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THEORETICAL TERMS: A NEW PERSPECTIVE[1]

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Philosophical reflection about theoretical terms can be traced back at least to John Stuart Mill. Later on, in logical empiricism, the issue reappeared as a question of whether certain terms were definable in a formal language. Theoretical terms were characterized as nonobservational terms, and observational terms as terms whose denotations could be "directly" perceived with the human senses. Observational terms, in contrast to theoretical terms, were feit to have a clear meaning. Logical empiricists tried to extend this clear meaning to theoretical terms by defining them explicitly. In this, however, they did not succeed, mainly (as will become clear in section III because their logical formalism was too narrow and too strong. Initially the dichotomy between theoretical and nontheoretical terms was drawn "globally," i.e., with respect to the whole language of science. Refinements allowed for a hierarchy of theories in which theoretical terms were to be introduced step by step on the basis of an antecedently available vocabulary.[2] In all variants, however, the observational language was taken to be given and well understood. One of the objections raised against empiricism was that this preferred position of observational language is not justified, and even "wrong".[3] The decline of logical empiricism was, accordingly, accompanied by abandonment of the assumption of a pre-given observational language and concentration on what goes on in "real-life" empirical theories.[4] In line with this development, J. D. Sneed proposed a new criterion

[1]Special features of the subject treated in this paper have been presented in my Habilitation-lecture at the Philosophical Faculty of the University of Munich (May 1983) and in my contribution to the 7th International Congress of Logic, Methodology, and Philosophy of Science in Salzburg (July 1983). I am indebted to Wolfgang Stegmüller and Ulrich Gaehde for illuminating discussions, and to Ch. Pinnock for correcting my English.

[2]See Rudolf Carnap, "The Methodological Character of Theoretical Concepts," in H. Feigl and M. Scriven, eds., *Minnesota Studies in the Philosophy of Science*, vol. I (Minneapolis: Minnesota UP, 1956), pp. 38-76; and Carl G. Hempel, "The Meaning of Theoretical Terms: A Critique of the Standard Empiricist Construal", in P. Suppes, L. Henkin, A. Joja, and G. C. Moisil, eds., *Logic, Methodology, and Philosophy of Science*, vol. IV (Amsterdam: North-Holland, 1974), pp. 367-378.

[3]Norwood Russell Hanson, *Patterns of Discovery* (New York: Cambridge, 1958).

[4]See Hilary Putnam's complaints on philosophical discussions of theoretical terms in his "What Theories Are Not," in E. Nagel, P. Suppes, and A. Tarski, eds., *Logic, Methodology and Philosophy of Science* (Stanford, Calif.: University Press,1962), pp. 240-251; especially p. 243.

of theoreticity[5] which did not rely on a given observational language. According to his criterion, the distinction between theoretical and nontheoretical terms is drawn relative to a given theory. The criterion, however, is formulated on a pragmatic level which leaves much room for vagueness and discussion. There were many reservations, especially among those accustomed to thinking in the tradition of logical empiricism.

In the present paper we take Sneed's criterion as a point of departure, and we try to bring the discussion back to the level of clarity characteristic of logical empiricism. We provide a nonpragmatic explanation for Sneed's "problem of theoretical terms," which shows how this problem arises as a question about meaning rather than as a question of how to distinguish theoretical from nontheoretical terms (section II). Further, we offer a new and precise definition of theoretical terms (section III), which applies to existing theories, reproduces existing distinctions, is in line with the intuitions of logical empiricism, and also throws some clarifying light on Sneed's account. The claim associated with the new approach is that it opens the door for an empirical investigation of more comprehensive parts of the web of empirical theories by establishing which terms in which theories are nontheoretical and, therefore, have to be presupposed as given by other, underlying theories.

I. SNEED'S PROBLEM OF THEORETICAL TERMS

With some simplification Sneed's criterion can be stated as follows. Term $\bar{t}$ of theory T is *T-theoretical*$_1$ iff in any determination of that term, T is presupposed as valid. (The index '1' is used in order to distinguish Sneed's notion from the notion to be treated in section III. Consequently, we will also speak of *theoreticity*$_1$.) Some explanation is necessary here; some of the technical details, however, will be used only in the following sections. We consider a theory (that is, in our context, an *empirical* theory) as constituted (among other things) by a language L, a class of models M, and a set of intended applications I:

$$T = \langle L, M, I, ... \rangle.$$

The language will have to be many-sorted and of higher order in most cases. For reasons of simplicity we assume the nonlogical vocabulary to consist of only finitely many function symbols $\bar{f}_1, ..., \bar{f}_m$; extension to individual constants and "proper" predicates does not create any difficulties. These symbols will be called the *terms* of T, and the i-th term or the i-th function of T will be just $\bar{f}_i$. L determines the class S of *structures* (or interpretations) for L in the usual way.[6] If L is the language of theory T we will also say that structures for L are

[5] *The Logical Structure of Mathematical Physics* (Dordrecht: Reidel, 1971), p. 31.

[6] See Joseph R. Shoenfield, *Mathematical Logic* (Reading, Mass.: Addison-Wesley, 1967), p. 18.

structures for T. If there are, say, k sorts, then each structure for T has the form

$$\langle D_1, ..., D_k; f_1, ..., f_m \rangle$$

where $D_1, ..., D_k$ are sets (of "objects") and $f_1, ..., f_m$ are functions "over" these sets.[7] For each $i \leq m$, f_i is an interpretation or realization of the term $\bar{f}_i$. The dass M of models is a subclass of S, and is usually characterized by a set A of axioms (formulas of L) such that the axioms are valid precisely in all members of M.

