M = the sum of the M in each district, and (2) M in district 1 = \$50 billion; (3) M in district 2 = \$39 billion; (4) 50 + 39 = 89; then (5) total M = \$89 billion. The first and fourth sentences are analytic, true by definition; two, three, and five are synthetic, empirical.

Synthetic or empirical sentences are not true by definition; they say something about the world. They are synthetic in that the predicate is not contained in the subject. Thus in "All ravens are black," "black" is not contained in the definition of "raven." It adds something new. Some ravens in western Manitoba could conceivably have a green head or bars on their wings, and still be ravens, just as western flickers have red rather than yellow underwings. The assertion "Some ravens in western Manitoba have green heads" is not false by definition, in the way that "2 + 2 = 5" is.

"Empirically true" had already been defined by Wittgenstein in his *Tractatus Logico-Philosophicus* (1922, secs. 2.2, 4.06, 4.4, 5.1, etc.) and again by Tarski (1944, originally 1933; cf. Schlick, 1933–34, p. 53). Wittgenstein's *Tractatus*, along with ideas of Mach, Bolzano, and others, inspired the Vienna Circle and provided many of its early ideas. According to Wittgenstein and Tarski, empirical truth is correspondence with reality. Thus "All ravens are black" is true if all ravens are black. What else could it be?

2. The second characteristic of science was that its statements were known to be true. That is, they had been tested and verified. Anyone could construct sentences that might happen to be true, but to know and to prove that they were true was a much more difficult enterprise. Testing was thus an important scientific activity, perhaps the most important one.

We test the truth of analytic statements by substituting equivalent symbols until we get an identity, A = A (Schlick, 1925, pp. 189–90). Empirical or synthetic statements are about the world, so we have to test them there. If empirical truth is correspondence with reality, then testing consists of examining reality to see whether some statement corresponds to it. For example, Einstein's theory of relativity had been tested in 1919 when some astronomers observed a star during an eclipse. "Examining reality" consisted of observing the star, and the observation was expressed in a sentence. Thus the correspondence was between two sentences, one by Einstein, "Star *x* will appear in space-time line *y*" and one by the observer, "Star *x* appeared in space-time line *y*." If the *x*'s and *y*'s and their relation are identical, the theory has been verified (Schlick, 1938, p. 225). Carnap called the observer's sentence a *protocol sentence* to distinguish it from the sentence in the theory (Carnap, 1937, p. 317).

3. Carnap's *Der Logische Aufbau der Welt* (the logical structure of the world, 1928) was the first sketch of a formal language for describing the whole physical and psychocultural world. The language was modeled on the logical language of Whitehead and Russell's *Principia Mathematica*. The elementary terms of the language referred to elementary experiences

such as "red" or a musical chord; the terms could not be defined, but their meaning could be given by pointing to the experiences (ostensive definition). There was a single primitive relation, a recalled similarity between two experiences. Other concepts were defined in terms of this relation, and still others in terms of those concepts. For example, a *part similarity* is a similarity between parts of two experiences; a *similarity circle* is all the experiences that have a part similarity relation to each other. The musical chord C-E-G belongs to three similarity circles.

Carnap's *Abriss der Logistik* (1929) claims to be an instruction manual for explicating the formal language of any science. It is again based on *Principia Mathematica* (PM). The analyst is instructed to define actual concepts of the science in terms of other concepts of the science, and so on until ultimate concepts such as "red" or "similar" are reached. Syntactic rules can be set up as needed for the task. The analyst can pick whatever ultimate concepts are convenient for defining the others, as in PM. Note that the *Abriss* analysis runs in the opposite direction from the *Aufbau* construction.

One of Carnap's examples is physics. Here the elementary terms refer to space-time points in world-lines. There are two basic relations between points: K, coincides with; and Z, in the same world-line at different times.

4. The sentences of a science have to be related in some deductive fashion, so that particular sentences can be deduced for testing purposes. According to Carnap (1929, p. iii), this means that each science has some basic axioms from which the others are deduced. The axioms cannot be directly compared with protocol sentences for testing, but low-level sentences deduced from them can. Conversely, the definitions of the low-level concepts are provided by more abstract ones, so that the whole set is a system (Schlick, 1925, p. 87).

We now shift from syntax to semantics. In the 1920s the Vienna Circle advanced their attack on metaphysics by applying Wittgenstein's (1922) definition of meaning. There are two kinds of meaning, logical and empirical, corresponding to the two kinds of truth. Logical meaning is simply definition, that is the equivalent words and sentences. The empirical meaning of a proposition is what it says about the world—that is, what about the world would be different if it were true rather than false. "All ravens are black" says they're not white, green, or mottled; they're black. But this is precisely what we look for when we test the proposition. Consequently, Wittgenstein asserted, we know what a proposition means when we know how to test it. "All ravens are black" means (1) instructions on how to identify a raven, for instance by showing two or more pictures and saying, "Any experience similar to that one but different from that or those is a raven," and (2) instructions on how to identify black, using a hairpin and a tomato, or a color chart (cf. Schlick, 1933–34, p. 129; Carnap, 1963, p. 45; Ayer, 1936, p. 35: "We say that a sentence is factually significant to any given person if, and only if, he knows how to verify the proposition which it purports to express").

The delicious consequence of this concept of meaning is that metaphysical propositions are not only misused language; they are meaningless. Consider "The Absolute is perfect," Hempel's favorite example. What instruction does the metaphysicist[1] give for locating this Absolute? Does he have a photograph of it, or directions on where to go to find it? How would one recognize it—does it sneeze or bark? And what would "perfection" look like? What color is it? What temperature? Bradley's theory does not answer these questions, so the statement is empirically meaningless. Similarly, all metaphysical statements about ultimate reality are meaningless.

