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ON THE STRUCTURALIST APPROACH TO THE DYNAMICS OF THEORIES

I

One of the main events at the Fifth International Congress of Logic, Methodology and Philosophy of Science (London, Ontario, 1975) was a symposium on theory-change. The participants of this symposium were Professors J. Sneed, W. Stegmüller, and T. Kuhn.

In their talks Sneed and Stegmüller presented their set-theoretical (or 'structuralistic') view on the structure and dynamics of theories (cf. Sneed, 1976; Stegmüller, 1976b). Sneed's talk was a further elaboration of the approach developed in his book *The Logical Structure of Mathematical Physics* (1971). This book contains a partial explication of some of Kuhn's ideas on normal science and scientific revolutions. This explication was adopted by Stegmüller, who in his book *Theorienstrukturen und Theoriendynamik* (1973) made an attempt to further elaborate these Sneedian views and to relate them to contemporary philosophical discussions of theory-change. Stegmüller (1976b) contains a partial summary and a further development of the results of this book.

Kuhn (1976), in his comments on the set-theoretic or structuralistic Sneed-Stegmüller approach, strongly endorses this explication of his thoughts. This fact considerably adds to the importance of this new explication. I will below undertake a critical evaluation of Sneed's and Stegmüller's approach in order to see to what extent we now have a viable paradigm (or the beginnings of such) for research in the science of science. My detailed comments will mainly concern Stegmüller's book, which recently appeared as an English translation entitled *The Structure and Dynamics of Theories* (1976a). The page references will be to this English edition.

My discussion below will be conducted in two parts. First I will give a critical exposition of the Sneed-Stegmüller approach. After that I will present some, both general and specific, criticisms against this approach from the point of view of scientific realism. Let me point out right at the beginning that, in spite of the criticisms to be presented, I regard Sneed's and

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Stegmüller's work as very important. It is the first comprehensive and exact attempt to explicate such important notions as paradigm, normal science, scientific revolution. etc.. I see it as a prolegomenon to a full blown pragmatics of science, which we have so far lacked. My realist criticisms below should therefore be understood as an attempt to suggest improvements to the Sneed–Stegmüller approach so that its technical machinery becomes better acceptable to a scientific realist as well.

II

Stegmüller (1976a) starts his discussion by a detailed presentation of Sneed's (1971) set-theoretical conception of (physical) theories. This 'non-statement' view is based on the well known 'West Coast approach' (Suppes, Adams, etc.) to the formalization and axiomatization of a theory by means of a set-theoretical predicate. According to this approach a theory is taken to consist of (1) a bunch of *set-theoretical structures* (i.e. models in the sense of logical model theory, for all practical purposes) which satisfy certain axioms (i.e. a theory in the sense of the statement view, couched in a set-theoretical language) and (2) a bunch of *'intended applications'*. Thus e.g. Adams represented a theory T (essentially) by the couple $\langle M, I \rangle$, where M and I are, respectively, the mentioned components. The set of intended applications or models I is assumed to be a subset of the set of all models M.

Sneed (1971) added to this Adamsian approach two things: constraints and an account of theoreticity. Constraints are needed to suitably connect the various applications of a theory. For instance, if we measure masses of objects we may want to require that the same object must have the same mass in all (or most) of the intended applications of the theory and that, furthermore, mass is an extensive quantity. Let us call the set of such (set-theoretically formulated) requirements or constraints C.

Before we can say what a theory T is in Sneed's and Stegmüller's sense we still need an account of how to divide functions appearing in the intended applications into theoretical (relative to T) and non-theoretical (relative to T). This they do, roughly, as follows (cf. Stegmüller, 1976a, pp. 44–45). A function f is defined to be *theoretical relative to* T if and only if for every model (structure) $m_i \in I$ each method (to be found described in 'the existing expositions' of T) of measuring values of f for some individuals in m_i presupposes that $m_j \in M$ for some $m_j \in I$.

We can immediately see from the above definition that this theory-relative notion of theoreticity, which, to be sure, is based on a plausible idea, is very 'descriptivistic' and 'relativistic' (with respect to the current state of science). Consider this. I may write a physics book (no matter whether commonly accepted or not), which almost overnight changes a T-theoretical function into non-theoretical (cf. the quantification in 'every model $m_j \in I$'). Furthermore, Stegmüller's (1976a) discussion indicates that it must be the case that for every intended application there is an existing exposition. This makes the proposed notion of theoreticity at the same time overly normative. (This should keep textbook authors busy. They must also be alert and closely follow changes in the set of intended applications!) – We shall later mention still some further criticisms against the Sneedian notion of theoreticity.[1]

If the Sneedian account of theoreticity is accepted, a problem arises in accounting for the empirical content of a theory. For example, it won't do to claim that the set-theoretical statement

(1) $I \subseteq M$

does the trick. This is simply because I becomes to depend conceptually on M on the Sneedian account, and therefore (1) is non-contingent.

Now it seems that as a way out one must either 1) give up the Sneedian account of theoreticity, or 2) refuse to construe I on the basis of Sneedian theoreticity, or 3) give an account of the empirical content of a theory by means of intended applications where only non-theoretical functions (or relations) occur. Sneed and Stegmüller opt for the third of these alternatives.