Intended applications are "real systems" to which scientists "intend" to apply the theory and which are conceived of in the language of T. An intended application x of T therefore is given by some real system, which, in addition, *gives rise* to a structure for L. We cannot and need not be very precise about what it means to "give rise to" a structure for L; fortunately the present discussion does not crucially depend on this point. A proper treatment would lead us deep into the theory of knowledge. Also, we believe that in the present discussion reference to intended applications can be completely avoided, but only at the price of much technical complication. An example will provide as much intuition as is needed in the present context. If scientists refer to some real system as an application (i.e., an intended application in our sense) of classical particle mechanics, they look at the system as exhibiting position, mass, and force functions. They "see" moving particles, and they assume or believe that constant masses are attached to the particles and that some forces act on the particles. If the same system (a roulette wheel in action, for example) is called an application of probability theory, scientists (eventually the same as before) "see" only possible outcomes, events, and relative frequencies as realized in the system.

In order to understand Sneed's criterion, we have to specify two expressions which are used in the above formulation and also in Sneed's original version, namely 'a determination for some term' and 'to presuppose T as valid in the determination for some term'. It will turn out that each of these expressions requires far-reaching considerations. Therefore, we will say that Sneed's criterion contains two important features: *determination* and *presupposition*.

The intuition forming the basis of this criterion is an intuition about scientific practice, about what scientists actually do and believe. Essentially, this is a matter of pragmatics, not of logic. Roughly, the idea is this. There is given a theory T and a group of scientists working with T. For some reason or other it becomes necessary to know some function-values of certain of T's functions

[7] Compare my "Empirical Claims in Exchange Economics," in W. Stegmüller, W. Balzer, and W. Spohn, eds., *Philosophy of Economics* (Berlin-Heidelberg-New York: Springer-Verlag, 1982), pp. 16-40, especially the appendix, for a simple definition of this "over".
In the following we will denote by $x_i[f]$ the result of replacing f_i in x by f (always on the assumption that f is of appropriate type).

for certain given arguments. This knowledge cannot be acquired by "direct observation", at least not in cases of advanced theories. So some further action is necessary: experiments and measurements will be performed in order to determine the desired values. All these activities, insofar as they result in one or more uniquely identifiable function-values, represent *determinations* for some term. (Of course, all this needs further clarification.)

By looking at concrete examples of such determinations we recognize that, in the course of a determination, scientists in general (i.e., with the exception of a few very basic measurements) use theoretical knowledge, formulas, equations. They perform certain calculations or draw inferences on the basis of given formulas in order to obtain the function-value they want to know. Usually, this theoretical knowledge in a given concrete context is used without further justification: it is (hypothetically) *assumed* or *presupposed*. All kinds of theoretical assumptions used in the course of a determination are "presuppositions." For one "piece of presupposition" (one equation, one formula) two cases are possible: either it stems from ("is part of") some theory T' different from T, or it is part of, or identical with T (i.e., with T's axioms). In order to determine (or "calculate," as they say), for example, the masses of elementary particles, physicists use the relativistic version of the law of conservation of momentum, which is the central part of relativistic collision mechanics; in order to determine the angles of particles' paths after collision they use formulas from (physical) geometry, which is a theory different from collision mechanics.

According to Sneed, this distinction of whether or not the theories presupposed in a determination for some term are all identical with T is the same (by definition) as the distinction between T-theoretical and T-nontheoretical terms. A term $\bar{f}_i$ of T is T-*theoretical*$_1$ iff in all determinations for f the pieces of theory used (presupposed) by scientists for the purpose of determination are part of, or identical with T. In other words, T-theoretical terms are terms that can be determined only by means of[8] (by using) T.

With respect to what will be said in sections II and in III it must be stressed that this criterion of theoreticity$_1$ is pragmatic. That term $\bar{f}_i$ is T-theoretical is a statement about how scientists holding T proceed, if they want to determine $\bar{f}_i$. The criterion distinguishes theoretical terms by how scientists act. In contrast to this, the criterion – or rather definition – of theoreticity to be presented in section III) will be purely logical.

From this criterion of theoreticity and from the hypothesis that some of T's terms are T-theoretical$_1$, Sneed deduced what he called the *problem of theoretical*$_1$ *terms*. Again we will discuss only a simplified version. The problem consists of a kind of circularity with respect to the testing of a theory by

[8]Some authors have attempted to clarify the issue by means of further explication. See Stegmüller, op. cit.; Andreas Kamlah, "An Improved Definition of 'Theoretical in a Given Theory'," Erkenntnis, X, 3 (October 1976): 349-359; and W. Balzer and Carlos Ulisses Moulines, "On Theoreticity," Synthese, XLIV, 3 (July 1980): 467-494.

means of measurement. For if we want to test T by means of measurement we have to determine (among other things) at least some values of T's theoretical functions. But this, by virtue of the criterion, is possible only if we presuppose T as already valid. So, in order to te T by means of measurement, we have to presuppose that Talready is valid – which is just what we want to find out by means of testing. Still more briefly: in order to find out whether T is valid we have to presuppose that T is valid. If we accept a proposal of Wolfgang Stegmüller's,[9] namely to explicate 'to presuppose' by "to imply logically" we arrive at the following: to test the validity of T by means of measurement logically implies that T is valid. This of course seems hardly compatible with a certain view about testing, namely that testing a theory should be an enterprise independent of and certainly not presupposing the theory to be tested. On the other hand, the present formulation does not reveal any strict logical circulatory, and it is doubtful whether by reformulation such a circle can be constructed. This may be the reason why the problem of theoretical$_1$ terms has not been accepted as a real problem by many philosophers of science, especially by those who work with logical tools.

