THOMAS MORMANN

6 The Structure of Scientific Theories in Logical Empiricism

THE THEORY QUESTION

A central question of philosophy of science, arguably the most central one, is "What is the structure of scientific theories?" Some contemporary philosophies of science have challenged the centrality of this "theory question" (henceforth T-question), arguing that it is more important to get an adequate understanding of the practice of scientific inquiry. Yet the logical empiricists placed great emphasis on theory: "Theories ... are the keys to the scientific understanding of empirical phenomena: to claim that a given kind of phenomenon is scientifically understood is tantamount to saying that science can offer a satisfactory theoretical account of it" (Hempel 2001 (1970), 218). Hence, investigating the structure of these "keys" ought to be a central task for philosophy of science. As has been often observed, the basic problem of logical empiricism was how to be a good empiricist and at the same time "logical." Or, to cast it in a more historico-philosophical setting, the problem was how the empiricist legacy of philosopher-scientists such as Ernst Mach and Pierre Duhem could be combined with the exigencies of modern logic and mathematics. As will be shown, this problem is intimately related to the problem of answering the T-question in an acceptable empiricist way.

The importance the logical empiricists attributed to the T-question does not mean that they always formulated it explicitly. Often they addressed it by asking questions about the nature of scientific knowledge, or they embedded it in more general problems such as "What is the structure of empirical science?" or "What is the structure of the language of empirical science?" Another, more empiricist way of dealing with the T-question was to investigate the structure of what may be called the empirical evidence of empirical theories. This was done in the so-called protocol sentence debate of the Vienna Circle that took place in the early thirties of the last century (see Uebel 1992). In their mature accounts, however, the logical empiricists intended to answer the T-question quite explicitly (e.g., Carnap 1966 or Hempel 2001).

In the philosophy of science of the twentieth century one may distinguish three different kinds of answers to the T-question: according to the *syntactic* view a theory essentially has the structure of an axiomatized system of sentences. This has been challenged by the *semantic* view that conceptualizes a theory as a collection of nonlinguistic models, mathematical ones or of other types. Both accounts are opposed by the view that a theory is a more or less amorphous entity consisting of sentences, models, problems, standards, skills, practices, etc. Not having a better word, we may call this a *pragmatic view.* Usually, logical empiricism is identified with a strictly syntactic view. As we shall see, this claim is in need of qualification.

The T-question has a bearing on all central philosophical topics logical empiricism struggled with, in particular, the analytic/synthetic distinction, the difference between mathematical and empirical theories, problems of verification and meaningfulness, description and explanation, and questions concerning the realist or instrumentalist character of scientific knowledge. It goes without saying that we cannot deal with all these topics in detail. Rather, we will concentrate on problems of meaningfulness of scientific terms, and how the difference between mathematical and empirical theories is to be conceptualized. With respect to the T-question, Carnap was the most influential figure among the logical empiricists. Often, his answer has been identified with *the* logical empiricists answer in general. This is misleading. The accounts of Reichenbach,[1] Neurath, Hempel, or Feigl, to name but a few, cannot be considered as inessential variations of Carnap's. Moreover, Carnap's thought underwent important changes during the almost 40 years when he was dealing with the problem. Hence, *the* answer of the logical empiricists to the T-question does not exist. Rather, we

[1] The variant represented by the work of Reichenbach will not be dealt with here since his work features prominently in the discussion of the logical empiricist philosophy of physics in Chapter 8.

find a family of more or less closely related accounts covering a much larger spectrum than is usually acknowledged by the post-empiricist critics of logical empiricism. As will be treated in some detail, logical empiricism not only comprised strictly syntactic approaches to the T-question, but also semantic and pragmatic ones that took into account historical and sociological aspects of scientific theorizing.

The outline of this chapter is as follows: In Section II we recall the essential components that determined the logical empiricists' answers to the T-question: on the one hand, the problem of taking into account the conventional, logical, and mathematical aspects of our theories, and, on the other hand, to do justice to the empirical character of our knowledge. As will be shown, a particularly important role for the emergence of a genuine logical empiricist account of scientific theories was played by the axiomatization of mathematical theories whose main exponent was Hilbert (see Hilbert 1899; 1901; 1916). As is shown in Section III, Carnap's early answers to the T-question may be described as a stepwise emancipation from the dominating pattern of axiomatization of mathematical theories. Neurath's maverick "encyclopedic" approach is treated in Section IV. His account reveals that logical empiricism was in no way restricted to a strictly syntactic approach. In Section V we discuss several stations of Carnap's thinking culminating in his *The Methodological Character of Theoretical Concepts* (1956). Section VI deals with the solution of the problem of theoretical terms proposed by Ramsey already in 1929 and reinvented by Carnap in the late fifties. In Section VII some of the later logical empiricist approaches (in particular Hempel's and Nagel's) are considered. They may be characterized by the fact that they took into account pragmatic and historical considerations. This feature is usually attributed to the postempiricist approaches only. In Section VIII we close with some observations on the relation between logical empiricists and postempiricist answers to the T-question.

AXIOMATIZATIONS AND CONVENTIONS

Any answer to the T-question is marked by the specific scientific and philosophical context in which it is located. In the case of logical empiricism, this truism leads into a thicket formed by the interpretations, adaptations, and sometimes even plain misunderstandings of the doctrines of the philosophers and scientists that influenced the logico-empiricist approach. Let us concentrate on two figures who represented complementary currents in the debate dealing with the T-question, David Hilbert and Duhem. Hilbert is the exponent of the axiomatic approach that had provided a widely accepted answer to the T-question for mathematical theories, while Duhem is the scientist-philosopher who insisted that the structure of empirical theories is essentially different from that of the mathematical or formal theories, acknowledging at the same time the importance and even indispensability of mathematics for empirical science. Hence, ''Hilbert'' and ''Duhem'' should not be considered simply as components to be combined in one way or another; rather, they act as forces pulling in opposite directions.

Hilbert's axiomatization of mathematical theories, in particular geometry, provided a fairly satisfying answer of the T-question for mathematical theories. Mathematical theories are to be conceived as relational systems whose entities are defined by implicit definitions that may be considered as axioms in disguise. As an early logical empiricist witness for Hilbert's influence one may mention Schlick's *General Theory of Knowledge,* where geometric concepts are regarded as entities ''whose only being consists in being bearers of the relations laid down by the system of axioms'' (Schlick 1918, § 7/ 1985, 34).[2] The challenge for an empiricist philosophy science was to adapt Hilbert's answer to the case of empirical theories. Hilbert himself had put this problem on the agenda: in his epoch-making lecture delivered at the International Congress of Mathematicians at Paris in 1900 he had stated his ''Sixth Problem'' in the following way: ''The investigations on the foundations of geometry suggest the problem: to treat in the same manner, by means of axioms, those physical sciences in which mathematics play an important part; in the first rank are the theory of probabilities and mechanics'' (Hilbert 1901, 14).