Let us now follow Stegmüller's (1976a) construal. Given the Sneedian account of theoreticity he gets from the set M_p of all *possible* structures or models (of which set M is a subset) the set M_{pp} of all *possible partial* models. The set M_{pp} is obtained by simple reduction from M_p. Thus if $m = \langle D, f_1, \ldots, f_k, f_{k+1}, \ldots, f_n \rangle$ and the functions $f_{k+1}, \ldots, f_n$ are T-theoretical then $m_0 = \langle D, f_1, \ldots, f_k \rangle$ is a possible partial model belonging to M_{pp}. The structure m_0 is called a *reduct* of m, in symbols $m_0 = r(m)$. We also say that m is an *enrichment* of m_0 (note that m and m_0 have the same domain). Analogously with this, M_p can be defined to be an enrichment of M_{pp}.

Now we can define a theory T to be a couple $\langle K, I \rangle$. Here $K = \langle M_p, M_{pp},$

$r, M, C\rangle$ is called the *core* of the theory and I the set of its intended applications. Note that now, and from hereon, $I \subseteq M_{pp}$.

Given the above account of the structure of theories Stegmüller goes on to discuss the dynamic aspects of theorizing. He defines a set-theoretic entity $E = \langle M_p, M_{pp}, r, M, C, L, C_L, \alpha\rangle$ called the expanded core of a theory (cf. Stegmüller, 1976a, p. 117). It consists of a core and the new elements L, C_L, and α. Here L is a set of *laws* (a set of subsets of M_p) with $M \in L$, and C_L is a *constraint* set accompanying L. α is an *application* relation which mainly serves to guarantee suitable closure properties for laws.

As indicated, a law (for a core) is simply a class of models belonging to M_p. But this characterization is surely too wide. For instance, Stegmüller would have to accept as a law the singleton of a model with one-element domain, i.e. set $\{\langle\{a\}, f_1, \ldots, f_k, f_{k+1}, \ldots, f_n\rangle\}$. The inadequacy of Stegmüller's account of lawlikeness and laws is obvious.

The laws in L serve to strengthen the core theory e.g. by saying something more specific about a certain subset of I and, perhaps, to apply the core to totally new areas. The constraints C_L are needed, especially, to suitably connect those intended applications that L says something new about.

Stegmüller now goes on to define a function $A_e(E)$ (Stegmüller (1976a), p. 117). The value of this function is the set of those subsets of M_{pp} which subsets can be enriched with theoretical functions so that there exists a set Y of enriched models satisfying the original axioms of the theory (which defines M) and complying with C C_L, and L.

The set I of intended applications is now claimed by the theory to belong to $A_e(E)$. That is, the *empirical content* of the theory $\langle K, I\rangle$ relative to E (or when it has been expanded to E) is given by

$$(2) \qquad I \in A_e(E).$$

Stegmüller calls (2) the *theory proposition* (relative to E). (If we speak of a theory in a strong sense and identify it with $\langle E, I\rangle$ rather than $\langle K, I\rangle$ (2) is a theory proposition simpliciter.) (2) is a contingent proposition contrary to (1), given the Sneedian account of theoreticity. Thus we have finally obtained a solution to the problem of giving the empirical content of a theory.

There is also another way of stating the empirical content of a theory within the Sneed–Stegmüller approach. We can use 'informal' set-theoretic language to express (2). This is accomplished by the following (expanded)

Ramsey–Sneed sentence for a theory:

$$\text{(3)}\qquad \begin{aligned} &\bigvee x\{x \subseteq S \wedge Co(x, a, R, \rho) \wedge \\ &\bigvee x^1 [x^1 \subseteq x \wedge x^1 \subseteq S^1 \wedge Co(x^1, a^1, R^1, \rho^1)] \wedge \\ &\vdots \\ &\bigvee x^n [x^n \subseteq x \wedge x^n \subseteq S^n \wedge Co(x^n, a^n, R^n, \rho^n)]\}. \end{aligned}$$

The symbols in this '*central empirical claim*' of a theory can be explained as follows. (I think Stegmüller would agree with the following, too.) The set-theoretic predicate symbol S designates M, and its restrictions $S^1, \ldots, S^n$ designate the subsets $M^1, \ldots, M^n$ of M. Co is a constraint predicate such that $Co(x, a, R, p)$ means: the set of theoretical functions in the set (designated by) x, which is an enrichment of the set (designated by) a, is constrained by $\langle R, \rho \rangle$. Here x stands for enriched sets of models and a designates a set of possible intended applications. (Note: I is just such a set a could designate.) R stands for a relation suitably constraining the domains of the functions while the relation designated by ρ at the same time makes the values of the functions comply with the domain-constraint. The super-indices $1, \ldots, n$ refer to the various further specifications and restrictions in different areas (subclasses of what a and x stand for). (This corresponds to L and C_L in the earlier purely semantic treatment.)

In Stegmüller's view (3) is a statement *expressing* the proposition (2). As indicated, we must think that (3) be formulated in an 'informal' set-theoretical language with an 'informal' set-theoretical semantics yielding propositions like (2). Given this, we have four different things to deal with here. First, we have a theory, which is a couple $\langle K, I \rangle$ (theory in the *weak* sense) or, alternatively, a couple $\langle E, I \rangle$ (theory in the *strong* sense). A theory thus viewed is just an ordered bunch of set-theoretic structures, which *says* nothing about anything (although it can be *used* to say; cf. (2)). Within the statement view, which construes a theory as a set of (suitable) statements, a theory on the contrary, *is* an entity *saying* something.