If we do not bother about details, the present discussion may be summarized as follows. Term $\bar{f}_i$ is T-theoretical$_1$ if T is presupposed (used) in any determination for $\bar{f}_i$. The problem of theoretical$_1$ terms consists of the existence of T-theoretical$_1$ terms $\bar{f}_i$. For if T is presupposed in any determination for $\bar{f}_i$, how can we test T (on the assumption that this is possible only on the basis of some concretely determined values of T's functions including $\bar{f}_i$)? The characterization and the resulting problem of theoretical$_1$ terms are only slight variants of each other; one might say that the problem of theoretical$_1$ terms is just a slight reformulation of the claim that a theory contains T-theoretical$_1$ terms. The heart of the matter is given by the observation or claim that scientists for some$\bar{f}_i$ in fact presuppose T during all determinations for $\bar{f}_i$ ($\bar{f}_i$ being T-theoretical$_1$).

The question whether this is so for some function $\bar{f}_i$ of an existing theory (e.g., the mass function of classical mechanics) has been actively discussed, but without definite results. The main reason for this may be seen in the strongly pragmatic character of the original formulation.[10]

[9]See *The Structure and Dynamics of Theories* (New York: Springer-Verlag, 1976).

[10]It may be noted that on Sneed's original account the notion of a theory contained the distinction between theoretical and nontheoretical terms as a constitutive element. But by accepting such a distinction we are not forced to accept his criterion of theoreticity, too. In principle, his theory concept might go together with different criteria of theoreticity. The two items are separated in the present frame because we do not even insist that a theory in general should contain a distinction between theoretical and nontheoretical terms. It should also be noted that we have included language as an explicit element of a theory, though the concept described above still represents a kind of "nonstatement" view of theories. We do not attach much importance, however, to the distinction – sometimes discussed recently – between "statement" and "nonstatement" views.

II. AN EXPLANATION OF THE PROBLEM OF THEORETICAL TERMS

We will now "derive" (and thereby explain) the problem of theoretical$_1$ terms from an assumption which goes beyond the mere observation of scientific practice and which can be regarded as a special instance of a philosophical position concerning meaning and reference. Besides being of interest on its own, this explanation will make clear that the *problem* of theoretical$_1$ terms arises from a particular view about the meaning of a term in a theory (and thus in fact is a problem of meaning) and that it is not really connected to the distinction between theoretical and nontheoretical terms. In order to state our explanation, some preliminary, clarifying considerations are necessary.

First we have to clarify what we mean by measurement. In general, measurement aims at finding the function-value of some function f for a given argument b. This value, $f(b)$, results from a process during which measurement takes place. We call this a *process of measurement.* Processes of measurement consist of real systems which change over time (zero change as a special case is included). Often, the process is initiated by an experimenter; it may consist of what is going on in a certain experimental setup. It also may be given by what is happening in some measuring instrument. The important point here is that the system realized during a process of measurement in fact is a *system* that may be conceptualized and properly differentiated from its "environment". If we assume that only some objects and functions are realized in such a System, it may be conceptualized in the form $x = \langle D_1, ..., D_k; f_1, ..., f_m \rangle$ already introduced. The function f the value of which at b is to be measured, then will be among $f_1, ..., f_m$, say $f = f_i$, and b will be in the domain of f_i: $b \in Dom(f_i)$. b may be some object proper or may be constructed out of objects and (or) numbers. In mechanics, for instance, the mass-function m takes particles as arguments, in geometry the distance function takes pairs of points of space, and in mechanics, again, the force function takes particles, real numbers, and integers;[11] so the respective function-values have the following forms: $m(p)$, $d(\langle b, c \rangle)$, $f(p, t, i)$. The function-value $f_i(b)$ will be called the *measured value.* In the following, the i-th function) occurring in x will be denoted by f_i^x and, consequently, a measured value (measured in x) by $f_i^x(b)$.

As long as we are concentrating on the measurement of one single function-value, we may assume that the process of measurement by means of which this value is obtained is *governed* by some theory T in the sense that the system realized during that process is an intended application of T. That is, all the functions of T (more accurately: all function-symbols of T) have interpretations in that system, and scientists intend to apply T to that system. This assumption seems rather restrictive at first sight; it may be objected that in cases

[11] At least in the formulation of J. C. C. McKinsey, A. C. Sugar, and P. Suppes, "Axiomatic Foundations of Classical Particle Mechanics," *Journal of Rational Mechanics and Analysis*, II (1953): 253-272.

of fundamental measurement as well as in cases where complicated apparatus involving different theories is used our assumption will not be satisfied. But in cases of fundamental measurement T will just be some theory of extensive systems (or the like), and in cases of complicated procedures involving several theories the procedure can be split up – at least conceptually – into a "chain of measurements"[12] so that each link of the chain will be governed by only one theory. In the latter case we may restrict our analysis to one of the chain's links without changing the situation.

Each process of measurement thus constitutes a structure for some theory, which moreover is an intended application. Such a structure x will be called a measuring model or a *measuring model for* $f_i^x(b)$. The general condition characterizing measuring model x for $f_i^x(b)$ is that $f_i^x(b)$ be uniquely determined by the other functions of x and by some lawlike connection between those and f_i^x. Note that this kind of uniqueness is different from the unique dependency of $f_i^x(b)$ on b. The latter is expressed by the formula

$$\forall b, c \in Dom(f_i^x)(b = c \rightarrow f_i^x(b) = f_i^x(c))$$

whereas the former condition of uniqueness reads like this (see fn 6 above for notation):

$$\forall f, f'(\sigma(x_i[f]) \wedge \sigma(x_i[f']) \rightarrow \forall b(f(b) = f'(b)))$$

where σ represents the lawlike connection among the different functions of T.

In a second step we have to think about what it is that is to be measured in a measuring model. Of course, if x is a measuring model for $f_i^x(b)$ then $f_i^x(b)$ is measured by means of x. But this is not an interesting answer to our question. Usually, if a process of measurement is started, we have certain ideas about *what* we want to measure even before the process has given a result. Measurement makes sense only if we can identify the entity to be measured independently of the result of measurement. This is indicated already in the grammar of 'to measure'. We always say that we measure *something*, and in more specific contexts this "something" usually will have a name. In physics, for instance, we measure mass or weight or force; in chemistry we measure atomic weights or valences; in economics we measure prices or quantities. But we never "just measure." The problem is to identify the entity we want to measure.