It seems that Hilbert considered the axiomatization of physical theories perhaps ''more complex'' than that of geometry, but not in principle different. His approach tended to blur the difference between mathematics and the empirical sciences. Indeed, Hilbert

[2] This is almost exactly the definition of theoretical terms given by Hintikka (1998).

maintained that given what he had determined as the fundamental equations of his *Grundlagen der Physik:* "the possibility is approaching that in principle physics becomes a science of the kind of geometry: certainly the most magnificent glory of the axiomatic method which here, as we see, put the most powerful instruments of calculus, i.e., the calculus of variations and invariant theory, to its service" (Hilbert 1916/2001, 407). Most physicists and philosophers did not share Hilbert's enthusiastic expectations. Although virtually all logical empiricists agreed on the importance of Hilbert's work in the axiomatization of mathematics, they remained skeptical with respect to the axiomatization program as formulated in the *Sixth Problem.* The basic reason for their reluctance probably was that they considered it as a metaphysically charged program threatening to deprive the natural sciences of their empirical foundations. An important ally in the logical empiricists' struggle against the empiristically unacceptable metaphysical tendencies in Hilbert's axiomatization program was conventionalism as presented in the works of French scientist-philosophers such as Duhem, Henri Poincaré, Abel Rey, and others.[3] Duhem was a vigorous defender of the empirical character of empirical theories. He subscribed to a strictly antimetaphysical conception of science free of those Kantian ingredients that could have disturbed the logical empiricists, whereas Poincaré's philosophy of science had somewhat heterodox Kantian features.

Accepting that a purely mathematical axiomatization was not a fully satisfying answer to the T-question for empirical theories, the logical empiricists began to develop other models of the structure of empirical theories designed to incorporate an axiomatic mathematical system as only one component of the more complex conceptual apparatus of an empirical theory. Thereby they came to describe pictorially the structure of an empirical theory as a "free-floating" system of concepts which mutually determine their respective meanings somehow anchored in reality. This geometric metaphor can be traced back to Schlick (1918) but can be found in the works of

[3] In the following I concentrate on Duhem not only for reasons of space but also because Duhem's radical conventionalism is the version of conventionalism that is most congenial to Logical Empiricism. This is not to deny the important role Poincaré played for many Logical Empiricists.

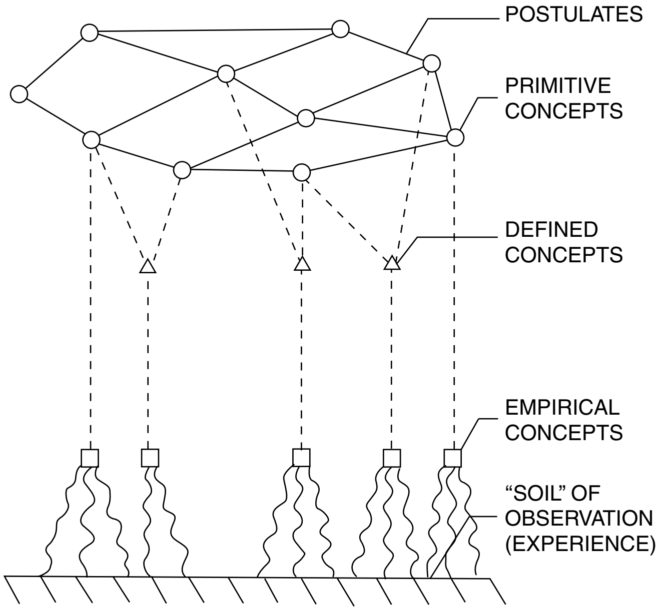

FIGURE 6.1. A logical empiricist picture of a scientific theory. Reprinted with permission from Feigl, Herbert. "The Orthodox View of Theories: Remarks in Defense as Well as Criticism." In Michael Radner and Stephen Winokur (eds.), *Theories of Physics and Psychology,* Vol. 4, Minnesota Studies in the Philosophy of Science. Minneapolis, MN: University of Minnesota Press, 1970, p. 6.

various authors, for example, Carnap (1939, 1966), Hempel (1952), or Feigl (1970). Its content may be illustrated by the well-known diagram shown in Figure 6.1

A pictorial diagram such as this one can be interpreted in many ways. The logical empiricists, in rough terms, interpreted it in terms of a two-language model of empirical theories: the structure of an empirical theory T is described by the structure of the language L in which T is formulated. L has two essentially different components, to wit, a vocabulary LO used for the description of the lower empirical level, and a vocabulary LT dealing with the formulation of concepts and postulates of the upper theoretical level. Moreover, there was assumed to be some sort of translation manual which allowed (at least partially) one to interpret the statements of the upper theoretical level in terms of the empirical level with the help of correspondence rules or bridge laws. In this way, there was a kind of "upward seepage" of meaning from the observational terms to the theoretical concepts (see Feigl 1970, 7). As will be discussed in more detail below, the alleged "seepage" of meaning pointed toward a crucial problem of the logical empiricists' account of empirical theories, to wit, how the "free-floating" theoretical concepts obtained at least some kind of empirical meaning whereby they could be distinguished from meaningless metaphysical terms.

The above diagram is to serve only as a rough map; even the "received view" attributed to the logical empiricists in the fifties and sixties is more complicated (see Suppe 1989). Nevertheless, the pictorial description provided by this diagram may serve as a first orientation of the core problem that the logical empiricists from Schlick onwards were struggling with, namely, philosophically to understand how empirical science succeeds in bringing together two different components, the theoretical and the empirical. The problematic relation of these two components is already dealt with in Duhem's *The Aim and Structure of Scientific Theory* (1906). According to him, a physical theory "is a system of mathematical propositions, deduced from a small number of principles, which aim to represent as simply, as completely, and as exactly as possible a set of experimental laws" (Duhem 1906, 19). The main aim of such a representation is "intellectual economy" in the sense of Ernst Mach, not a true description of how the world "really looks." To achieve this kind of economic representation, a physical theory has to deal with two different kinds of facts: on the one hand, one has the symbolic (or theoretical) facts expressed in the language of pure mathematics, and on the other hand, the experimental (or practical) facts described in ordinary language. One of Duhem's most original theses maintained that there is no direct translation between the two areas. Rather, there is a many-many correspondence: to every symbolic fact there corresponds an infinity of experimental facts and vice versa. It is up to the scientist to interpret these correlations in an adequate manner (Duhem 1906, ch. 8). Duhem's distinction of symbolic and experimental facts was not immediately acceptable to logical empiricists. They could not settle the empirical character of empirical theories simply by ontological stipulations; rather, they had to rely on the structure of the language(s) of empirical sciences.