The simplest, most dogmatic account of the logical empiricist program was Ayer's *Language, Truth, and Logic* (1936), written in a burst of youthful enthusiasm for the new philosophy, at a time when that philosophy was already being changed. Ayer concentrated on truth, verification, and meaning, and had little to say about the formal-mathematical language of science that so concerned Carnap and the Vienna Circle. Ayer belonged to the British empiricist tradition of Hume rather than to the mathematical tradition of Frege, Hilbert, Whitehead, and Russell that inspired the Vienna Circle.

Ayer enthusiastically applied the verification rule for empirical meaning to metaphysical statements, such as "The Absolute is lazy." (Bradley never actually said that.) However, he also applied the rule to ethical statements. He observed that they are not only untestable and therefore nonscientific; they refer to no possible observables and thus have no meaning. Ayer's assertion was immediately challenged by Stevenson in his *Ethics and Language* (1938). Stevenson argued that a typical ethical statement like "Honesty is right" does indeed have no cognitive meaning, as Ayer correctly asserted, because "right" refers to nothing. But it has emotive meaning. It means "I approve of honesty, and you should, too." Ayer later agreed with this. Stevenson's argument opened a whole new field of philosophic inquiry, since if there were emotive meanings there could be other kinds of noncognitive meanings, too, such as imperative meanings, and there might be various kinds of emotive meanings. There could even be metaphysical meanings.

DIFFICULTIES AND CHANGES

Even in the 1920s there was discussion and disagreement in the Vienna Circle over some of the foregoing points, and the discussion expanded as the new ideas caught on. Between 1931 and 1938 the Vienna Circle spread all over Europe and America. In part they made contact with like-minded thinkers and held annual conferences that drew 100 to 150 participants; in part they accepted university appointments, chiefly in the United States (Kraft, 1968, sec. 1). The new American audience called for a shift of emphasis. For one thing, the metaphysical philosophies of Europe were scarcely known to American philosophers, and when known were rejected. Thus philosophers who sought to distinguish science from metaphysics were speaking to the already convinced. For another thing, U.S. philosophic schools and especially pragmatism seemed to agree with logical empiricism in part and so were potential collaborators.

The result was a gradual dropping of the original "combat metaphysics" program and a concentration on the "explicate science" program. This program was also redefined as a program to provide a logical foundation for empirical science. The foundation, by clarifying what science is, would help scientists avoid errors and blind alleys.

Neurath's *Encyclopedia of Unified Science* project, begun in 1935, embodied this new self-definition of logical empiricism. The first two volumes were supposed to provide the logical foundations of science in general, plus mathematics and logic, and later volumes would analyze the methodological problems of special sciences and the degree of unity that they had achieved. The project also afforded an opportunity to draw pragmatists into collaboration: Charles Morris, already collaborating with Carnap at Chicago, wrote the *Foundation of the Theory of Signs*, and John Dewey wrote *Theory of Valuation* (1939). Dewey's work attacked emotivist ethics, including the work of Ayer and Stevenson, by arguing that valuation in the sense of appraisal was certainly a rational process, therefore cognitively meaningful, since it involved estimation of costs and consequences, comparisons, predictions, correction of estimates, and so on.

The new aim of providing a logical foundation for science brought with it some additional tasks. It was not enough to clarify what truth, meaning, and verification were; other scientific activities needed clarification as well. These included theory building, model building, explanation, and prediction. Since the old tasks were thought to be pretty nearly completed by 1936, at least by Ayer, the new tasks were welcome. However, not all activities of scientists were to be clarified, as Reichenbach emphasized in his distinction between discovery and justification (1938). Discovery was a creative process of inventing new hypotheses; it had no logic to it, since one could not use logical calculation to make a new idea pop into one's head. It just happened. But justification, the testing of hypotheses, had a logic to it, the logic of verification. Only the logical activities of scientists could have a logical foundation; the other activities belonged perhaps to psychology but certainly not to philosophy.

During the following thirty years, the discussions, difficulties, and changes continued to appear, until the original doctrines had either been changed beyond recognition or abandoned. (See Manicas, 1987, p. 243, for a list of abandoned doctrines, though there may be disagreement on some of them.)

We begin with Carnap's project of explicating the language of science—that is, its syntax and semantics—and of specific sciences. In the mid-twenties he had discussions with Neurath and others on what the elementary terms of the language should refer to, and gives several possibilities in paragraphs 62–67 of the *Aufbau* (1928). They include electrons, space-time points, atomic sensations, and whole experiences. Carnap chose the latter, based on Gestalt psychology, but later was convinced by Neurath to shift to physical elements; he used space-time points in his *Abriss der Logistik* (1929). Empiricists like Ayer (1936) thought that space-time points were mighty abstract; Ayer preferred atomic sensations, sense data. If one asked, "What does 'space-time point' mean?" the physicist could not answer by pointing to one; he would have to give a theoretical definition. We would still be operating in theory, not in direct experience. But an empirical science should be based on experience, and preferably the simplest. Since observables were the locus of testing and therefore of empirical meaning, they should really be observable, like "red," and unlike space-time points or electrons.

Sense data, however, are private; I do not experience yours. What then does it mean to me when you say, "I see a raven and it's black!" How can I test this sentence? That is, how can I see whether you are telling the truth about your sense data? Of course, I can look in the same place, but suppose I see a crow there? Now, there are two assertions to be tested and thereby get some meaning on them, and the same problem recurs. The problem is that I can test my own assertion by looking again—I know what I mean—but I cannot test yours because I do not have your sense data. All observation reports are untestable and meaningless except for the person who utters them. Science is founded on nearly meaningless statements.