Secondly, we are here dealing with a theory proposition (of the form (2)), which gives the empirical (or, if you like, also the factual) content of the theory. Thirdly, we have the (expanded) Ramsey–Sneed sentence, which says what the total empirical claim of the theory is. Fourthly, we have the parts of reality, something 'out there', that we are investigating. They are supposed to be represented by the models in I.

At this point we may briefly mention three – so far unanswered – criticisms by Wójcicki (1974) against the 'relativistic' character of the Sneedian notion of theoreticity. This relativity shows up when considering the empirical claim (in Sneed's and Stegmüller's sense) made by a theory. The first of Wójcicki's criticisms relates to the 'decomposition' of a theory (here taken to consist simply of the couple $\langle M, I \rangle$ or, equivalently, the corresponding set of axioms defining M and I). We may now decompose a theory into parts so that although the parts taken together are equivalent to the original theory yet they do not jointly yield the same total empirical claim as the original theory. This is basically due to the change in the status of theoreticity among the functions when decomposing the theory. Secondly, Wójcicki shows that if we define a new function concept in terms of some concepts occurring in a theory I the obtained definitional extension T' (of T) may differ in empirical content from T. This is again due to Sneed's relativistic notion of theoreticity. Thirdly, the addition of new empirical laws (law statements) to a theory T gives a theory T' stronger than T. Yet the empirical claim made by T' need not be stronger than that by T. This is mainly due to the fact that some non-theoretical functions of T become theoretical in T'.

III

Given the above account of the structure of theories within the Sneed–Stegmüller approach, we are ready to discuss theory-dynamics and the Kuhnian notions of normal science and scientific revolution. In order to explicate the course of normal science Stegmüller (1976a) defines two pragmatic concepts of *holding a theory*: a Sneedian and a Kuhnian one. What interests us here most is the Kuhnian one. Let us consider it briefly and sketchily.

First we need the notion of a *Kuhnian-type theory of physics.* It is defined to be a triple $\langle K, I, I_0 \rangle$. Here the core K is as above, I is a set of homogenous physical systems such that the domains of these systems are suitably 'linked' (see Stegmüller (1976a), p. 164). I_0 is a subset of I. It is the set of paradigm-examples to which the originator, say p_0, of the theory first successfully applied his theory at t_0. In the case of Newtonian mechanics, for instance, the solar system with its various sub-systems, several comets, freely falling bodies near the earth's surface, tides and pendulums are included in I_0. I think that I_0 cannot here, nor usually in other cases either, be *extensionally* determined. Stegmüller requires that, however. He understands an extensional description of a set to be a list (recursively enumerable?) in which the individuals belonging to the set are explicitly named. I don't think that, for instance, Newton so named (nor should he have) the falling bodies, tides, pendulums, etc. which he considered his theory to apply to. (We shall return to the problem of extensional describability later.)

The notion of a Kuhnian-type theory of physics requires, furthermore, that, for all times t, any person p assuming the theory $\langle K, I \rangle$ to apply to some set $I_t^p \subseteq I$ at t will believe that $I_0 \subseteq I_t^p$ (see Stegmüller, 1976a, p. 194 for a more exact account).

Now we may say that a person p holds, in the Kuhnian sense, a physical theory $T = \langle K, I \rangle$ at time t if and only if the following conditions are fulfilled. First, $\langle K, I, I_0 \rangle$ is a Kuhnian-type theory of physics. Secondly, there exists a strongest expansion E_t^* among those expansions E_t concerning which p believes that $I \in A_e(E_t)$ at t and concerning which p has (at t) observable data supporting the mentioned theory propositions. Thirdly, p chooses I_0 as the set of paradigm examples for I. Fourthly, p believes at t that, for any $t', I_0 \subseteq I_{t'}^p$, where $I_{t'}^p$ is a set of intended applications to which p believes, at t', the theory to successfully apply. Fifthly, p believes at t that there exists an expansion E of K such that a) $I \in A_e(E)$ and b) $A_e(E) \subset A(E_t^*)$. In other words, p believes at t that stronger expansions than E_t^* exists for the core K.

The above is (apart from some small corrections) Stegmüller's definition for holding a theory in the Kuhnian sense (cf. Stegmüller, 1976a, pp. 194–195). I find this notion of holding a theory somewhat too strong in one sense and somewhat weak in another. The second condition seems slightly too strong. Why should we require that among those expansions which p believes to be successful at t there must be *the* (or even *a*) strongest such expansion? I think we can *at most* require that E_t^* be an expansion that p *believes* to be the strongest. Furthermore, people can surely continue *using* a theory (e.g. Newtonian mechanics) even if they no longer hold it or believe in it.

Stegmüller's above definition is too weak in that it requires only *observable* support for believing a theory proposition to be true. As physical theories cannot in general be directly, i.e. without any auxiliary theories and assumptions, confronted with data, it seems that we should also require suitable *theoretical* support in addition.

A vagueness in Stegmüller's characterization is that nothing is said about the concepts of observable support and belief. Concerning belief I would require that an analysis of it referring (among other things) to scientists' various *actions* (and dispositions to act) would be essential.

Now, if several persons hold the same theory, they will be said to belong to the same *normal scientific tradition.* The course of normal science then involves this community of scientists working to further expand the core K to apply to elements in I–I_0 and to use it to say more about the elements in I_0. In most cases such normal-scientific 'puzzle-solving' does not proceed without anomalies arising and disconfirming data coming up. In such cases a normal scientist may revise either the special laws L, the constraints C_L or the set I. In fact, the fate of the elements in I–I_0, and hence of the content of the set of intended applications, may be determined on the basis of whether the theory is judged to be expandable to them. (This is the principle of *autodetermination* of I.) Thus during normal science the core K and the set of paradigm-examples I_0 are kept immune to revision. Progress within normal science comes to mean finding new expansions for the core or enlarging the set of intended applications. (Note: in our above definition of holding a theory the set I in $\langle K, I \rangle$ must be regarded as an 'open' set if we are to allow changes in its content.)