Thus, the common format of all logico-empiricist answers of the T-question is that they attempt to explicate the specific structural features of the languages of empirical sciences which render them empirical sciences.

EARLY ANSWERS

Maintaining a difference between the empirical and the purely mathematical, and, at the same time, giving a plausible answer to the problem of how science succeeds to "[master] reality through ... systems of hypotheses and axioms" (Neurath, Carnap, and Hahn 1929/1973, 311) was a problem that concerned not only the logical empiricists. The neo-Kantians of the Marburg school, the most important rival of logical empiricism in the 1920s, were at pains to explain the differences between mathematical and empirical sciences in a way that did not to blur the distinction between both domains in an untenable way. Although logical empiricists and neo-Kantians started from a similar base, in the end they came to different conclusions. Nevertheless, in the twenties neo-Kantians' and logical empiricists' answers to the T-question had much in common. In *Substance and Function* Cassirer proposed the following differentiation between mathematical and empirical concepts:

> In contrast to the mathematical concept, however, in empirical science the characteristic difference emerges that the construction which within mathematics arrives at a fixed end, remains in principle *incompleteable* within experience. But no matter how many "strata" of relations we may superimpose on each other, and however close we may come to all particular circumstances of the real process, nevertheless there is always the possibility that some co-operative factor in the total result has not been calculated and will only be discovered with the further progress of experimental analysis. (Cassirer 1910, 337/1953, 254)

According to Cassirer, the difference between mathematical and empirical concepts resided in the fact that the latter are open ("incompleteable"), whereas the former are closed: the implicit definition of a point in Euclidean geometry fixes the meaning of this concept once and for all. Cassirer claimed that the key concepts of empirical science had a "serial form" (*Reihenform*) in that their meaning was not fixed once and for all by a single theoretical framework; rather, it emerged in a series of theoretical stages in the ongoing evolution of scientific knowledge.[4] His thesis resembled the guiding idea that would be of utmost importance also for the logical empiricists' approach to the T-question, namely, that the theoretical concepts of empirical theories are somehow "open" concepts, lacking full meaning and being only partially determined by any given conceptual framework. In contrast to Cassirer, however, the logical empiricists were not content to express this idea only in a vague and metaphorical way.

When Carnap started investigating the T-question in the late twenties, he began in a rather ingenuous manner emphasizing the close resemblance between mathematical and empirical concepts. According to him, both were closed, that is, fully determined within one system. The most naive answer to the T-question Carnap ever gave may be found in his *Abriss der Logistik* (Carnap 1929). It closely followed the lines of the axiomatic approach outlined by Hilbert in *Foundations of Geometry* (Hilbert 1899). As a dedicated follower of Whitehead and Russell's *Principia Mathematica,* Carnap considered the "theory of relations" as the most important part of logic, and the axiomatization of scientific theories as the most important application of the theory of relations. It is "expedient for the presentation of conceptual systems and theories ... of the most different realms: geometry, physics, epistemology, theory of kinship, analysis of language etc. Thereby, the definitions and deductions in these areas get a precision that can hardly be obtained otherwise which does not hinder, but rather facilitates the practical work" (Carnap 1929, 2). One should note that here Carnap did not make a difference between mathematical and empirical theories. Both can be axiomatized in the same way as had been suggested already Hilbert almost 30 years earlier.

It was in the conceptual practices of mathematics and empirical science that a difference emerged eventually. Although in *Abriss* such a difference was still lacking, already here Carnap pointed out

[4] This claim may be traced back to the core thesis of the Marburg neo-Kantian school, according to which the "fact of science is a fact of becoming" (Natorp 1910, 14).

that the explication of the structure of empirical theories is ultimately a practically motivated endeavor:

> The [*Abriss*] ... does not intend to present a theory, rather it aims to teach a practice. ... The result of a logicist treatment of a domain is first an analysis of its concepts and assertions, and then a synthesis in two forms: the concepts of the domain are defined step by step from some appropriate basic concepts and thereby ordered in form of a conceptual genealogy ("constitutional system"); the assertions are deduced step by step from appropriately chosen basic assertions ("axioms") and thereby ordered in a genealogy of assertions ("deductive system"). (Carnap 1929, iii)

At the time, axiomatizing was for Carnap "simply to order the sentences on the one hand, and the concept of some non-logical domain according to their logical dependencies" (Carnap 1929, iii). Although axiomatization may be regarded as a fruitful heuristic for the logical analysis of scientific theories, it did not answer the epistemological and ontological problems concerning empirical knowledge. In particular, it did not express any difference between the empirical and the mathematical. But, obviously, there *is* a difference between mathematical and empirical theories. Not *all* concepts of empirical theories can be defined by implicit definitions. There is more in empirical theories than implicitly defined mathematical structures. The problem is to find out what it is, and how it affects the structure of empirical theories.

What renders empirical theories empirical may best be studied by investigating their empirical bases. At least this was the way the logical empiricists tackled this problem in the so-called protocol sentence debate.[5] Often logical empiricism has been blamed for having naively accepted the "myth of the given" according to which there is an unproblematic stratum of empirical knowledge on which the more lofty stages of theoretical knowledge can be built.[6] A closer inspection of protocol sentence debate reveals that this is an oversimplification. Although sometimes the empirical base was indeed called "the given," the logical empiricists did not consider "the given" as something unproblematic. The problem of the given rather was the question of the proper form and structure of the statements

[5] For a detailed presentation of the various stages of this debate see Uebel (1992).
[6] The diagram of Section II may convey such an idea.

that deliver empirical evidence. Interpreted in this manner, the protocol sentence debate was one episode in the logical empiricists' ongoing struggle to come to terms with the T-question by concentrating on the component of empirical theories complementary to the one that "Hilbertian" answers of the T-question emphasize, the "free-floating" theoretical concepts.