Ayer in 1987 asserted that he had no solution to this problem, which he did not appreciate in 1936 (1987, pp. 30–31). Presumably he rejected Schlick's solution (1933–34, chap. 16), which located empirical meaning in formal relations of similarity and ordering of colors, not in colors or sounds. Schlick in turn rejected sense data as too positivistic; he rejected the positivism of Hume, Mill, and Comte (p. 182).

We can solve Ayer's problem in practice, pragmatically, by discussing, pointing out characteristics, asking questions, checking our instruments—or by using rhetoric: "Stupid! That's a crow!" But practice is not logic and does not solve the logical problem. The basis of practice is intersubjectivity; but what we share is language, not sense data. And, as Carnap observed, if we study the language of scientists when they discuss observations, they speak about things with attributes and relations, not sense data. Consequently, he and Neurath argued in 1931–34 that the elementary terms of scientific languages should refer to things (Carnap, 1928, 2d ed. 1961, p. viii).

Both Carnap's physicalist position and Ayer's phenomenalist position were held by some logical empiricists for a time; in the 1950s some philosophers were still talking about red patches, while others scorned them. Still others preferred to drop the whole problem and assume that it is solved in practice by discussion among scientists. Once it is solved, what comes out of the discussion is a sentence, an observation report, not sense data. Consequently, meaning is a relation between sentences, not between a sentence and sense data, electrons, or world lines. The generalization "All crows are black" refers to observation reports like "Black crow observed on 9/4/84 at 11:15 A.M. in the Murphys' corn patch." We can imagine these reports to be made by a standard observer, or an ideal, honest, and infallible observer, or certified as correct by a panel of scientists. Thus meaning, being a relation between sentences, is similar to confirmation, which is a relation between hypothesis (sentence) and evidence (sentences).

Sense data were real in the highest degree for Ayer in 1936 and for

Hume; but when they were no longer needed for the logic of confirmation and for meaning, they were neglected and scorned, until for some philosophers they no longer even existed. "I don't happen to believe that there are such objects as 'sense data,' " wrote Putnam (1962, p. 362).

Theoretical terms, at the opposite end of the language from the elementary observation terms, presented a more serious problem. The idea of a tree of definitions running from the most abstract concepts like electrons and intramolecular forces right down to elementary things proved to be too complex to work out. In any case, physicists use many terms that do not refer to or mean anything observable or conceivably observable, such as curved spaces or space-time points. Yet physicists know what they mean, because they can use these terms in theorizing. The terms are defined by reference to other theoretical terms, not by reference to observables. The whole chain of theorizing, however, eventually leads to some empirical prediction.

In other words, physicists do not have a tree of definitions in which every theoretical term eventually is defined in terms of elementary observables. Instead they have a network of concepts connected at a few points to observation. The concepts are partly defined in terms of each other, and partly defined by some indirect observable consequences (Kraft, 1968, p. 103).

This means that all current sciences are expressed in two languages, logically: a theoretical language which may be mathematical or symbolic or highly technical and assumes nothing about existence; and an observation language for reports of pointer readings and Commerce Department statistics (Carnap, 1956). Carnap had already developed the idea of two languages in 1936 (1963, p. 78), and by the mid-1940s it was well established among logical empiricists, such as Margenau at a 1944 Yale colloquium. The two languages are connected at some points by "rules of correspondence" (Margenau, Carnap), or "interpretative sentences" (Hempel, 1965, p. 184). The correspondence rule connects some theoretical term with observation reports but does not define the theoretical term. Definitions stay within the theoretical language, so that any theoretical term has additional meaning beyond any observation report.

Empirical meaning now is a relation between two languages, not two sentences. This implies that the unit of meaning is a whole theory and its language, not a sentence or a term. Terms and sentences get their theoretical meaning within the theory, and the theory gets its empirical meaning from the occasional connections to observation sentences. Similarly, we test a whole theory, not one hypothesis, because the whole theory is needed to connect to some observation sentence (Carnap, 1937, p. 318).

As a consequence, the empirical meaningfulness of a theory varies according to the number of correspondence rules that have been established. A new and abstract theory may be mainly a mathematical exercise with few empirical interpretations—for instance, game theory as of 1944—while an old and well-established theory may have developed many empirical interpretations. Complete lack of meaning is the limiting case of a completely uninterpreted theory.

A further consequence is that the distinction between scientific and metaphysical terms can no longer be maintained. One can no longer ask of a suspected metaphysical term or sentence: to what possible observation reports does this refer? The answer can be that this term or sentence functions as part of a scientific theory that has meaning as a whole. And indeed, argues Hempel, the supposedly metaphysical term may have a heuristic value for the scientist, suggesting theoretical constructions and implications that eventually lead to empirical predictions. For instance Einstein's metaphysical belief that God does not depend on chance was important in influencing his arguments in quantum mechanics.

Even whole metaphysical theories can no longer be sharply distinguished from scientific theories. Any metaphysical theory, even one about the Absolute, can work out some few interpretive sentences and thus have some empirical meaning. It would be hard to imagine a theory with no meaning at all. "There must be some rudimentary sense in which all terms actually in use have meaning" (Rozeboom, 1962, p. 352). The difference between science and metaphysics can at most be one of degree, if indeed there is any difference. Presumably scientists control their observations more carefully than metaphysicians, but that is a pragmatic difference, not a logical one.

The reluctant readmission of metaphysics into science in the 1950s was not nearly as shocking as it would have been in 1935. In 1937 Carnap observed that eliminating metaphysics from science was not so easy after all (1937, p. 322). About 1948 a student at Chicago asked Carnap whom he had in mind when he described metaphysics as meaningless. He answered immediately, "Heidegger!" "What about Whitehead's metaphysics?" Pause. Silence. "That would be a very difficult question." By 1950 it was not news that any scientific language and any logic expressed some metaphysical commitments, in the sense of what is real, not in the sense of what is beyond all experience. Bergmann (1954) even began to investigate the metaphysical implications of symbolic logic and basic logical empiricist concepts. Others like Craig, however, continued to try to eliminate metaphysics from science, in principle at least; and Ayer (1987) still insists that Heidegger and Derrida at least are meaningless.