There are some further remarks concerning Stegmüller's explication of normal science to be made. First we notice that there can be several linear partially ordered 'branches' of nested expansions (or, more exactly, nested $A_e(E)$'s) starting from the core K. However, one single person can only 'hold' one such branch. This means that 'subschools' or 'subsciences' are possible here, as they should.

It is to be noted that Stegmüller's normal science has nothing to do with dominant science or theorizing. It is in no way required that e.g. a majority of scientists belong to one and the same normal science within a given field. For instance, to take an example from outside physics, in psychology there are lots of different theories of motivation. It seems that we could say that there are several 'normal sciences' of motivation research in (something like) Stegmüller's sense. I think we should account for the 'distribution' of such normal sciences and for their interplay in our science of science. (Kuhn might perhaps here say that we are dealing with preparadigmatic science only. If so, Stegmüller's explication of Kuhn at this point is not quite successful if the above kind of cases are to be covered.)

Another feature that Stegmüller's explication handles poorly is the role of common *data-collection* and *data-analysis routines*. In my view they are quite central to any paradigmatic scientific research. A benevolent interpretation of Stegmüller's characterization would be that they are (in part) accounted for by the constraints C and C_L (and perhaps by the characterization of theoreticity). But I think there is much more to be said about them even in formal terms.

Under what conditions do people come to hold theories and when do they cease to hold them? Stegmüller says very little about this. If we are to think of our science of science as an *empirical social science*, as e.g. Sneed (1976) requires, we may perhaps postulate various economic political, social, historical, social-psychological, psychological, etc. factors to explain the rise and fall of normal sciences. But this is a sociologist's rather than a philosopher's task and something to be done only after we have built an adequate conceptual framework for our science of science and after our sociologists and historians have given us the data and regularities to be so explained.

Let us now go on to discuss scientific revolutions. They are typically related just to giving up a core theory. Stegmüller distinguishes between two types of scientific revolution: (1) the transition from pretheory to theory, and (2) the dislodging of one theory by another. The first kind of 'revolution' in physical science consists of the appearance of a physical theory explaining a piece of reality for which so far no theory, at least no physical theory, existed. The second type of revolution consists of the rejection of a current theory – i.e. a theory scientists have held in the Kuhnian sense for a certain period of time – in favor of another one.

While Stegmüller's analysis of normal science is informative and interesting his treatment of revolutions is rather meagre and programmatic. Let us briefly consider his discussion of the second, and more important, of the above kinds of revolution. His basic thesis is:

(R) The dislodged theory is reducible to the dislodging theory (Stegmüller, 1976a, p. 216).

Before discussing in more detail the content of this thesis let me note that its epistemic and semantic status does not become quite clear from Stegmüller's discussion. Is it true a priori (or analytically) or is it to be taken as a contingent thesis? Perhaps Stegmüller would want to regard it as a contingent one. But in that case he should have given us an independent analysis of the concept of revolution (or revolutionary theory). This he does not do. If again (R) is regarded as analytic or true a priori, then I think it hardly covers all cases of revolution of the second type (cf. Laudan, 1976, for arguments to this effect). To what extent it does also very much depends on what is meant by reduction. Let us thus summarize Stegmüller's approach to reduction.

Stegmüller basically employs Adams' and Sneed's account of reduction, which – as Stegmüller emphasizes – is free from any *teleological* metaphysics. Roughly speaking, if a theory T' serves as a reducing theory for T, then the respective sets of possible models M'_p and M_p must be suitably correlated with each other so that for any element of M_p there is a counterpart in M'_p. Given such a correspondence relation ρ, it is required, roughly, that if ρ obtains between any respective partial possible models m_0 and m'_0, and if m'_0 has an enrichment in the set M', then, on the basis of ρ, to this enrichment corresponds an enrichment of m_0 within M. On the basis of this notion the reduction of an expanded core E to a respective expanded E' can be defined. The reduction of T to T' now comes to mean that the expanded core of T reduces to the expanded core of T' and that every intended application of T must stand in the reduction relation to an intended application of T' in the sense that both applications have enrichments which (in the appropriate truth-preserving way) stand in the reduction relation to each other.

One immediate problem with Sneed's and Stegmüller's relation of reduction is that it is a purely formal relation. I would not be surprised if some arbitrary and entirely disconnected theories could be proven to stand in this reduction relation to each other. The reduction relation should be defined in a non-arbitrary fashion, for instance, in terms of *explanation*, I think (cf. Tuomela (1973), Ch. VII).

Stegmüller claims that no incommensurability problems, due to the entirely different conceptual apparatuses often employed by the dislodging and the disloged theory, arises. I do not think this approach fares any better at least than the statement approach here, however. We shall later return to this problem.