Of course, neither the empirical base nor the theoretical structure can be studied in isolation. A balanced account has to investigate both components. Hence, from the mid-1930s onwards, Carnap and other logical empiricists described empirical theories by two complementary types of concepts. Their characterization varied: sometimes they were called "primitive" versus "introduced" (Carnap 1936/37), then "elementary" versus "abstract" (Carnap 1939) or "observational" and "theoretical" concepts (Carnap 1956). The varying interpretations of the two kinds of concepts show that what is most important is their complementarity. Their duality aims to account for the specific practice of empirical theories, which comprises activities as testing, confirming, or (conditional) falsifying that have no (direct) analogues in the formal sciences. Logical empiricist answers to the T-question are best described as attempts to characterize the linguistic practices of empirical theories that distinguish them from mathematics and other formal sciences. Carnap's proposals maintain that empirical theories are characterized by "open" concepts. In the first approximation, this openness can be considered as a partial underdetermination that may be reduced in the ongoing development of science. This does not, however, make the open character of scientific theories disappear, since new underdetermined "theoretical" concepts may be introduced. In *Testability and Meaning* Carnap intended to capture the distinct conceptual practice of the empirical sciences under the rubric of confirmation and testing. The point was to explicate the open character of dispositional terms of empirical theories. Dispositional terms being forerunners of the later theoretical concepts, it is therefore of secondary importance whether Carnap gave a satisfactory account of dispositional terms or not. Rather, his account is remarkable for being his first attempt to distinguish between formal and empirical theories in terms of the different kinds of concepts they use.

NEURATH'S ENCYCLOPEDISM

More than any other logical empiricist, Neurath was convinced that an adequate answer of the T-question for empirical theories could not consist in modifications of the answer Hilbert had given for mathematical theories. Although he did not deprecate the role of logic and mathematics for empirical science, Neurath always insisted that empirical theories were of a quite different sort from mathematical and other formal theories. For him, formalization and mathematization were useful tools but with a limited scope of application. He was one of the few logical empiricists who did not take it for granted that scientific theories should be thought of as formal axiomatic systems endowed with some sort of empirical interpretation. Instead, he aimed at an explication of those characteristics of scientific theories that do *not* fit the neat and clean systems that preoccupied his colleagues. Calling his account "encyclopedism," he stressed the contrast between "encyclopedia" and "system," the latter being an axiomatized system of propositions in Hilbert's sense. Hence, the central task of Neurath's encyclopedism was to answer the question "What is the structure of an encyclopedia of unified science?" According to him, the attempt to answer this question by invoking some sort of "deductive system" was to commit the sin of metaphysical "pseudorationalism" overstating the possibilities of human rationality.

The basis of Neurath's encyclopedism was a robust physicalism according to which all intellectually respectable concepts can be defined ultimately in terms of physicalist concepts and/or the concepts of logic and mathematics. It is important to note that physicalistic concepts are not to be identified with concepts of physics. Rather, Neurath placed the everyday language of spatio-temporally located things and processes in the center. This physicalist everyday language had to be cleansed of metaphysical phrases and possibly be enriched by scientific concepts. Hence, the Neurathian physicalist language as the language of unified science is a mixed language. Moreover, this "universal jargon" unavoidably contains precise *and* vague terms. On the one hand, one has the physicalist base language with its unclear and ambiguous common day concepts, called by Neurath *Ballungen* ("congestions"); on the other hand, there is the "highly scientific language" with its neat "formulas." For him, the common language with its *Ballungen* is an indispensable part of empirical science that can never be eliminated in favor of a clean and fully analyzed language:

> If we want to embrace the entire unified science of our age, we must combine terms of ordinary and advanced scientific languages, since in practice, the terms of both languages overlap. There are certain terms that are used only in ordinary language, others that occur only in scientific language, and finally terms that appear in both. In a scientific treatise that touches upon the whole range of unified science, therefore, only a "jargon" that contains terms of both languages will do. Neurath (1932–3/1983, 92)

A difficult problem faced by Neurath's account is how the interactions of the precise and imprecise elements are to be conceptualized. This problem corresponds to that in the standard account of how the *correspondence rules* or *bridge laws* are to be conceptualized. Neurath did not say very much on this topic; he insisted, however, on the general thesis that an empiricist could never accept an answer to the T-question that ignored the part of the *Ballungen* and solely dealt with that of "formulas." This means that an empiricist answer to the T-question cannot be content with an answer that takes into account only Hilbertian aspects of empirical theories.

Neurath's model of scientific knowledge may be characterized as pragmatic in the sense that it conceived an empirical theory as an inhomogenous entity that comprises components that fall under different syntactical, semantic, and pragmatic categories. In his criticism of Popper's falsificationalism, he described his conception of empirical knowledge as follows:

> We start from masses of sentences whose connection is only partly systematic, which we discern only in part. Theories and single communications are placed side by side. While the scholar is working with the help of part of these masses of statements, supplementary additions are made by others, which he is prepared to accept in principle without being quite certain what the logical consequences of this decision might be. The statements from the stock with one really works use many vague terms, so that "systems" can be always be separated only as abstractions. The statements are linked to each other sometimes more closely, sometimes more loosely. The interlocked whole is not transparent, while systematic deductions are attempted at certain places. ... one could say that we ... start from

model-encyclopedias; this would express from the outset that systems of clean statements are not put forward as the basis of our considerations. (1935/1983, 122)

Summarizing, we may say that in Neurath's encyclopedism the T-question appears in the form "What is the structure of a 'model-encyclopedia'?" Neurath's answer to this question is largely negative: encyclopedias do *not* have the structure of axiomatized systems, deductive derivations do *not* play an all-embracing role, etc. Rather, encyclopedias exhibit the local, limited, and often ambiguous character of human knowledge. Although Neurath concentrates on encyclopedias instead of theories, his encyclopedism nevertheless exhibits an analogy to the orthodox two-language account with his stress on the complementarity of exact mathematical or logical formulas and imprecise *Ballungen.* Hence, with respect to its underlying structure Neurath's special brand of logical empiricism may not be so special after all.

ON THE ROAD TOWARDS A THEORY OF THEORETICAL CONCEPTS

In "Testability and Meaning" Carnap treated the T-question from a decidedly dynamic point of view: the main problem of philosophically understanding empirical science was to understand how *new* terms may be introduced into the scientific discourse and how they become endowed with meaning. There were essentially two methods to achieve this. The first we already know from the mathematical and other formal sciences, the method of explicit definition. This method also is applied in the empirical sciences, but the characteristic method of these sciences is another one, namely, the method of reduction.