Unfortunately, once the two languages had been distinguished, it proved difficult to keep them apart. Some philosophers argued that most scientific observations nowadays are made through instruments, and the instruments embody a theory by which we interpret the observed fuzzy lines and patches as moons of Jupiter, microbes, or Brownian motion. Thus even "observation terms are themselves for the most part theoretical terms whose credentials we have come to accept at face value" (Rozeboom, 1962, p. 339). Consequently, there could be no pure and distinct observation language.

We turn next to problems with the central concept of verification. It was clear from the start that complete verification of an empirical proposition was impossible, because things might always change (Schlick, 1925, p. 193; Carnap, 1937, p. 321, crediting Popper). Logically, any universal hypothesis such as "All crows are black" requires an infinity of observations to verify it, because new crows keep getting born. Since complete verification is impossible, all we get in science is confirmation, supporting evidence. This in turn implies that no scientific law is absolutely certain; even a highly confirmed one might someday be disconfirmed. The same point is true of protocol sentences; here Carnap and others accepted Popper's arguments (Carnap, 1963, p. 32). A further consequence is that complete falsification is also impossible, since the protocol sentence might itself be mistaken (Carnap, 1937, p. 318). All we can get is disconfirmation.

However, as Schlick later observed, this does not discredit scientific knowledge. "No theory which has been at all verified by experience was ever entirely overthrown" (1938, p. 233). Confirmed theories have been made more accurate, more detailed, or have been absorbed into broader theories (like Newton's theory), but have not been completely falsified.

The philosophical problem then is to provide an explication of the logic of confirmation, not verification. Confirmation is the relation between a hypothesis *h*, or a whole theory, and a finite set of protocol sentences or observation reports *e*, such that *e* provides favorable evidence for the truth of h, but not conclusive evidence. Hempel reviewed the various proposed definitions of *confirmation* in 1945 (1965, chap. 1) and proposed his own definition: e confirms h if h is true for the set e. This definition does not accomplish much, as Hempel recognized. Suppose we have only one observation report that confirms h; is this as good as many reports? Obviously we need a concept of the degree of confirmation, either cardinal or ordinal. This further problem is the one that Carnap worked on for many years, and we will examine it presently.

However, Hempel, notes that we always have much more than one observation report, and in fact e is always very large. Observation reports about ravens are relevant to "All ravens are black," but reports on nonblack objects are also relevant. Since "All ravens are black" is logically equivalent to "If it's not black it's not a raven," anything that confirms the second hypothesis confirms the first as well. Consequently, any nonblack object that's not a raven confirms both versions of h, and a nonblack raven disconfirms both. "Consequently, any red pencil, any green leaf, any yellow cow, etc., becomes confirming evidence for the hypothesis that all ravens are black" (Hempel, 1965, p. 15). By similar reasoning, all nonravens also confirm h. In short, all possible observations about anything are relevant to any hypothesis; e is always infinite in number. To be sure, the class of nonravens and nonblack objects is larger than the class of ravens, but this is a mere empirical fact, Hempel observes, and thus is irrelevant to the logic of confirmation.

The above argument is known as "the paradoxes of confirmation" or *Hempel's paradoxes* and was the subject of much discussion among logical empiricists in the 1950s and 1960s, without any solution. More recently, the paradoxes seem to have been quietly ignored.

We turn now to Carnap's problem, how to measure the degree of confirmation of h by e. This is a problem in the logic of a priori probability, not in methodology. Carnap did not want to tell scientists what sort of evidence would be most relevant for confirming h—and indeed by Hempel's paradoxes any sort of evidence would be equally relevant, logically. He wanted only to get a number between zero and one for $C(h,e)$. To simplify the problem, he worked with a simple formal language, hoping to eventually extend any results there to actual science. Even so, the problem was difficult.

Goodman (1955) pointed out one basic difficulty. He argued that any e that confirms h_i also confirms an infinite set of alternative h_j, all contradicting h_i somewhat. Thus any degree of confirmation of h_l applies also to

h_2 . . . and since the h's are infinite and the degrees sum to 1, $C(h,e) = 0$. Any e provides zero confirmation for any h; confirmation is as impossible as verification. This is known as *Goodman's paradox*.

Goodman's example was unfortunate because it directed discussion in the wrong direction. He argued that any e that confirmed h_1 "All ravens are black" also confirmed h_2 "All ravens are blite," where "blite" means "being black until, say, 1991 and white after that." By varying the date, one can get an infinite series of hypotheses.

Unfortunately, the philosophers of science argued over the example and forgot the problem. They exclaimed, "Whoever heard of *blite*? If scientists would stick to familiar concepts, i.e., those in daily life, such odd problems would not come up." Others argued over whether *blite* even is a real color, or only a schmoller. Color, schmoller. But if scientists had stuck to familiar concepts we would never have discovered neutrons or the galvanic skin response, an observable.

Goodman pointed out a real problem, as Hempel has observed (1965, pp. 70–71). Any evidence that confirms (is consistent with) one hypothesis may also be consistent with somewhat different hypotheses. Thus it is a mistake to accept h, even if a great deal of evidence supports it, until we have ruled out, disconfirmed, some of the alternatives. How many alternatives must we rule out? Who knows? This in turn means that we must know some of the alternatives before we can test h. Testing is always a comparative process. Friedman's neglect of this point is one error in his "methodology of positive economics" (1953, chap. 1)—an error that he could not have known of in 1953, of course. In terms of significance tests, finding a significant correlation does not confirm a hypothesis that predicted the correlation, because a variety of other theories might also predict the same correlation. And worse, still other theories that define and measure the variables somewhat differently might produce still better correlations. In addition, we can never conclusively rule out any alternative theory, because the disconfirming observation reports might be mistaken.