The identification of what it is that we want to measure straight-forwardly involves philosophical reflection, because it requires us to take a definite point of view about how theory and evidence are related to each other. We know the different accounts that have been given of this relation, ranging from the operationalist view that what we want to measure by means of measuring model x in fact is *nothing but* $f_i^x(b)$, to the radical holistic or coherentist view that what we want to measure can be identified only by the *totality* of our

[12]See my "Theory and Measurement," Erkenntnis, XIX, 1 (May 1983): 3-25, p. 15.

knowledge, at the other end of a spectrum of possible alternatives. For the purposes of this paper it is *not* necessary to take a particular position here. The crucial point is that what we want to measure by means of measuring model x has to be somehow identified independently of the concrete process of measurement represented by x, and that there are different alternative views about how the identification proceeds (or should proceed).

For our purposes it is sufficient to introduce a new label denoting the entity to be measured *in general*, i.e., regardless of the particular philosophical point of view one may take. Let us denote the entity to be measured in a measuring model for $f_i^x(b)$ by $\hat{f}_i(b)$. The index 'i' here does not indicate any position of f_i in some structure; it only indicates that $\hat{f}_i$ is of the same type as f_i^x. Identity of the types of $\hat{f}_i$ and f_i^x seems to be a minimal requirement which will be part of any identification of $\hat{f}_i$. In concrete cases the function $\hat{f}_i$ always will have a name, e.g., 'mass,' 'weight,' 'force,' 'atomic weight,' 'valence,' 'quantity,' etc. It is not necessary, however, to introduce a symbol $\bar{f}_i$ for such names, because we do not need to talk here about the representation of f_i in the language.

The problem of how to identify the function one wants to measure by means of measuring model x, i.e., the problem of how to identify $\hat{f}_i$, in its present formulation seems to have received little attention. In another formulation – equivalent to the present one – the problem has been at the center of discussions for the last hundred years. In this other formulation the problem is whether operationalism or coherentism or some intermediate position represents the *correct* account of scientific practice. In the present context of measurement and in the conceptual frame here developed we will speak of the *problem of meaningful measurement*. In the terminology developed above it may be restated as follows. Suppose we want to measure $\hat{f}_i$ for some given argument b; i.e., we want to determine the value $\hat{f}_i(b)$. Some process of measurement will be initiated such that b occurs in the system corresponding to that process. This system will be governed by some theory T and will give rise to a measuring model $x = \langle D_1^*, ..., D_k^*; f_1^*, ..., f_m^* \rangle$. But what is obtained (measured) by means of x is $f_i^x(b)$ and not $\hat{f}_i(b)$. The problem of meaningful measurement then consists of specifying a set of conditions under which $f_i^x(b)$ can be identified with $\hat{f}_i(b)$. The specification of such conditions will provide a (partial) identification of $\hat{f}_i$.

We can now see that "measuring $\hat{f}_i$" means applying some process of measurement that results in some value and then "some how" passing over from $f_i^x(b)$ to $\hat{f}_i(b)$. In order to get a clear picture, we have to spell out how precisely this transition from $f_i^x(b)$ to $\hat{f}_i(b)$ works or is justified; that is, we have to provide a solution of the problem of meaningful measurement. There are two "classical" solutions: the operationalist and the coherentist. In a third preliminary step we have to describe the coherentist solution of the problem of meaningful measurement, and this may best be done by indicating also the operationalist alternative.

The operationalist solution is simple. The operationalist defines $\hat{f}$ by the measured values that occur in various distinguished processes of measurement. So $\hat{f}_i(b) = f_i^x(b)$ by definition of $\hat{f}_i$. A bit more precisely, $\hat{f}_i$ is *operationally defined* in the following way. There is specified some *method of measurement* B which in our frame we can conceive of as just consisting of a class of measuring models (those models which result from applying the specified "method"). Then $\hat{f}_i$ is defined as the union of all i-th functions f_i^x occurring in measuring models x of B: $\hat{f}_i = \bigcup\{f_i^x / x \in B\}$. This is not the place to discuss the merits and shortcomings of operationalism. If there is reason to believe that different measuring models of B will yield identical values for objects they have in common, i.e., if, for all $x, y \in B$ and all $b \in Dom(f_i^x) \cap Dom(f_i^y)$, we have $f^x i(b) = f_i^y(b)$, then the above definition of f_i is one possible solution of the problem of meaningful measurement. This solution yields an "operationalist way of measuring $\hat{f}_i$" in the following way. We choose some measuring model x such that x is a member of the class B that defines $\hat{f}_i$ and such that the argument b at which $\hat{f}_i$ is to be determined is in the domain f_i^x; we then determine $f_i^x(b)$ by means of x. Since, by definition of $\hat{f}_i$, $\hat{f}_i(b) = f_i^x(b)$, we have determined $\hat{f}_i(b)$ in this way.

The coherentist solution of the problem of meaningful measurement is a bit more complicated because of the fact that $\hat{f}_i$ here has a more independent Status. We restrict ourselves to the case in which "coherentism" comprises only one theory T. The more general and more radically holistic case perhaps could be treated in the same way. The problem in the general case is that we do not have (yet) a clear picture of the over-all structure of science which we could substitute for our present "theory T." Roughly, the coherentist solution proceeds in two steps. First, $\hat{f}_i$ is defined by means of T, and, second, an assumption is made from which, in the situation of measurement with the help of a measuring model x, it follows that $f_i^x(b) = \hat{f}_i(b)$.