Stegmüller does not really say much in favor of his thesis (R). He offers the reduction of rigid body mechanics to particle mechanics and the reduction of thermodynamics to statistical mechanics as examples. Even if it were granted that these reductions have been successfully explicated, we are still far from accounting for e.g. the dislodgment of Newtonian mechanics by relativistic mechanics and for other similar cases. Stegmüller (1976b) suggests that we may perhaps have to liberalize the above reduction notion and allow for *approximation reduction.* At least that is definitely needed. But I think we should rather try to work with *approximative* and *corrective explanation* (cf. Tuomela, 1973, Ch. VII). The idea is that the dislodging (new) theory approximately explains the dislodged (old) theory by showing why the domain-objects obey the old theory to the extent they do and why they fail to obey the old theory to the extent they have been found to do so.

We have now presented Stegmüller's basic concepts and results concerning the structure and dynamics of theories. In addition, Stegmüller (1976a) contains lots of interesting discussions of many relevant philosophical topics, especially of topics that Kuhn's critics have brought up. Thus he discusses the charges of irrationality against Kuhnian normal scientists and revolutionaries, of relativism and holism, and so on. The general tenor of Stegmüller's remarks is that science in all its phases is rational and holistic as well as less relativistic than charged. In the context of his discussion of these issues Stegmüller also gives analyses of e.g. Popper's, Lakatos', Feyerabend's, and others' views of theory-dynamics. I find Stegmüller's comments reasonable and more or less acceptable on the whole. (His comments are rather obvious consequences from his formal account of the structure and dynamics of theories.) Let me, however, take up some points where I seem to disagree.

Stegmüller (1976a) analyzes Lakatos' notion of sophisticated falsificationism in terms of the above concept of reduction. Thus he thinks that within a Lakatosian research programme consisting of a sequence T_1, $T_2, \ldots, T_k, T_{k+1} \ldots, T_n$ of theories the relation between T_k and T_{k+1} would be that of reduction. However, I am afraid that Stegmüller goes wrong here. For in Lakatos' account a research programme seems to correspond to Kuhnian normal science and not to Kuhnian revolution. Lakatos' explication of the latter is a shift from a research programme to another.

To my surprise Stegmüller (1976a) seems to be happy to accept the *dual-language* view (in something like a Carnapian sense) for theories rising from 'pre-paradigm periods' (p. 208). Furthermore, equally surprisingly he seems to believe in the existence of *crucial* experiments in spite of his holism (p. 238).

Finally, in my opinion Stegmüller puts definitely too much emphasis on his 'non-statement' view of theories. I say this because the statement view and the non-statement view are in a sense logically intertranslatable, as we shall see. Stegmüller too often bases his arguments against other philosophers on the fact that his notion of theory is a different sort of entity than what these philosophers (or ordinary usage) take it to be (e.g. one obviously cannot corroborate or falsify a couple $\langle K, I \rangle$). But such arguments are rather cheap. Stegmüller should of course have used what in his treatment corresponds to these other philosophers' notion of theory.

IV

We shall now proceed to a critical evaluation of the Sneed–Stegmüller approach from the point of view of scientific realism (and especially the realist view on theories and theory-dynamics sketched and discussed in Tuomela, 1973).

As already claimed, Stegmüller (1976a) makes too much out of the contrast between Sneed's and his non-statement view and the traditional statement view, for there is a far-going intertranslatability between the two approaches. To see this, we first note that the structures that Sneed and Stegmüller consider are just models in the sense of logical model theory – or at least they are, so to speak, elliptic representations of such models. I say the latter because set-theoretical structures often contain e.g. real-valued functions but yet the set of real numbers does not occur in these structures. But we may easily add such components into them to get ordinary well defined Tarskian models. Let us below suppose for our metatheoretical purposes that this has been accomplished.

How about the axioms used to define a set-theoretical predicate? Consider the definition: m is an S (set-theoretical predicate) if and only if m satisfies certain axioms A. In fact, already this definition indicates that there is an equivalence between the non-statement view and the statement view. A is typically couched in informal set-theoretic language (cf. our (3)).

In those cases when A can be translated into first-order predicate logic it is easy to show that, from a logical point of view, there is no essential difference (as to satisfaction and truth) between the set-theoretic, the ordinary model-theoretic, and the corresponding linguistic (proof-theoretic) approach. (In part this is of course due to the completeness theorem first-order logic.) Let us consider this case first.

Suppose we are dealing with a formal first-order theory with uninterpreted (in the sense of factual or empirical interpretation) predicate constants. Suppose further that this theory is going to be interpreted by means of some set of observational (or empirical or non-theoretical, as you like) models such as the set I above. (Here we shall not yet distinguish between semantical and 'non-semantical' interpretation). We cannot accomplish it by means of an analogue to formula (1). Taking I^r to be a set of enriched intended models, it would be $I^r \subseteq M(A)$. (Here $M(A)$ of course means the set of models in which A is true.) That analogue would, however, turn out to be true a priori, given the Sneedian account of theoreticity (cf. Przełęcki, 1974, for a longer treatment of this).