Let us consider the following simplified example.[7] Suppose a scientist wishes to introduce a new predicate Q, for instance, "spin," "helicity," or whatever.[8] He may do this by an explicit definition,

[7] The account of reduction pairs Carnap proposes in "Testability" is actually more sophisticated than the one sketched here. For our purposes, however, the technical details are irrelevant.

[8] As a concrete special example for the method of introducing new terms by reduction, Carnap discussed dispositional terms such as "visible," "fragile," or

but often, he will be able to determine the meaning of Q only partially. For certain empirical circumstances Q1 he possesses experimental methods that allow him to claim that Q obtains, and given other empirical circumstances P1 that Q does not obtain. In still other circumstances he simply does not know whether Q obtains or not. Formally, this may be expressed by a "reduction pair" (Q1, P1) of sentences:

$$Q1 \rightarrow Q \text{ and } P1 \rightarrow \text{not } Q.$$

Carnap's procedure can be rendered more perspicuous by assuming that we have an interpretation of the predicates involved. Then predicates P, Q ... are represented by their extensions, that is, the sets of objects to which they apply, and a reduction pair (Q1, P1) may be considered as an approximation of the extension of Q "from below" and "from above" by the inclusions $Q1 \subseteq Q \subseteq P1$, **C**P1 being the set theoretical complement of the extension of P1.

The reduction pair (Q1, P1) partially determines the predicate Q in that it asserts that the extension of Q contains that of Q1 and is disjoint from P1. This is only a partial determination that may be further improved by further reduction pairs (Q2, P2), (Q3, P3), etc. The introduction of new terms by the method of reduction pairs has the advantage that it renders the development of scientific theories more continuous. If one always fixed the meaning of a new predicate Q by relying on the experimental methods just available, one would have to revoke the definition of Q at every new stage of the development of science. Relying on the methods of reduction pairs allows for a more flexible attitude with respect to meaning variance; we need not rescind the determinations laid down in the previous stage but can simply supplement them. Carnap's introduction of new terms by reduction pairs instead of explicit definitions may be considered as a first attempt to take into account the open character of the theoretical concepts. We are thus led to an important

"soluble," arguing that the reductive introduction of these concepts cannot be replaced by explicit definitions. As Hempel remarked, reduction pairs do not solve the problem of dispositional terms, which requires a conception of lawlike sentences not provided in "Testability"; see Hempel (1963, 689). Thus one may consider as the most interesting feature of Carnap's account not the treatment of dispositional terms, but rather the advent of what soon was to become the problem of theoretical terms.

distinction between the terms of the language for science: primitive terms with fully determined meaning on the one hand, and terms introduced by reduction pairs, whose meaning was only partially defined by the primitive terms, on the other hand. In *Testability and Meaning* Carnap is not very explicit about the epistemological and methodological relevance of this distinction, but later it will occupy center stage.

From about 1935 Carnap was a firm adherent of Tarskian semantics. Hence it was only natural for him to apply the apparatus of semantics to the task of answering the T-question. For this purpose Tarskian semantics had to be modified to cope with the distinction between elementary (observable) and abstract (theoretical) terms. One may say that Carnap struggled with this problem until the early 1960s, when he thought he finally had found a solution in terms of the Ramsey sentence of a theory (see Carnap 1966, ch. 24). The first step in this direction was to note that we need not give a semantical interpretation for every term, since the physical terms form a system and are interconnected (1939, 204). That is to say, a physically interpreted calculus inherits the holistic character of the system of implicit mathematical definitions. Although already in 1939 one can find a two-language account in Carnap's work, its full-fledged, classical version only appears in "The Methodological Character of Theoretical Concepts" (1956). There the discussion was explicitly couched in the framework of a Tarskian semantics, and problems of partial *interpretation*s of theoretical terms came to the forefront. Moreover, the discussion of their meaningfulness is explicitly relativized to specific theories: a theoretical term may be meaningful with respect to one theory but not meaningful with respect to another (1956, 48, 50).[9] Carnap no longer dealt with the general problematic of the structure of scientific language but concentrates on the T-problem quite explicitly. The principal thesis of the open character of theoretical concepts is maintained and strengthened. Instead of explaining conceptual openness in terms of (multiple) reduction sentences, Carnap now argued that this feature is more adequately represented by the so called C-rules (correspondence rules) that connect the terms of the theoretical vocabulary

[9] This may be a result of the constant criticism of Hempel, who emphasized time and again the necessity to relativize the considerations to specific theories.

with those of the observational vocabulary. The particular form chosen for the C-rules is not essential; Carnap requires only that these rules connect sentences of the observational language LO with certain sentences of the theoretical language LT, "for instance, by making a derivation in the one or the other direction possible. These rules are similar to the 'correlative definitions' of Reichenbach and the operational definitions of Bridgman but may be more general" (Carnap 1956, 48).

We have reached what may be considered as the "received view's" answer to the T-question. If τ is an empirical theory, the vocabulary of τ is divided into the theoretical vocabulary LT and an observational vocabulary LO. Assuming finite axiomatizability, the axioms of τ can be expressed as a conjunction of purely theoretical postulates T and (mixed) correspondence postulates C. Thus a theory τ may be written as

$$T\&C = (\ldots t_1, \ldots, t_k, \ldots) \& (\ldots o_1, \ldots, o_j; \ldots t_1, \ldots, t_k, \ldots)$$

where the first conjunct represents the theoretical postulates T of τ, and the second (mixed) conjunct represents the correspondence postulates C. Against a common interpretation of the received view it must be stressed that the underlying distinction between observational and theoretical concepts was viewed as a pragmatic issue that might have various solutions. As Carnap himself put it: "The line separating observable from nonobservable is highly arbitrary" (1966, 227). The important point was that in the practice of science such a cut was always made to take into account the open character of at least some of the theory's concepts.