First, the definition of $\hat{f}_i$ refers to "all of" T; this is in contrast to the operationalist definition of $\hat{f}_i$ which refers only to some method B of measurement. According to the coherentist account, $\hat{f}_i$ is defined as the union of all functions f_i occurring in models of T which also are intended applications:

$$\hat{f}_i = \bigcup\{f_i^x / x \in M \cap I\}.$$

We will assume in the following that $\hat{f}_i$ so defined again is a function, although this does not follow from the definition. Analysis of existing theories shows that this assumption (in the present or in some other equivalent form) is often used implicitly, and may be regarded as an essential feature of empirical theories.[13] It should be noted that, even though $\hat{f}_i$ is explicitly defined in terms of (the

[13]In Sneed's work this assumption acquires the status of a component of empirical theories in general. There, it is given by a special kind of Sneedian "constraints," the so-called "identity constraints."

constituents of) T, this does not imply that $\hat{f}_i$ can be formally characterized, because the set I of intended applications appearing in the *definiens* of $\hat{f}_i$ can be fixed only pragmatically.

Second, given this definition of $\hat{f}_i$ and given some process of measurement captured by measuring model x for $f_i^x(b)$, how can we get from $f_i^x(b)$ to $\hat{f}_i(b)$? The coherentist answer is: by *presupposing* T during the process of measurement. That is, it is presupposed that the axioms of T are valid in the system that is realized during the process of measurement. In other words: it is presupposed that the measuring model given by the process of measurement is a proper model of T. In other words: if this measuring model is denoted by x then to presuppose T during the process of measurement giving rise to x just means that $x \in M$. It is not difficult to see that presupposing T during the process of measurement (i.e., presupposing that $x \in M$) in fact implies that $f_i^x(b) = \hat{f}_i(b)$. For if x is a measurement model then x is an intended application of T ($x \in I$) by our stipulations on measurement models. This, together with $x \in M$, implies that $f_i^x \in \{f_i^y / y \in M \cap I\}$, and from our assumptions that $\bigcup\{f_i^y / y \in M \cap I\}$ is a function and by definition of $\hat{f}_i$, we finally obtain $f_i^x(b) = \hat{f}_i(b)$ for any $b \in Dom(f_i^x)$.

Accordingly, the coherentist way of measuring $\hat{f}_i$ is this. Choose some measuring model x so that b occurs in the domain of f_i^x and determine $f_i^x(b)$. Presuppose T as valid during this process (i.e., presuppose that $x \in M$). From this and the general assumption that $\hat{f}_i$ is a function, it follows, as was just shown, that $f_i^x(b) = \hat{f}_i(b)$, and so we have obtained the desired value $\hat{f}_i(b)$.[14] In this situation, we will say that $\hat{f}_i$ *is measured in x in the coherentist way.*

It seems helpful here to reflect on why we have chosen the label ‘coherentist’ for this second solution. Roughly, coherentism is a view about meaning and truth. Words acquire their meaning through their role and position in a whole language, and truth, correspondingly, also has to be understood relative to the total language. As a consequence truth cannot be checked on the basis of some unshakable observation sentences; it can be checked only from “inside”, by coherence of the whole system. The coherentist way of measuring $\hat{f}_i$ is just a very special case of this general point of view. In order to find out whether a theory T is “true” we cannot rely on results of measurements that are independent of T, because, according to the coherentist account of measuring $\hat{f}_i$, every measurement of $\hat{f}_i$ already presupposes T. But if no measurements independent of T are possible, then such measurements independent of T (“basic sentences”) are not available as a basis for testing T. Only the coherentist, internal way of assessing T’s truth remains open. The holistic view that the meaning of a term depends on large portions of science is not new, of course.

[14] Formally, the assumption that $\hat{f}_i$ is a function might be weakened or even dropped and replaced by some complicated or conventional “definition” of $\hat{f}_i$. However, there is no evidence from existing theories that would back such a move.

Even with respect to physical theories it had been advanced already by Norman R. Campbell.[15]

We now are in a position to state the argument that explains the problem of theoretical$_1$ terms as a problem about meaning. The argument has three premises:

(P_1) For any measurement that is performed in order to test theory T it is necessary that some function of T be measured during the process of measurement.

(P_2) There are different ways of measuring the functions of T, among them the operationalist and the coherentist way.

(P_3) The coherentist way of measuring the functions of T is the correct way.

From (P_1) – (P_3) it is concluded that

(C) For any measurement that is performed in order to test theory T it is necessary to presuppose T as valid during the process of measurement.

With a few further steps it can be seen that (C), in fact, follows from (P_1) – (P_3). First, in (P_1) we have to substitute something more accurate for the phrase 'some function of T is measured.' We may replace this by 'there is some $i \leq m$ such that $\hat{f}_i$, is measured.' By (P_2) and (P_3) any noncoherentist way of measuring $\hat{f}_i$ is excluded; so in (P_1) we have to substitute 'there is some $i \leq m$ such that $\hat{f}_i$ is measured in the coherentist way'. (P_1) then becomes (P'_1): For any measurement which is performed in order to test theory T and which is represented by a measuring model x, there is some $i \leq m$ such that $\hat{f}_i$ is measured in x in the coherentist way. But, by our previous explication of 'to measure $\hat{f}_i$ in x in the coherentist way,' this implies that the measuring model according to the coherentist account of measuring $\hat{f}_i$ is a proper model of T, and this in turn means that T has been presupposed as valid during the process of measurement. By putting all this together we obtain, in fact, (C) from (P_1) – (P_3).[16]

If we think about the status of premises (P_1) – (P_3) it seems that (P_1) and (P_2) have the character of "analytic" sentences serving to fix the meaning of 'test of a theory' and 'measurement,' whereas (P_3) is clearly empirical in character.[17] (P_3) is an empirical statement about how scientists actually

[15]See *Foundations of Science*, unaltered republication of the first edition of the work formerly published under the title *Physics: The Elements* (New York: Dover publications, 1957), chap. II, especially pp. 42/3 and 49/50. In spite of such similarities with respect to approaches to meaning, there is no proposal to be found in the literature of a *criterion* of theoreticity similar to that put forward by Sneed (as far as I know).