If we want to avoid making the assertion of the theory's axioms true a priori, we may proceed as follows. We can (at least technically) divide the axioms A into a synthetic and an analytic component. One relatively good way of doing this is to take A^R, the Ramsey sentence of A, to express the synthetic component (and correspondingly $A^R \supset A$, the Carnap sentence of A, to express its analytic content). Now the resulting improved statement

(4) $\qquad I^r \subseteq M(A^R)$

can be shown to be equivalent, given the Sneedian account of theoreticity, to

(5) $\qquad I \subseteq M(A)/0,$

where $M(A)/0$ means the set of models of A restricted to the non-theoretical functions (and other relations if present). (See Przełęcki, 1974, p. 100 for a proof). Thus we can say that the empirical claims made by a physical theory turn out to be equivalent to statements asserting the truth of the axioms of the theory. We can see that (5) corresponds to the special case of (2) with no constraints C or C_L and no special laws L. (Would we have a first-order formalization of these elements they would immediately be taken care of by our above treatment.[2])

How far can we then get with first-order formalization? It is not really necessary to try to answer this question here. Let me just point out a couple of relevant things. First, we can formalize set-theory in first-order logic and thus get a powerful machinery. However, we have to tolerate non-standard models all along and thus cannot get as firm a grip of e.g. real numbers as we usually like. Thus, (syntactically) first-order theories incorporating lots of mathematics seem to need a non-elementary model theory, we may at least say. It is not my intention here to really speak for first-order formalization. It suffices for a defense of a statement view of theories to point out that given practically any bunch of structures we may define, and indeed need to, for many purposes, a language to speak about those structures. Although model-theoretic treatment typically gives more information than the corresponding linguistic (in the mentioned sense) treatment, I will argue below that there are not only logical but also several philosophical reasons for complementing the model-theoretic approach with the linguistic one.

What kind of philosophical reasons are there then for complementing model-theoretic machinery with the use of a well-defined language? One of my most basic reasons is related to giving *semantic content* and (what is to be kept separate) *empirical import* to a theory. What I am going to say is especially pertinent to the sciences closely connected to 'everyday thinking' and the 'manifest image' (to use Sellars' term). At least in those sciences (including e.g. the social sciences, archeology, macrobiology) – but also in physics, I think – we should think of theories primarily as sets of (suitable) statements formulated in an interpreted language, i.e. in a language where the extra-logical predicate constants have been (at least technically) interpreted (given reference) by means of suitable semantic postulates (typically speaking about kinds of individuals, properties or other generic entities) even if these postulates need not be *fully* understood.

For instance, to go to psychology, a predicate '*S*' in a psychoanalytic theory could be interpreted by the semantic postulate "'*S*' refers to the class of subconscious wishes'; or a predicate '*M*' in a theory of cognitive processes might be postulated by a suitable rule to refer to an agent's short-term memory, and so on. Thus we give reference to predicates by suitable semantic postulates. The senses, and thus the meanings, of predicates then become specified at least in part through the axioms of the theory (plus perhaps through some other means, e.g. considerations related to analogy). The axioms of a theory thus come to have also an important semantic function. (See Tuomela, 1973, Chapter V, for more details of this kind of account.)

The above view of interpretation is in several respects different from that we get from Stegmüller's account. To start our comments, we first notice that in a theory $\langle K, I \rangle$ K is just a bunch of purely formally characterized structures. Thus, whatever factual *semantic* content the theory has, must come through I. In other words, I must serve to give both semantic content and empirical import to the theory. In fact these factors, which in my view have to be kept sharply distinct in principle, are never really separated by Stegmüller (1976a), and I do not know whether he thinks he should. (Would he accept e.g. the earlier discussed approach to the analytic-synthetic division?)

Stegmüller does not either speak much about the meanings of scientific terms. I think this is at least in part due to the fact that Stegmüller's approach is so excessively obsessed with set-theoretic structures. It gives hardly any consideration at all to 'terms' (functors, predicates) and other linguistic entities. But only these are direct 'meaning-carriers'. Set-theoretic entities are, however, referents of linguistic entities. Thus they may be taken to (at least vicariously) give meanings (at least references) to the terms of the theory. Stegmüller never really defines such a thing as *the* function f (e.g. mass) as opposed to the specific functions f_i, where i ranges over all the models of the theory. We do need something like that, however. We may satisfy this need simply by defining a functor F which is interpreted, respectively, as the various f_i's in the models m_i of the theory.

Without such functors (or something comparable, such as 'similarity types') we can make no sense, for instance, of Stegmüller's central holistic claim (Stegmüller, 1976a, p. 240): "A change in the 'range' of a theory entails a change in the truth conditions of the empirical claims about the values of T-theoretical functions. This changes the meaning of the T-theoretical terms designating these functions." In this thesis we are in fact implicitly required to have a well defined language with functors (or something comparable). As Stegmüller never gives us such a language this thesis is somewhat hard to evaluate. In any case I think it is wrong. The second statement is simply false. It would be true only under a strong verificationist theory of meaning.

Recall: any changes in the $A_e(E)$-values, no matter toward how 'pragmatic' technological applications the theory is driven, would change the truth values of relevant empirical statements and hence the meanings of the theoretical functors, as I understand Stegmüller. Furthermore, what would happen to the meanings of theoretical terms if the empirical claim of the theory were empty (i.e. if $A_e(E) = \{X \mid X \subseteq M_{pp}\}$)? I think the Sneed–Stegmüller approach definitely must give us an improved account of the meaning of theoretical terms.

A related problem comes up in connection with reduction. Stegmüller seems to think that his set-theoretical approach avoids any 'incommensurability' problems (in the Kuhnian or Feyerabendian senses) that might arise for a dislodging and a dislodged theory. (For instance, 'mass' in relativistic mechanics has been claimed to be incommensurable with its counterpart in Newtonian mechanics.) But if there are any such incommensurability problems they will of course reappear when defining a reduction relation ρ. A successful definition of ρ means just comparability in an extensional sense. If more is needed, ρ does not (as such) accomplish it. (The use of a merely formally defined ρ, moreover, will usually worsen the situation as far as philosophical clarity is concerned, for it lumps together unsolved lots of important problems other philosophers have been busy distinguishing and pointing out.)