Hempel's proposed solution to the problem takes us in a different direction. He argues that the acceptance of h_1 rather than h_2 is a pragmatic process that involves estimating the epistemic utility of the two and choosing the one with greater utility, not the true one. However, he does not explain how epistemic utility is measured.

Finally, the most fundamental distinction in the logical empiricist armory weakened and perhaps disappeared. This is the distinction between logical or analytic truth and meaning, and empirical or synthetic truth and meaning. Quine published the basic objection to the distinction in 1953. He did not question mathematical statements like "$2 + 2 = 4$," but focused rather on empirical entities like ravens and oculists. If all oculists are eye doctors by definition, who made the definition? If it's in the dictionary, where did the dictionary writer get it? Presumably he looked at common usage and practice, and decided that *oculist* and *eye doctor* meant the same thing.

Turning to science, then, we decide whether a sentence is analytic by looking at the meaning of its terms. According to logical empiricist doctrine, the meaning of a statement is its mode of verification; so we can compare terms by putting them into two statements and seeing whether they are tested in the same way. But by 1950 logical empiricists had agreed that a scientific theory is tested as a whole, not statement by statement, so we cannot perform the tests on two individual statements.

Nor can we by-pass meaning and just look at the definitions. Scientists make the definitions, not the dictionary writer, and scientists can change the definitions as a theory develops. If the data do not confirm a theory, the scientist has a range of freedom to decide which propositions or correspondence rules he will declare falsified and subject to change. The others then have been immunized from falsification by redefining them; they have been treated as analytic, since analytic sentences cannot be empirically falsified. The scientist will probably treat the basic propositions as immune and the auxiliary ones as falsifiable; but he has leeway to choose.

For example, a theoretical statement like "The ratio of permanent consumption to permanent income is a constant" ($Cp/Cy = K$) takes its meaning from the associated theory, which is connected at some points to possible observations. The statement cannot be tested directly, since neither permanent consumption nor permanent income is observable; they are defined within the theory, by auxiliary hypotheses and tentative operational definitions. These hypotheses and tentative definitions are used to estimate Cp and Cy.[2] Then the two sets of estimates are compared over time to see whether the ratio is a constant. But if the ratio varies erratically, such puzzling results can be corrected by reestimating on the basis of improved definitions. At some point, the ratio becomes approximately a constant. Does this mean that the statement has been treated as analytic, so that Cp/Cy will be operationally defined by whatever way it equals K? Or has one discovered the empirically correct definitions of *Cp* and *Cy* so that the empirical *K* is now observable? It's hard to say which it is, analytic or synthetic.

In the decade after 1953, others worked out variations and compromise positions. (See Putnam, 1962, for a clear statement and development of Quine's argument, and Suppe, 1977, pp. 67–80, for a summary of later arguments.) For instance, Feigl (1956) argued that the analytic-synthetic distinction is useful for clarity of thought, and it does apply to the artificially fixed languages that logicians invent. However, he agreed with Quine that it does not apply to growing and changing natural languages, like the language of actual science. In a growing language, a sentence that once was synthetic can become analytic as our definitions change. Thus actual scientific sentences do not fit into either category, analytic or synthetic.

THE IDEAL LANGUAGE

By the mid-1950s, all the original distinctions of the Vienna Circle had become unclear, and all the original certainties had collapsed. The distinctions between logical and empirical, analytic and synthetic, theory terms and observation terms, meaningful and meaningless, even science and metaphysics, had become differences of degree, circumstance, interpretation. The problem of meaning had become quite complex, and the problem of defining "confirmation" and "degrees of confirmation" for theories rather than for statements about ravens seemed insoluble. And worse, disputes over all these issues divided the originally unified logical empiricist school into many different positions. Some of the differences appeared in the contrasts between various units of the *Encyclopedia of Unified Science*, whose publication had almost ceased by 1955. The last unit of volumes 1–2 was assigned to a historian of science, Thomas Kuhn, and plans for additional volumes were dropped. Of course, when Kuhn's essay appeared in 1962 it was a disaster for the idea of a unified science built on logical empiricist foundations.

Carnap's proposed solution to these problems, from the 1930s on, was to design the language or languages of science in such a way as to avoid them. His original purpose in the 1920s had been to explicate the actual language of physics, and then other sciences, along the lines of *Principia Mathematica*. This proved difficult—impossible, in fact—and gradually his attention became focused on the difficulties philosophers had brought up, and away from the actual language of physicists.

By the 1950s Carnap and others were trying to construct simplified formal languages in which the problems did not occur (Scheffler, 1963, pp. 160–82). If one could solve a problem in a simple language, this might reveal a solution for the more complex languages of science (Hempel, in Hintikka, 1975, p. 10). Thus ideally any sentence that followed the rules of the desired language would have meaning in the language and would be either analytic or synthetic; the built-in correspondence rules would ensure that synthetic sentences were testable in principle; observation terms and theoretical terms would be clearly distinguished; and the degree of confirmation of a hypothesis by given observations could be exactly measured. Meaning would come directly from following the rules of the language; if the rules were followed, no metaphysics could sneak in. A further possibility considered in the 1950s was to build in some ethical terms, thus conferring cognitive meaning on selected ethical theories as well.

Some philosophers called this projected problem-free language the "ideal language" for science, though Carnap apparently did not use that term.