Returning to the problem of meaning, and ignoring the technicalities, the criterion of meaningfulness states that a theoretical term t is meaningful relative to T & C if there is a sentence S(t) and a purely observational sentence S(o) such that S(t) & T & C is not logically false and implies S(o). In the simplest case, a meaningful term already occurs in a correspondence rule, but there may be meaningful terms related to the observational level in a more indirect way. Since Carnap had become extremely liberal with respect to the correspondence rules, this meaning criterion is very weak. Nevertheless, it is not vacuous, since it can spot at least some meaningless "metaphysical" terms. Equally important, obviously meaningful terms came out as meaningful. Nevertheless, the criterion did not meet unanimous approval as "the" solution of the meaning problem. Critics spotted various technical difficulties: for instance, the meaningfulness of terms could be altered by innocent, purely linguistic manipulations or insignificant alterations of the theory (Kaplan 1975; Rozeboom 1960). As Kaplan put it: "It appears to be extremely difficult to toe that fine line between the electron and the absolute" (1975, 88). In sum, growing insight into the intricacies of scientific concept formation led the logical empiricists to the conclusion that a fully adequate answer to the T-question was much more difficult than they had thought when they started the endeavor of clarifying the structure of empirical theories with some sort of Hilbert-style axiomatization. It became doubtful if such an answer could ever be found in the realm of purely formal considerations, and it was this doubt that paved the way towards an account that overcame the limitations of a purely syntactic approach.[10]

RAMSEY'S APPROACH

By the measure of the standard histories according to which logical empiricism was discarded in the late sixties/early seventies, the Ramsey approach may be considered as logical empiricism's last stand on the T-question. This assessment is problematic for at least two reasons: first, because that particular answer to the T-question was formulated by Ramsey himself already in 1929,[11] and second, because long after the "death" of logical empiricism, the Ramsey approach to the T-question continues to find prominent advocates (e.g., Papineau 1996; Hintikka 1998) who can hardly be characterized as orthodox logical empiricists.

The Ramsey approach attempts to explicate the meaningfulness of theoretical terms in a semantical framework that strictly distinguishes

[10] A skeptical conclusion of this kind was drawn by Feigl, who declared that the ultimate reason why entities like "entelechies," "souls," and "spirits" are excluded from the realm of respectable scientific entities in contrast to legitimate ones such as atom, magnetic, or field is that the former "do not add anything to the explanatory power of the extant empirical laws and theories" (1950, 218–19).

[11] As Psillos has documented (2000), in the late fifties Carnap literally reinvented Ramsey's approach without realizing it; it was Hempel who enlightened him about this.

between analytic and synthetic truth. Carnap wanted to maintain the important insight that theoretical concepts are open concepts in the sense that they lack a complete interpretation. The content of theoretical terms is too rich to be exhausted by observational consequences. On the other hand, Carnap wanted to draw a sharp line between analytic and synthetic sentences that would enable him to distinguish clearly between pure mathematics and physics that contains mathematics only in applied form. Such a distinction can be drawn given fully interpreted observational statements and meaning postulates (Carnap 1966, ch. 27), but a real difficulty arises for the case of theoretical concepts whose meaning is only partially determined. The problem is to render the openness of theoretical concepts compatible with a strict analytic/synthetic distinction. Here, Ramsey's answer to the T-question comes to the rescue. Slightly modifying the account of the theory structure sketched in the previous section let us characterize an empirical theory as a complex conjunction T & C $(t_1, \ldots, t_n, o_1, \ldots, o_m)$ of theoretical and correspondence postulates. With respect to meaningfulness of T & C, problems are caused by its theoretical terms. Ramsey proposed to solve these problems by simply eliminating all theoretical terms, replacing the sentence T & C $(t_1, \ldots, t_n, o_1, \ldots, o_m)$ by the theory's Ramsey sentence R_T & C defined as

$$R_T \ \& \ C := \exists x_1 \ \ldots \ \exists x_n \ T \ \ \& \ C \ (x_1, \ \ldots, x_n, o_1, \ \ldots, o_m).$$

As is easily proved, the Ramsey sentence R_T & C is true if and only if the complex sentence T & C is true. However, in R_T & C the theoretical terms have disappeared, at least from the surface. In their place are variables. The variable x_i does not refer to any particular class, and the Ramsey sentence asserts only that there are at least some classes that satisfy certain conditions. We no longer need to care about the meaning of the theoretical terms, or so it seems. This has led some philosophers to the conclusion that the Ramsey sentence is an expedient tool to eliminate the bothersome theoretical terms. Actually, things are more complicated: if we consider "being a value of a bound variable" as a necessary and sufficient condition for existence, then the Ramsey sentence R_T & C is clearly an existential claim for the theoretical terms it allegedly eliminates. There is an ongoing debate on the question whether the Ramsey approach is to be interpreted as a realist, an instrumentalist, or a neutral stance towards the existential status of the entities the theoretical terms refer to. It seems Carnap preferred the last interpretation, but he can hardly be said to have maintained a clear and unambiguous position (see Psillos 1999, ch. 3).

The second problem the Ramsey approach was to solve was explication of the analyticity of the theoretical language LT. Given a theory T & C and its Ramsey sentence R_T & C, we may form the conjunction R_T & C & (R_T & C $\rightarrow$ T & C), sometimes called the "Carnap sentence" of the theory (see Lewis 1970). As is easily seen, the Carnap sentence has no factual content. Carnap took it as a sort of generalized meaning postulate of the theory that was to be considered as the analytical part of the theory. On the other hand, the Ramsey sentence may be considered as the synthetic part of the theory since it exactly implies its observational consequences (Hempel 1958, 80). The theory and its Ramsey sentence are functionally equivalent. Even if one were to accept the Ramsey sentence as a solution of Carnap's problem of neatly separating the analytical and the synthetic parts of empirical theories, the discussion about the ontological implications of the Ramsey account of empirical theories still continues. Since the advent of the model-theoretic account of theories the discussion has gained a new impetus, as many of its supporters claim that it represents clear progress over the syntactical account of logical empiricism. We cannot go into the details here, but Hempel was certainly right that Ramsey sentences do not provide a satisfactory way of avoiding theoretical concepts. This has been confirmed by most recent authors on the problem. Moreover, there may be nonformal, pragmatic reasons why theoretical concepts cannot be eliminated.

PRAGMATIC, REALIST, AND INSTRUMENTALIST INTERPRETATIONS

Not all logical empiricists followed Carnap into the logical thicket of possible interpretations of the Ramsey sentence. Some pursued more down-to-earth issues concerning the T-question aiming at what may be called a pragmatic elucidation of the structure of empirical theories that reflected more clearly the essential features of the practice of empirical science. In this group one finds authors such as the later C. G. Hempel, Ernest Nagel, Herbert Feigl, and

Philipp Frank. The general tenor of their contributions is that formal and logical means for modeling the structure of empirical theories are important but should not be overestimated. According to them, informal, in particular historical and pragmatic, considerations played an indispensable role for understanding the structure of empirical theories. Sharing more or less the same basic model, they differed considerably in the details of how they understood the structure of empirical theories. These interpretative differences concerned in particular the assessment of pragmatic, realist, or instrumentalist features of scientific theorizing.