Let me now briefly return to the problem of how to specify I, which is to give the theory all the factual content it has. As I have claimed in Chapter V of Tuomela (1973), there are good reasons to think that an extensional approach in terms of a set of intended models is insufficient. For one thing, I is usually a highly *idealized* set (e.g. contra data-structures it contains real-valued functions, etc.). Secondly, I is an *'open'* set which cannot be extensionally and recursively enumerated. These things would be admitted by Stegmüller as well. But contrary to Stegmüller I think that not even I_0 can always be extensionally given. For instance, in psychology we may be dealing with e.g. responses, actions, or cognitive processes. We cannot plausibly think that some paradigmatic *tokens* of e.g. thinkings-that-p or X-doings or something like that would constitute I_0. We are rather dealing with *types* in giving intended applications, and this is to be done by means of interpreted predicate constants as sketched earlier. (Yet, in the ontology, i.e. domains of models, of our theory we may have tokens of events, etc.)

As soon as we come to see that I cannot be extensionally given we should in fact draw the conclusion that we need something more than merely ordinary extensional set theory. Yet this 'more' has not been properly incorporated into the Sneed–Stegmüller approach (cf. again our postulational method using interpreted predicates). This is a defect in the system. Any loose talk about 'intensional descriptions' is insufficient.

There are yet some further reasons for introducing and using a well defined language and hence for accepting at least a part of the statement-view of theories to complement the model-theoretic view. One such further reason is that in speaking about *laws of nature* (or generalizations on the whole) we get a much better grip of them when using a linguistic formulation. (Note: under the set-theoretic construal the set of models expressing a law has got to be potentially infinite, in my view.)

Another topic where the need for language comes up is *explanation.* At least for me explanation is a *pragmatic* question-answering business and connected to how the explainer and explainee state what they have to say (see Tuomela 1973, and especially 1976 and 1977 for a discussion). If this is accepted we definitely need a language for our explanations. Of course, one might be able to partially translate such a linguistic account of explanation as mine into a model-theoretic account, but the model-theoretic conditions would turn out to be very complicated and hardly philosophically illuminating. (Note: Stegmüller's (1976a) brief set-theoretic account of explanation on p. 100 is obviously too simple and therefore open to paradoxes.)

There is yet another sense in which well defined language is important, and it relates to the notion of semantical Ramsey-eliminability, which Stegmüller makes rather much out of (see e.g. Stegmüller, 1976a, pp. 81 ff.). This notion does not really make clear sense except relative to a fixed language, which, it seems, has to be a first-order one (cf. Przełęcki, 1974). We say that a theoretical predicate (functor) F is semantically Ramsey-eliminable from a theory T in a language $\mathscr{L}$ with $\lambda \cup \mu$ as its set of extralogical predicates, where μ (let us identify it with $\{F\}$ here) is the set of theoretical predicates and λ $(=\{O_1, \ldots, O_n\})$ the set of non-theoretical ones, if and only if there is a recursively axiomatizable subtheory $T'(\lambda)$ in $\mathscr{L}(\lambda)$ of $T(\lambda \cup \mu)$ such that all models $m' = \langle D, o_1, \ldots, o_n \rangle$ of $T'(\lambda)$ have an enrichment $m = \langle D, o_1, \ldots, o_n, f \rangle$ such that m is a model of T.

In this definition $\mathscr{L}(\lambda)$ must be a first-order language to keep the game interesting, it seems. Otherwise we could simply use the Ramsey sentence of $T(\lambda \cup \mu)$ to do the trick. Then every theoretical predicate would turn out to be semantically Ramsey-eliminable. To the extent that physical theories (the sole concern of Stegmüller, 1976a) can only be formalized in a second-order (or a higher-order) language, the notion of semantical Ramsey-eliminability loses its significance.

Let me finally turn to some critical remarks against the Sneed–Stegmüller approach which more clearly rely on a scientific realist's view of theories and theory-dynamics. The general point I would like to make is that this approach has a strong flavor of empiricism and instrumentalism, both of which related doctrines I oppose. Perhaps one cannot give a conclusive proof that Stegmüller's account is committed to some versions of these views unless we take Stegmüller's (1976a) own claim about his 'sensible empiricism' (p. 240) at its face value to commit him to some version of empiricism. (Also note Stegmüller's concern with the *empirical* as opposed to *factual* content of a theory.) Let me, however, here concentrate on his instrumentalism and make some remarks which indicate that the Sneed–Stegmüller approach in its present state better suits an instrumentalist than a realist (cf. also the explicit statement in Stegmüller, 1976b, p. 163).

First, we note that theoretical functions are not treated with the same 'seriousness' as non-theoretical ones. For instance, in the Ramsey–Sneed claim of a theory no theoretical functor constants appear – only existentially quantified variables. Of course, we might yet technically use such functors, construe them as fully referential (and thus take the specific theoretical functions in the models of the theory to 'represent' real properties). Yet the whole tenor of Stegmüller's discussion points towards instrumentalism – or at least to something less than a 'full blown' realism (such as that of e.g. Tuomela, 1973).