The logical foundations of science were now to be constructed some distance away from the actual practice of science. However, Carnap and associates hoped that once the foundations had been solidly constructed, some science at least could be moved over and set upon the new foundation. This would be accomplished by developing a more complex language based on the ideal language and then reformulating the theories, quasi-theories, and models of the science into it. Some portions of the old science would not translate, and these would be exposed as metaphysical, incoherent, untestable, or otherwise defective. The result would be a purified science that could advance free of the trouble and confusion that plagued existing science, including physics.

The construction of the ideal language proved to be a slow and difficult task (Radnitzky, 1970, pp. 30–36). Most philosophers of science were too involved in various ongoing disputes to lend a hand, and those who did take an interest contributed further criticisms and disagreements. Carnap did succeed in providing a way to measure the degree of confirmation of a simple hypothesis, according to Hempel, though Popper disagreed (1963, p. 220). Eventually Carnap died and the project stopped. With its end, the larger project of constructing a sound logical foundation for science also came to an end.

However, philosophers of science continued their previous activities; the previous means became the new end. They now defined their activity as the analysis and clarification of concepts having to do with science, concepts like those of explanation, prediction, theory, law, observation, test, truth. Such clarifications presumably would help scientists think more clearly. Thus Bergmann observed in a 1958 colloquium I attended, "Let's face it; these scientists need our help in thinking; they are confused."

For example, Hempel and Oppenheim in 1948 clarified the concept *explain* (Hempel, 1965, chap. 10). They argued that in a well-formed theoretical language a factual or lawlike statement is explained when it is deduced from a more general law or laws. The laws would have to be true, of course, and the empirical conditions of applicability would also have to be stated. This became known as the deductive-nomological model of explanation, or the D-N model. Actual scientific explanations could be rearranged and interpreted as approximations to the D-N model. They fell short insofar as the explaining laws were not known to be true, or were not all stated explicitly, and because the conditions of applicability of the laws were not all stated. Actual explanations were "explanation sketches" that would be gradually filled in as our knowledge expanded.

Philosophers then attempted to reduce various kinds of explanation to the D-N model, including functional explanations, teleological explanations, explanations by reasons, motives, intentions, dispositions, causes, and so forth (Nagel, 1961). It wasn't easy. A breakthrough occurred when Hempel constructed an inductive-statistical (or I-S) model, and this led to further discussion (Salmon, 1971). Historical explanations received much attention. As late as 1969, Rudner argued with great vehemence and conviction in a colloquium that historical explanations were *necessarily* deductive; they logically *had* to appeal to (unknown) laws of human behavior and state (unknown) circumstances of applicability. All historical explanations are deductive, and there are no historical explanations.

Another topic was whether explanation and prediction are logically the same; some said yes, some no.

By the 1960s, the original aim of explicating or rationally reconstructing science had moved far into the background. The central topic now was not the activities of physicists, but the puzzles and paradoxes of the received philosophical doctrine. Philosophers of science were no longer expected to be thoroughly at home in physics or some other science; indeed, they hardly seemed to know much science, other than a few stereotyped examples they picked up from other philosophers. They were at home in the ever expanding philosophical puzzles and disputes. When they came across some bit of social science, they tried to squeeze it into the received philosophical categories, as Rudner and Nagel did; if it didn't fit, they criticized the science, not their categories. One exception was Nagel (1961, pp. 503–20) on the ideal line of development of some discovered statistical generalization, a perceptive analysis.

Thus a philosophy of science that began with the intention of explicating, clarifying, the logic of a well-known science ended by issuing prescriptions to largely unknown sciences.

These philosophers who analyzed and disputed are most properly called *analytic philosophers* (Pap, 1949). The earlier name *logical empiricists* was less suitable by 1960 because the logical-empirical or analytic-synthetic distinction was by no means clear anymore. *Positivist* suggested tired old arguments about sense data and theoretical terms, where it referred to one of the various positions and not the others. *Analytic* was the only term that covered all the various schools and positions by 1960.

There were two kinds of analytic philosophers, the "ideal language" or IL and the "ordinary language" or OL kinds. The IL were descended from the Vienna Circle logical empiricists, and some still hoped for results from Carnap's "ideal language" enterprise. They analyzed a concept by examining the characteristics of some ideal or constructed symbolic language, and their arguments depended heavily on a machinery of postulates, axioms, definitions, theorems, and symbolic notation. The OL were followers of the later Wittgenstein, followers who suddenly appeared about 1950 as a compact and enthusiastic new school and who steadily made converts from the IL ranks during the 1950s. These people analyzed a concept by examining its ordinary use in some ordinary language like English. They argued, with later Wittgenstein, that the language of science grows out of ordinary language, not out of symbolic logic or Boolean algebra, and the confusions of scientists result from misuses of ordinary language. They would clarify a concept like *explain* by listing the uses of explain-words in OL and then seeing which of these uses appear in science. (Brown, 1960, found nine uses in OL and seven in science.) They proposed to dissolve philosophical puzzles like sense data and how we can know other minds by examining typical OL sentences like "I cannot have your toothache" (Bouwsma). They pointed out the errors of behaviorism by distinguishing the uses of "itching" and "scratching." They argued that all such problems had long ago been worked out in daily practice by users of OL, so that if scientists could only be brought to return to the ordinary uses of OL, their puzzles would dissolve.

The IL analysts ridiculed the whole OL program, arguing that science is different from OL and that in any case OL lags far behind science and gradually absorbs the new concepts scientists invent, so it cannot be an arbiter of scientific practice (Hempel, 1965, pp. 485–86). Much of the dispute in the period of about 1958–65 was between IL and OL analysts; in the IL works of these years the main opponent is OL analysis, not metaphysics. Heidegger was long since forgotten. After about 1964 the IL people also began to criticize newer movements in philosophy of science, beginning with Kuhn (Scheffler, 1967).