Hempel's general contribution to logical empiricism may be seen in his insistence that logical empiricist philosophy of science must not lose sight of "real" science in favor of what may be called "philosopher's" science. Hempel explicitly criticized the so-called received view that most philosophers and scientists took as the official doctrine of logical empiricism: "I think that it is misleading to view the internal principles of a theory as an uninterpreted calculus and the theoretical terms accordingly as variables, as markers of empty shells into which the juice of empirical content is pumped through the pipelines called correspondence rules" (2001 (1969), 61). To improve this less than optimal state of affairs Hempel reinterpreted the standard conception. Instead of conceptualizing a theory as an abstract calculus C and a set R of rules of correspondence, he conceived a it as composed of a class of "internal principles" I and a class "bridge principles" B. Superficially, the component I corresponds to the uninterpreted calculus, and B to the rules of correspondence. This formal similarity, Hempel is at pains to point out, is misleading. There is a profound difference between the received view and his (I, B)-proposal. The distinction between I and B is not made in terms of the "theoretical" versus the "observational." Rather, the cut is made between the "antecedently known" and the "new theoretical" vocabulary. This cut is relative to the theory in question and largely a matter of pragmatic convenience. This implies, in particular, that "the elements of the pretheoretical vocabulary need not, and indeed should not, generally be conceived as observational terms ... : in many cases the antecedently known vocabulary will contain terms originally introduced by an earlier theory" (Hempel 1969/2001, 52).

Thus, although Hempel's internal principles will typically use a theoretical vocabulary, they cannot be viewed as a totally uninterpreted calculus. An analogous difference exists between the rules of correspondence and Hempel's bridge principles: according to the standard account the correspondence rules assign empirical meaning to the expressions of the calculus, and hence they look like metalinguistic principles which render certain sentences true by terminological convention (Hempel 2001 [1970], 229). For Hempel, this does not correspond to the actual practice of science. Although some scientific statements may be initially introduced by "operational definitions," they usually change their status in response to new empirical findings and theoretical developments and become subject to revision in response to further empirical findings and theoretical developments. Hempel is well aware of the consequences of these moves away from the standard conception: "In fact, it should be explicitly acknowledged now that no precise criterion has been provided for distinguishing internal principles from bridge principles. In particular, the dividing line cannot be characterized syntactically ... for ... both internal principles and bridge principles contain theoretical as well as antecedently available terms" (Hempel 1970/2001, 231). Hence there is no hope to find means by which the "new" theoretical terms are bestowed with meaning by some kind of transfer of meaning from the old "antecedently understood" terms to the new "theoretical" ones through explicit or implicit definitions: "We come to understand new terms, we learn how to use them properly, in many ways besides definition: from instances of their use in particular contexts, from paraphrases that can make no claim to being definitions, and so forth" (1970/2001, 233). This pragmatic description of how we come to terms with new concepts is rather close to Kuhn's approach according to which the usage of a new paradigm is learned by example and apprenticeship rather than by explicit definitions. Theoretical concepts, just like the concept of living organism, are "open-ended" (1970/2001, 233).

Hempel was not the only logical empiricist who felt misgivings with the "standard conception," in particular with its "uninterpreted calculus." Nagel also made proposals to improve the received account. In *The Structure of Science* (Nagel 1961) he attempted to enhance the standard account in two ways. First, he proposed to introduce a third component for the description of the theory's structure that should provide "an interpretation or a model for the abstract calculus, which supplies some flesh for the skeletal structure in terms of more or less familiar conceptual or visualizable materials" (Nagel 1961, 90). His main example of such a "model" was Bohr's planetary model of the atom. It should be noted, however, that Nagel's "model" did not just serve as heurististic means for visualization; rather, the statements induced by the model had a systematic function much like the formulas of the calculus. Through such a model the theoretical concepts of the theory received a fuller interpretation. Sticking to a specific model, Nagel admitted, runs the risk that features of the model may mislead us concerning the actual content of the theory. Actually, the theory may have many distinct models. If one likes, one may consider Nagel's proposal to introduce a model as a major component of a theory's structure, as a forerunner of the so-called semantic view of theories that was to flourish long after the dismissal of logical empiricism.[12]

Nagel's second amendment of the traditional answer of the T-question was his insight that scientific knowledge does not have (at least not up to now) the form of one great unified theory. Rather, it presents itself as a complex network of interrelated theories. Consequently, a natural widening of the T-question is to study the structure of this network constituted by theories and various "intertheoretic" relations. Nagel concentrated on intertheoretic relations that provided reductions of theories (Nagel 1961, ch. 11). According to him reduction is an essentially deductive relation: a primary theory T is reducible to a secondary theory T* if it is possible (1) to provide a common language for the theories T and T* ("condition of connectability"), and (2) to derive T from T* ("condition of derivability"). As the paradigmatic example for such a reduction Nagel considered the relation between thermodynamics and statistical mechanics. Later, "postpositivist" accounts of reduction relations blamed Nagel's proposal for being "too

[12] Indeed, the semantic view characterizes a theory "as comprising two elements: (1) a population of models, and (2) various hypotheses linking those models with systems in the real world" (Giere 1988, 85). This two ingredients are then related to each other by "relations of similarity" in a way that structurally resembles the base diagram of the Logical Empiricists (Giere 1988, 83).

deductive" to serve as a realistic account of actual science, but the emphasis Nagel put on deduction does not mean that he restricted the philosophical analysis of reduction solely to its logico-deductive aspects. If a reduction in his sense is intended to be more than just a logical exercise, the primary (reducing) science has to be "supported by empirical evidence possessing some degree of probative force" (Nagel 1961, 358). This means that history of science has a bearing on the topic of reducibility: "the question whether a given science is reducible to another cannot in the abstract be usefully raised without reference to some particular stage of development of the two disciplines. The question of reducibility can be profitably discussed only if they are made definite by specifying the established content at a given date of the sciences under consideration" (Nagel 1961, 361–2). Nagel's account was intended to elucidate the global logical structure of scientific knowledge without losing sight of its historical context.