[16]With some trivial intermediate explicative steps the argument can be transformed into a proper logical derivation. However, no further clarity is achieved thereby.

[17]We accept the thesis that there is no sharp distinction between analytic and synthetic sentences, and this applies to the distinction concerning the status of (P_1) – (P_3), too. On the other hand it should be noted that such acceptance does not imply any rejection of the

measure if there is already some theory governing the process of measurement. (P_3) might be false, and, for instance, the operationalist way of measuring $\hat{f}_i$ might be correct. The point here, however, is not to decide between (P_3) and its possible alternatives. The point is to show that (P_3) implies (C), given that (P_1 and (P_2) are not questioned. That is, the problem of theoretical$_1$ terms is essentially a consequence of the coherentist way of measuring the functions of a theory. In other words, it follows from scientists' beliefs (as expressed in scientific practice) that theories – if at hand – should be taken seriously in the context of measurement.

To summarize this section, we may say that the problem of theoretical$_1$ terms is a special case of or follows from a general coherentist view of meaning and truth. It arises because scientists seem to check the truth of theories by means of measurements in which those theories are already presupposed, i.e., in a coherentist way. The most interesting feature of our explanation, however, is that it applies to *all* terms of a theory: the distinction between theoretical and nontheoretical terms was used neither in the above argument nor in the subsequent explications. Since the criterion of theoreticity$_1$ is only a slight reformulation of the problem of theoretical$_1$ terms, our explanation casts some doubt on whether this criterion really provides an adequate distinction between theoretical and nontheoretical terms. With these doubts in mind we now turn to a new criterion – or better – a new definition of theoreticity which was found in the course of research aiming at clarification of Sneed's account.

III. A NEW DEFINITION OF THEORETICAL TERMS

Sneed's criterion of theoreticity is based on two items: presupposition and determination. As we just saw, the problems of presupposition essentially arise from a particular theory of meaning, and it is not clear how this feature contributes to the distinction among the terms of a theory. So "determination" seemed to be a more promising area. In fact, Ulrich Gaehde[18] was able to draw a formal distinction by characterizing "T-admissible determinations" as those which are compatible with T's invariances, and this distinction fitted with previous intuitions about theoretical terms. We will present here a modified version of Gaehde's definition which is slightly "weaker" and much more simple. In order to stress the formal character of the definition we choose to speak about "definability" instead of "measurement" or "determination."[19] Roughly, then, and in the conceptual frame outlined in section I, the new definition of theoretical

use of precise (and even formal) concepts in stating theoretical as well as metatheoretical ideas.

[18] *T-Theoretizität und Holismus* (Frankfurt/Main-Bern: Peter Lang Verlag, 1983). In English, see also his "Formal Conditions of Theoretical Terms," forthcoming.

[19] Compare my "Theory and Measurement," *op. cit.*, for a similar definition in terms of measurement.

terms can be stated as follows. Term $\bar{f}_i$ of theory T is *T-theoretical$_2$* iff $\bar{f}_i$ is weakly and invariantly definable in T.

For further explanation let us start by noting that in first-order theories explicit definability of $\bar{f}_i$ in T is equivalent to the semantic requirement that, in each model x, f_i^x be uniquely determined by the other functions of x (and by x being a model):

$$\forall x \in M \forall f, f'(x_i[f] \in M \wedge x_i[f'] \in M \rightarrow f = f')$$

Weak definability is obtained from this if we replace the class of models M by *some* subclass $B \subseteq M$ (representing some "subtheory" T' of T) in the above formula:

$$\exists B \subseteq M \forall x \in B \forall f, f'(x_i[f] \in B \wedge x_i[f'] \in B \rightarrow f = f')$$

In "real-life" theories B is given by so-called "special laws" like Hooke's law in mechanics, the ideal-gas law in thermodynamics, or the law of diminishing returns in economics. But weak definability as expressed by the last formula is not interesting because it is trivial: take B to be a singleton. Intuitively, what is missing in the formula is of course a characterization of B being a *law*. Fortunately, there is a way out of this problem which does not depend on an explication of lawlikeness. In the present context it is sufficient to restrict the class of "T-admissible subtheories" B by requiring that B have the same degree of invariance for $\bar{f}_i$ as M. In order to explicate the last expression, let us look very formally at possible variations of f_i^x in some model x. In general, among those functions f which we can substitute in x for f_i^x there will be functions f such that $x_i[f]$ is not a model of T but only a structure for T, and there also will be functions f' such that $x_i[f']$ is a model of T. The class of all those f' for which $x_i[f']$ again is a model of T we call the *range of invariance* of $\bar{f}_i$ in x with respect to M, denoted by $RI(M, i, x)$:

$$RI(M, i, x) = \{f / x_i[f] \in M\}$$

This definition makes sense and may be used also for subsets B of M. In concrete theories $RI(M, i, x)$ can be exhausted by some class τ_x of transformations in the sense that precisely all members f of $RI(M, i, x)$ can be obtained from f_i^x by some transformation $\theta \in \tau_x$: $f = \theta(f_i^x)$. It is then said that theory T is invariant under transformations of the form given by τ_x, and this is why we speak of the "range of invariance." The range of invariance of the position function in mechanics, for instance, is described by Galilean transformations in this sense.