In the 1950s and 1960s, the main argument between IL and OL analysts was about causes and reasons. Adherents to IL argued that the goal of the social sciences necessarily was to state confirmed causal laws, because that was what science was, laws. OL defenders argued that the goal of the social sciences necessarily was to find reasons for actions, because human beings act for reasons, not causes; they have free will. Thus the free will versus determinism argument got mixed in; philosophers had forgotten Schlick's solution to this metaphysical argument, namely, that laws just describe what usually happens (1933–34, chap. 15). For example, Flew (1956) praised Freud for extending the study of reasons into the unconscious, and for inventing a talking cure that used OL, while IL people condemned Freud for not finding causes. Flew called psychoanalysis a particularly rational enterprise, an educative force of liberation and enlightenment (p. 168) because it looks for reasons, while von Eckhardt (1982) called it unscientific because it didn't conclusively test any true causal laws (cf. also Diesing, 1985b). Some IL proponents said that reasons *were* causes; others distinguished different kinds of causes and reasons, looking for a compromise.

This analytic and critical activity continued, aimlessly, through the 1970s, the only noteworthy change being the breakup of OL analysts into various groups. Some IL advocates, including Cunningham (1972) and Leinfellner (1983), became Marxists or pro-Marxists, and used their logical skills to analyze and "clarify" various concepts in the Marxist literature.

The continuation of an activity after the official reason for it has disappeared is a familiar phenomenon of bureaucratic politics. Thus Kharasch observes that the real purpose of an organization is to continue its regular activity, and its official aims are pretexts or official justifications (1973, pp. 13, 24). If the official aim is achieved, or becomes completely unachievable, the organization will continue its activity, either by adopting a new goal or by emphasizing the preliminary nature of its work. For instance, suppose Congress were to decree that a replica of Mount Vernon should be constructed out of cottage cheese (1973, p. 112). The implementing agency would never report that the assignment is impossible; no, it would make endless "preliminary" studies of curds and whey, textures, and design specifications, all officially preparing for the great day when Mount Vernon would rise curd on curd. Or, in this case, when the Logic of Science would rise word on word.

The analytic philosophers continued their "clarifying" activity long after projects to "explicate science," "eliminate metaphysics," and "construct logical foundations" had ended, *because that was what they knew how to do*. Of course, they justified this activity, vaguely, as being helpful to science, but that was a pretext. That it was a pretext was clear from the fact that they did not seek feedback from science to see whether they had indeed been helpful. They did not check to see whether the scientists using some "confused" term were actually in trouble and needed help (Achinstein, in Achinstein and Barker, 1969, pp. 268–69, 290–91). Had they checked, I believe they would have found that their activity was of almost no relevance to the social sciences. Or had they actually collaborated in research, as Frederic Fitch did in the late 1930s in a learning theory project (Hull et al., 1940), they might have found something that needed clarification. Instead, if they noticed that some scientific activity did not fit their specifications, they simply condemned it as immature or unscientific. One exception to the above is the recent Sneed-Stegmüller structuralist school, which will be discussed in chapter 3.

PRELIMINARY EVALUATION

At this point, the only basis we have for evaluating logical empiricism is the basis provided by the movement itself. We can record the judgments analytic philosophers have made on their own past, their estimates of achievements and failures. Where their judgments disagree strongly, we must suspend judgment. Later chapters will provide perspectives that yield new insights, interpretations, and judgments. Though these later judgments are almost uniformly negative, we must remember that every negation is also an affirmation insofar as it builds on the negated material.

This movement approaches science from far above, from the ideal of perfect knowledge. Thus the treatment of testing begins with the ideal of complete verification, moves down to confirmation, and then gets tangled in Hempel's and Goodman's paradoxes. It never does get to the real problem of how one constructs an alternate hypothesis relevant to some test; that belongs to discovery, not logic. Explanation is defined first as deduction from true, verified laws with all relevant circumstances specified; then the analysis moves down to partial explanation, involving some well-confirmed generalizations and some circumstances, and ends with explanation sketches involving a somewhat confirmed hypothesis and unknown circumstances. *Theory* is defined first as a fully axiomatized structure of axioms, postulates, definitions, and theorems. All concepts are defined first for a fully mathematical-symbolic IL and later, if at all, extended to current scientific languages.

Actual sciences are interpreted as approximations to the ideal. Consequently the movement downward encounters first the most advanced science, physics, in its most advanced practitioners, Einstein and Newton. Physics has a theoretical language, concepts, laws, and some fairly complete partial explanations. As we move down the scale to the more imperfect sciences, we find that they resemble the ideal less and less. Thus political science, according to Isaak (1975), has no theoretical concepts, no theory, of course no laws, and consequently no explanations, though it has explanation sketches, prototheories, and a great deal of heuristics.

This approach to science brings with it a ready-made conception of scientific change: real change consists in moving up the ladder closer to the ideal. The immature social sciences should move up through economics toward physics, and physics should become more fully axiomatized and its laws should become more general and simpler. There was some attempt in the 1950s to argue that physics had actually done this in the move from Galileo to Newton to Einstein (Nagel, 1949); Galileo's laws were a special case of Newton's laws for objects near the earth's surface, while Newton's laws were a special case of Einstein's laws for objects in the solar system. However, Feyerabend's argument (1962) demolished this attempt. In general, the treatment of scientific change is normative—science ought to develop toward the ideal if it is to be real science—and analytic philosophers have avoided the study of actual change in science. The history of science is distinct from the philosophy of science for them.

Scientific practice is also outside the analytic philosopher's domain.

Logical empiricism deals with the logic of science, its syntax and semantics, not its pragmatics. It is not just the context of discovery that is excluded from philosophy; the context of justification as it actually occurs also belongs to pragmatics. All that is included is the logic of justification—that is, what structure a confirmatory argument must have to confirm a theory.