Not all logical empiricists showed a pronounced preference for matters logical with respect to the T-question. Feigl's importance resides less in his contributions to the task of elucidating the formal structure of theories and more in the particular interpretation of the formal apparatus he proposed.[13] He was the logical empiricist most eager to reconcile logical empiricism with some sort of realism. In the 1950s he touted himself as an "empirical realist" (1950, 221). He explicitly rejected the standard account according to which the postulates concerning the theoretical terms had a purely instrumentalist reading. According to him, "The system of statements and concepts that constitutes our scientific knowledge is best understood as a network that connects the directly confirmable with the indirectly confirmable" (ibid., 217). He admitted, of course, that theoretical entities may be unobservable but he insisted that they are "indirectly confirmable." This may be considered as a realist variation of the standard theme to elucidate the relation between the two basic components of scientific knowledge. But Feigl also emphasized the following point (1970, 13): "It should be stressed,

[13] Feigl once went so far to trace back the essentials of the Logical Empiricist account of empirical theories to an early (pre-Vienna) paper of Carnap that may well be classified as belonging to his neo-Kantian period (cf. Carnap 1923; Feigl 1970, 3). This stance betrays, to put it mildly, that Feigl did not pay too much attention to the amendments that had taken place since then.

and not merely bashfully admitted, that the rational reconstruction of theories is a highly artificial hindsight operation which has little to do with the work of the creative scientist." In an analogous way as in science, in philosophy of science as well idealizations and simplifications are indispensable. What philosophy of science is after is not to give us empirical theories as they "really are," but models of those theories that may help us to understand the structure and the aims of scientific knowledge.

Feigl's subscription to a realist version of empiricism should not be taken as evidence for a predominance of realist currents in the logical empiricism of the fifties and sixties of the last century. Quite the contrary is true. For instance, the physicist Frank, who had been Einstein's successor at the German University in Prague, stuck to a pragmatic instrumentalism. Having started his philosophical career as a radical conventionalist and instrumentalist inspired by Poincaré and Duhem, later he submerged his conventionalism and adopted an attitude according to which logical empiricism might be understood as a logically refined version of pragmatism:

> In contrast to the method of pragmatism, however, they [the logical empiricists] not only tried to characterize the system of science in a general and somewhat indefinite way by saying that the system is an instrument to be invented and constructed in order to find one's way among experiences, but also – and instead – they investigated the structure and the construction of this instrument. The investigation took place through an analysis of the method by which physics orders experiences through a mathematical system of formulas. (Frank 1949, 105)

In a similar fashion to Carnap in *Abriss*, Frank claimed that the clarification of the internal structure of empirical theories is preeminently guided by practical interests. In contrast to Carnap, however, Frank never engaged in serious work on the formal structure of empirical theories. Instead, he took the logical empiricist model to be valid at least in broad outline and developed an instrumentalist-pragmatic interpretation of it. He considered the "new" logic of Whitehead and Russell as a useful tool to "improve the ideas of Mach and James to a really scientific world conception" (Frank 1949, 105). Hence, the relation between the formal-constructive efforts to answer the T-question by Carnap and those working in a similar style and authors such as Feigl and Frank, who were more engaged in formulating epistemological interpretations, can be characterized as a sort of division of labor: the first concentrated on the more formal aspects leaving space for pragmatic considerations provided by the work of the latter. Although the two accounts are not always easily reconciled, it is remarkable that all considered themselves as working on a common project, namely, of elucidating how we "master reality through systems of hypotheses and axioms," as it was once put in the manifesto.

For reasons of space, we cannot deal with authors like Henry Margenau (1950), or Norman R. Campbell (1953), who developed quite similar accounts of empirical theories, although strictly speaking they cannot be characterized as logical empiricists. Nevertheless, it should have become clear that speaking of *the* logical empiricist view of theories is seriously misleading in so far as such an expression suggests that there was a unique logical empiricist answer to the T-question.

THE DISMISSAL OF LOGICAL EMPIRICISM

Postpositivist philosophers of science such as Feyerabend, Putnam, and others dismissed the logical empiricist answers to the T-questions as totally misleading and wrong-headed from the outset. To bring home their point they found two rhetorical maneuvers extremely useful. First, the plurality of logical empiricist answers to the T-question was reduced to what the postpositivists ominously dubbed the "received" or the "orthodox view." Moreover, the fact that logical empiricism had taken into consideration many pragmatic and historical aspects of scientific theorizing was systematically ignored. A striking evidence for this attitude was the utter neglect of Neurath's encyclopedism by virtually all of the postpositivist critics. Second, the logical empiricists' claims were stated in an overly strong fashion, not considering them as proposals or as models for the elucidation of the structure of empirical theories, but as "dogmas."[14] Thereby, an allegedly unbridgeable gap between the

[14] The tendency to portray Logical Empiricism as an obsolete doctrine centering around certain "dogmas" started with Quine's "Two Dogmas" of 1951. It reached its late and somewhat ridiculous culmination in the early eighties when allegedly "six or seven dogmas of Logical Empiricism" were discovered.

logical empiricist account and its modern ("enlightened") successors was constructed that played down as far as possible many of the existing similarities. Actually, many of the themes of postpositivist philosophy of science are extensions and variations of ideas found already discussed in the logical empiricism, whose variety and intellectual flexibility tends to be grossly underestimated. For instance, the distinction between observational and theoretical terms was never thought to be one that could be drawn in a clear-cut manner once and for all. Logical empiricism was much more liberal and pragmatic than many of its heirs believe. Even many of the seemingly radically novel arguments against a too rigid conception of the structure of empirical theories can already be found in Neurath.

With hindsight, then, the dismissal of logical empiricism as hopelessly obsolete was somewhat hasty, brought about more by interest-guided reconstructions than by solid new arguments. In particular, the complacent attitude of many postpositivist thinkers that postpositivist philosophy of science has moved far ahead of its empiricist ancestors is in need of qualification, to say the least. Carnap, Neurath, Feigl, and others were well aware of the fact that philosophy of science is engaged in making models of scientific theories. Its task is not to explicate what scientific theories "really are." Rather, the models of scientific theories it offers us are more or less adequate, depending on the purposes they are made for. Moreover, since there is no reason to expect that scientific theories of all times and types will always have the same structure, the T-question is unlikely to receive just one answer. Nevertheless, it seems equally unlikely that answers that completely ignore the proposals of logical empiricism will be good ones.