We say that a subclass B of M has the *same degree of invariance for $\bar{f}_i$ as T* iff, for all $x \in B$, the range of invariance of $\bar{f}_i$ in x with respect to B is at least "as great as" (i.e., contains) the range of invariance of $\bar{f}_i$ in x with respect to M: $RI(M, i, x) \subseteq RI(B, i, x)$. Because of $B \subseteq M$ this implies that the two ranges are equal. Intuitively, if B has the same degree of invariance for as T, then in the axioms characterizing B the connection of $\bar{f}_i$, with the other functions is not "stronger" than it is in the axioms for M. In first-order theories, this means that the axioms for B express further requirements only for the function-symbols different from $\bar{f}_i$. Still differently: B represents some special law for T which is invariant under the same transformations (of $\bar{f}_i$) as T. By putting together the two requirements we obtain the following, more explicit definition of theoreticity$_2$:

Term $\bar{f}_i$ is *T-theoretical$_2$* iff there is some $B \subseteq M$ such that
(i) $\forall x \in B \forall f, f(x_i[f] \in B \wedge x_i[f'] \in B \rightarrow f = f')$
(ii) $\forall x \in B(RI(M,i,x) \subseteq RIB, i, x))$.

In the verbal formulation we speak of "definability" because of requirement (i). In some suitable subclass of models ("subtheory"), i.e., in some special cases, $\bar{f}_i$ can be uniquely determined, and in this subtheory, $\bar{f}_i$ therefore is definable. The requirement on degrees of invariance narrows down the class of admissible "subtheories" which may be used in order to "define" $\bar{f}_i$. Without the latter requirement each term of T would be T-theoretical$_2$.

In quantitative theories the transformations describing $RI(M,i,x)$ usually will be composed of dilations and (eventually) other transformations. If this is so then requirements (i) and (ii) can never be jointly satisfied, because if B has the same degree of invariance as T then $\bar{f}_i$ is determined in B at most up to transformations of scale. In order to make room for quantitative theoretical terms occurring in theories with invariances of scale it therefore is necessary to weaken the uniqueness condition (i). Equality has to be replaced by "equivalence of scale"; i.e., '$=$' by '$\sim$' where '$f \sim f'$' means that f' can be obtained from f by some transformation of scale, i.e., some transformation of the form $f'(\alpha) = \beta \cdot f(\alpha) + \gamma$.[20] If this qualification is taken into account, then theoreticity$_2$ reproduces the informal distinction between theoretical and nontheoretical terms as drawn in the literature by philosophers of science. In classical mechanics, mass m and the component forces f_i are mechanics-theoretical$_2$; in classical collision mechanics mass m is theoretical$_2$; and in exchange economics equilibrium prices and equilibrium distributions are theoretical$_2$ – all other functions being nontheoretical$_2$, respectively.[21] According to our present approach, in contrast to earlier considerations of such questions, these results are now *provable*, once the axiomatization is given. The difficult part of such proofs is to show that some term is T-nontheoretical$_2$, for this amounts to showing that there is no invariant subtheory in which the term can be defined.

[20] Of course, here f and f' must take their values in spaces in which multiplication and addition make sense. But this is just another way of saying that $\bar{f}_i$ is a quantity. Compare *ibid.* for a more explicit treatment of this feature.

[21] See my "On a New Definition of Theoreticity", *Dialectica*, XXXIV, 2 (1985): 127-145, for axiomatizations of these theories and for formal proofs about theoreticity.

This remark reveals the close connection that theoreticity$_2$ has to the original account of logical empiricism. The T-nontheoretical$_2$ terms are part of the observation language (if we use this term in the more sophisticated version as referring to the antecedently available vocabulary), their meaning has to be already established if we want to use them in the context of T. For T offers no means to determine them even in very special cases; i.e., T does not contribute to fixing their meaning. T-theoretical$_2$ terms, on the other hand, are those terms introduced by means of T which become meaningful through their role or position in T. The present definition also shows why logical empiricists (as well as their successors) did not succeed in defining theoretical terms. The notion of a definition as explicated in terms of first-order logic simply is too strong to give an adequate picture of how new terms are introduced in the frame of comprehensive empirical theories. "Theoretical" terms are not definable in the sense of first-order definability; such definition would deprive them of any importance. But they are "definable" in a weaker sense, namely, weakly and invariantly, and by this their meaning is determined at least as far as is necessary for us to use them in connection with the theory "they come from."

The status of T-nontheoretical$_2$ terms as being not determinable at all "in T" is also relevant for a deeper understanding of theoreticity$_1$. The explanation for the problem of theoretical, terms given in section n does not depend on whether the term under consideration is T-theoretical$_1$ or not. The problem of theoretical$_1$ terms arises from the coherentist way of measuring the functions of T, and it arises for T-nontheoretical$_1$ terms in the same way. So, in connection with this explanation, a natural question to ask is why, then, there can be T-nontheoretical$_1$ terms at all. For if T is presupposed in all determinations for all terms, then *all* terms of T should be T-theoretical$_1$. But in the light of theoreticity$_2$ we can now see why T-nontheoretical$_1$ terms may indeed occur. In determinations of T-nontheoretical$_2$ terms it makes no difiference whether T is presupposed or not. For if a term is T-nontheoretical$_2$, then, according to the above definitions, its determination by means of T is *impossible* (there is no subtheory B in which the term can be "defined"); so, even if T is presupposed during the course of measurement (T-theoreticity$_1$) this does not help, does not contribute to finding the measured value. Therefore, on the pragmatic level of what scientists do, it will be difficult to detect evidence for their actually presupposing T in determinations of T-nontheoretical$_2$ terms. In other words, T-nontheoretical$_2$ terms to the determination of which T cannot contribute are likely to function as T-nontheoretical$_1$ terms on the pragmatic level. In this sense we may say that the definition of theoretical$_2$ terms in the light of what was said in section II above explains why there are T-nontheoretical$_1$ terms at all (at least in some theories).