The ideal of science is derived from Hume and from early Russell's atomistic metaphysics, as embodied in the symbolic logic of *Principia Mathematica*. If reality consists of many entities with attributes and dispositions, then knowledge must consist of the regularities that connect attributes and dispositions to various kinds of entities. The typical scientific sentence therefore is "All ravens are black." For Hume, *regularities* was another name for *causes*, so science consisted of causal laws. This identification was unsatisfactory for many of his twentieth-century followers, who tried to distinguish causal laws from regular coexistence or succession in time. The regular blackness of ravens might be an accidental coexistence rather than a true causal law. However, this topic remained unsettled.

Since the ideal is a statement of what knowledge must necessarily be (given an atomistic metaphysics), it is not derived from scientific practice. If practice and ideal differ, the practice is deficient. However, in the intermediate area between ideal and practice, there is some tendency to compromise. A treatment of confirmation, or partial explanations, or nonaxiomatized proto-theories might try to reconcile the logical analysis and the practice. The argument is that the better physicists are already approximating the ideal, so the logical structure of their theories and tests should be a guide to the philosopher in his reconstruction of the logic of the intermediate area. Of course, the immature sciences are no guide to logic at all because they are so deficient in it.

The early logical empiricism of Wittgenstein's *Tractatus* (1922) and Ayer's *Language, Truth, and Logic* (1936) is now entirely discarded as too simple and abstract. Ayer recently asserted, partly in jest, that it was all false (1987, p. 27). It pointed the way, but its sharp distinctions have softened or disappeared, and its naive certainties have given way to problems and complexities. As Hempel observes, the successive changes in Neurath's originally bold and simple doctrine of physicalism have made the doctrine increasingly cautious, elusive, and weak (in Achinstein and Barker, 1969, p. 190). The reasons why people needed those simple distinctions and certainties have been forgotten; they are merely historical circumstances, not logic.

The newer analytic philosophy comes from the 1940s and early 1950s. In place of the naive analytic-synthetic, logical-empirical, meaningful-meaningless, observation-theory distinctions, there is a vague, unsatisfactory distinction between theory language and observation language, with theory connected to observation sentences by occasional rules of correspondence. Theoretical terms, including probably some metaphysical ones, get their meaning mainly from other terms in the theory, and ideally from the theory's axioms, postulates, and definitions. As a theory develops, its empirical meaning increases through increased connections to observations. Many observation terms, conversely, have some theory built into them or into the instruments producing the observations.

The big achievement of the newer analytic philosophy is Hempel's work on explanation, including the D-N, D-S, and I-S models and also Salmon's R-S model. (For a recent statement, see Van Fraassen, 1980.) The D-N and D-S models were not developed empirically from scientific practice but were derived from the ideal of perfect knowledge, with some heuristic guidance from examples. If science consists of laws and if laws are deduced from axioms, postulates, and other laws in a theory, then a scientific explanation must have a law and a deduction in it somewhere, or it is not even science. When analytic philosophers look at an actual explanation, they try to find a law or generalization in it somewhere. If they find one, the explanation is deductive; if they do not, it is no explanation at all, but only a description. The I-S model was developed later, in the early 1960s, for the intermediate region between advanced real science and the ideal science, so it combines inductive logic with some reference to scientific practice.

The unsolved and perhaps unsolvable problems include the logic of confirmation, the process of observation, the meaning of meaning, and the quest for an ideal scientific language. *Confirmation* can be defined, but there is no measure of the degree of confirmation, and Goodman's and Hempel's paradoxes bar the way to ever getting a measure other than zero. Nor is there any logical procedure for testing a hypothesis. By Hempel's paradoxes, absolutely any observation is equally relevant to testing any hypothesis, so logic offers no guidance here. Testing must be regarded as a pragmatic process. The simplified versions of logical empiricism such as Isaak (1975) and Payne (1975) either go back to the early logical empiricists and talk of sense data, or dodge the problem by asserting that there *must* be *some* valid process of induction (Isaak, 1975, pp. 94–97). "If there is any way to know the world, it is by induction" (p. 97; cf. also Feigl, 1971: If anything works, then induction does). Carnap never completed his inductive logic project, even for the ideal language. Isaak, however, agrees that induction and therefore testing is a pragmatic process.

The related issue of how scientists produce valid observation reports has also been consigned to the realm of practice; that is, scientists seem to do it somehow. Here, again, if there were no valid observations, there could be no science, so scientists *must* somehow have some. Ayer's early certainty that the red patch before me really is a red patch has been discarded as inadequate to the complexity of actual scientific observation, or even to the observation "This is a raven," but no other certainty has replaced it.

The issue of meaning that once seemed so simple has gotten lost in the complexities of scientific theories and their varied relations to observation. As for the ideal language, the attitude seems to be that it would be good for logic if we had one, but nobody seems to be constructing it and it probably is too difficult to be worth doing.

These insoluble problems plus other disputed issues like the relation of explanation to prediction do not suggest to analytic philosophers that they are on the wrong track, since they know of no other track. Rather, they suggest that there is yet much work to do. However, each of these difficulties except the ideal language one has served as a point of departure for some other movement. In addition, the omitted realms of history, practice, and the growth of science have been central to other movements. Thus the failures and omissions of the logical empiricist-analytic movement have had a double effect; internally they have captured IL analysts in a net of endless, aimless dispute, and externally they have given impetus to the growth of antithetical movements.

What do recent analytic philosophers have to offer to social scientists? Basically they offer a clarification of concepts like *explain*, *theory*, *law*, *confirm*. But since social scientists do not have any of these things, the clarifications are not exciting. The main message is, "You have no science. But keep trying."