When discussing the interpretation of a theory $\langle K, I \rangle$ by means of I we noticed the possibility that $A_e(E) = \{X \mid X \subseteq M_{pp}\}$, i.e. the theory imposes *no* restrictions concerning M_{pp} i.e. what can be 'directly' (T-independently) measured. For instance, some realist axiomatizations of quantum mechanics (e.g. Bunge's) seem to me to fit this case. I don't see how the theory under Stegmüller's account really gets its empirical content and 'meaning' in this case, as any subset of M_{pp} would equally well qualify as an I. The situation gets still worse if the theory contains only theoretical functions and no non-theoretical ones (cf. again quantum theory as an example). Can there be any I in M_{pp} then at all? I don't think so. (Recall that M_{pp} was obtained just through the Sneedian dichotomy theoretical/non-theoretical relative to the theory.)

One further but related thing is that in the Sneed–Stegmüller approach the theory is principally about the individuals in the domains of the models in I. It is logically possible, however, that in this approach M (the set of models of the axioms A) contains also other individuals. Such 'new' individuals would not have to be 'directly' measurable relative to T (i.e. there is no requirement concerning the T-independent measurability of any functions, theoretical or non-theoretical, concerning these individuals). Among them we might have important new ontological entities. Scientific revolutions often introduce such entities. (e.g. light quanta, electrons, etc. might be such new, not directly measurable and also clearly unobservable entities.)

Yet the Sneed–Stegmüller approach rather completely neglects such ontologically new individuals. Its concern in creating new expansions for theories and in giving empirical content to them (whereby only enrichments with the *same* domains as the I-models have count) is with the 'directly' measurable and, I think, normally observable individuals in the domains of the I-models. At least for a scientific realist this is a clear defect in this approach.

How would a scientific realist then cope with the above problems? If he likes, he could use the set-theoretic machinery, but complemented with a well defined language to speak about the set-theoretic structures. He would (or might) solve the meaning-questions by semantical postulates as indicated earlier. As a matter of fact he could also use a set of *typical* intended applications, say I', to give (partial) reference to the theoretical functions of the theory. But he also accepts the possibility that the theory has no 'directly' measurable content (i.e. either no non-theoretical functions at all or at least no non-empty non-theoretical claims).

The set of intended applications I in Sneed's and Stegmüller's sense might be considered to give partial characterizations of the empirical situations in which the theory – which itself may be solely about unobservable individuals and their theoretical properties – is tested and which it may be used to explain. Also various technological applications of the theory might be included in this set I.

In a sense, then the roles of theoretical partial models and non-theoretical partial models (i.e. elements of M_{pp}) become reversed in the scientific realist's construal. We accordingly come to ask questions like: Is it possible to find non-theoretical (or empirical) enrichments or other extensions for a partial model $\langle D, t_1, \ldots, t_k \rangle \in I'$ such that this extension is of the form $\langle D', t_1, \ldots, t_k, o_1, \ldots, o_n \rangle$, where $D \subset D'$ or where D' perhaps only some how weakly corresponds to D (cf. Tuomela, 1968), and that it satisfies some required 'measurement' laws and constraints. (Whether constraints would be needed at all in the proper or 'core' theory in this account is an open problem.)

In switching the roles of theoretical and non-theoretical (or empirical) functions in this way we come to regard the role of theoretical parts of theories (or, if they have no non-theoretical content, simply core theories) more important to theory-dynamics than anything like the empirical contents or claims 'connected to' these theories. I think this is as it should be and I think an examination of past and current science will support this, too.[3]

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NOTES

[1] I would like to point out here that none of the criticisms against the Sneedian account of theoreticity mentioned above or later in this paper apply to the account given in Tuomela (1969) and further discussed in Tuomela (1973) and (1977). According to that account a concept is theoretical relative to a theory T roughly when in 'measuring' this concept the 'paradigmatic' scientists in a scientific community K must rely on the truth of T in at least *some* applications. This characterization relies on the scientists' 'measurement-*actions*' (relative to the measurement routines in K) rather than to scientists' saying or to 'existing expositions' or any equally problematic notions. As my account only requires dependence on T in *some* applications it avoids the circularity and regress problems that the Sneedian account must face.

[2] Let me mention here that the examples of constraints Stegmüller (1976a) discusses seem formalizable in first-order logic. Consider, for instance, the requirement that the mass of an object be the same in all the intended applications. We might simply formalize this, for instance, by means of a suitable multi-sorted logic, which enables us to keep track of objects in different structures. If we are allowed to speak of models in the language we could, alternatively, formulate the mentioned constraint by the following statement:

$$(x)(r)(i)(j)\ [O(x)\ \&\ R(r)\ \&\ I(i)\ \&\ I(j)\ \&\ M(x, i) = r \supset M(x, j) = r].$$

Here the meanings of the symbols are as follows: $O(x) = x$ is a physical object, $R(r) = r$ is a real number, $I(i) = i$ is an intended application of the theory $M(x, i) = r$ translates 'the mass of x in i equals r units'.

[3] Some aspects of the kind of realist account of theory dynamics sketched above were examined in Tuomela (1973), especially Ch. VII. There I started with (linguistically formulated) *core theories* (which may be stated solely in the theoretical language) and briefly investigated their extensions to new areas. However, we still lack a comprehensive systematic and exact realist account of theory-dynamics.

It may be mentioned here some of the idealized features of the Sneed–Stegmüller approach could be removed on the basis the method of 'semantical and structural operationalization' that I have sketched in some of my works during the 1960's (see e.g. Tuomela, 1966, 1968). In these writings I studied in great detail the 'application process' (i.e. essentially the creation of expansions in Sneed's and Stegmüller's sense) of some mathematical learning theories axiomatized by means of set-theoretic predicates.

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