Finally, we want to stress the importance of the new definition for the philosophy of science. Up to now no precise and "workable" distinction between theoretical and nontheoretical terms was available.[22] But only if it is available will it become possible to investigate more global structures of science. More precisely, with the new definition of theoreticity$_2$ we obtain a precise and workable relation of "presupposition" among theories. Theory T *presupposes* theory T' if some T-nontheoretical$_2$ term is T'-theoretical$_2$. The idea here is that T' gives some meaning to (some way of determination for) the T-nontheoretical$_2$ term which cannot be provided by T. On the basis of the new defmition the structure of this presupposition relation can be investigated empirically, i.e., by determining the relation in the domain of empirical theories. By this possibility the holist's claim that science (and knowledge) forms one (or at least only a few rather big) inseparable unit(s) gets a new dimension of precision and empirical decidability.

One of this journal' s editors noted – quite correctly – that T-nontheoretical$_2$ terms may fall into two categories, namely (a) those whose determination is via some other theory T^*, and (b) those for which we do not have such a T^*. In fact, there may be T-nontheoretical$_2$ terms that are not determined by any other theory T^*. The existence of such terms raises philosophical questions about how science is anchored in human practice and in natural language. These questions are beyond the scope of the present paper, but the fact that our distinction leads into such deep philosophical water certainly does not count against our approach.

CONCLUSION

Given two criteria or definitions of theoreticity which obviously nei- ther are identical nor lead to identical results, it is natural to ask which is to be preferred. This, however, turns out to be difficult, because the two definitions are not really compatible. Let us first consider some aspects of detail.

First, theoreticity$_2$ has little to do with "presupposition", which was the central feature of theoreticity$_1$. Whether T is or is not presupposed during measurements for $\bar{f}_i$ is irrelevant to whether $\bar{f}_i$ is or is not T-theoretical$_2$. In order to apply the new definition, no problem of meaningful measurement needs to be solved; the question of identifying function $\hat{f}_i$ does not arise. This, of course, does not mean that the latter problem and question are not interesting in other respects; it only means that they can be separated from the question of theoreticity$_2$. In view of the two aspects to be found in theoreticity$_1$ – presupposition and determination – we can say that theoreticity$_2$ is a more economical concept because it involves the second aspect only.

Second, as concerns the aspect of determination, theoreticity$_2$ gives a much

[22]Formal, philosophically minded proposals, like, e.g., David Lewis's "How to Define Theoretical Terms" have never been applied to real empirical theories, and for good reasons; for Lewis's paper see this JOURNAL, LXVII, 13 (July 9, 1970): 427-446.

more detailed picture than theoreticity$_1$. Even on a very informal level theoreticity$_2$ stresses the role of special laws and of invariances. This, together with the conceptual apparatus of measuring models, represents a good deal of refinement over theoreticity$_1$.

Third, theoreticity$_2$ does not imply any problem of theoretical terms. This is due to an exchange of quantifiers in the two definitions. Whereas the definition of theoreticity$_1$ in section II starts with an universal quantifier over "determinations" (or "measurement"), the definition of theoreticity$_2$ starts with an existential quantifier 'there is some $B \subseteq M$...' Intuitively, the problem of theoretical$_1$ terms arises precisely because of the universal form of quantification. If only the existence of some possibility of determination (some B) is required, then there is room for determinations that eventually do not presuppose[23] theory T – even if the term to be determined is T-theoretical$_2$. In the light of the discussion of section II this result is not very surprising. If the problem of theoretical$_1$ terms essentially follows from the coherentist way of measuring $\hat{f}_i$, i.e., from the aspect of presupposition in theoreticity$_1$, and if theoreticity$_2$ does not contain any such aspect, then theoreticity$_2$ will not yield such a problem.

Fourth, theoreticity$_2$ can be made formally precise, provided we are willing to talk about theories of the form outlined above. In this respect theoreticity$_2$ is clearly superior to theoreticity$_1$, which – as already stressed – is largely a pragmatic affair.

Fifth, theoreticity$_2$ provides an explanation for the existence of T-nontheoretical$_1$ terms. That such terms exist is not obvious, at least if we look at theoreticity$_1$ from the more general point of view developed in section II.

These five isolated items all favor theoreticity$_2$, but they are not sufficient to bridge the gap between pragmatic considerations and logical distinctions. Very broadly, the relation between the two notions is this. Theoreticity$_1$ is more comprehensive, in its possible range of application (which includes nonformalized theories) as well as in its conceptual approach (because it relies on or at least is closely connected with a theory of meaning). Theoreticity$_2$, in contrast, applies only to formalized theories and uses only "one half" of the conceptual frame of theoreticity$_1$, namely the "half" centering on determination. But in this domain theoreticity$_2$ provides a more precise and refined picture. If we want to press this relation into a single expression, perhaps 'specialization' is best. Theoreticity$_2$ may be regarded as a specialization of theoreticity$_1$. This relation does not devaluate theoreticity$_2$, because often it is through "interesting" specializations that comprehensive theories gain empirical content and scientific reputation (think of Newtonian mechanics and the law of gravitation).

Let us conclude by summarizing our main results in the form of four theses: (1) The distinction between theoretical and nontheoretical terms of a theory can

[23]At least if we do not adopt the coherentist picture.

be drawn in a precise way without reference to questions of meaning, and in line with the original ideas of logical empiricism. (2) Sneed's pragmatic "problem of theoretical$_1$ terms" – which arises from the existence of theoretical$_1$ terms – can be explained by a coherentist theory of meaning, which is applied to the special case of the meaning of a function in a theory. (3) If the criterion of theoreticity$_1$ is reformulated in a way that focuses on questions of meaning, it no longer contributes to the drawing of a distinction between T-theoretical and T-nontheoretical terms. (4) The new definition of theoretical$_2$ terms explains why – despite (3) – on the pragmatic level of scientists' behavior there do exist T-nontheoretical$_1$ terms.