Annus LXXIX–I-II-III-IV 1985
Vol. 120



Rivista internazionale di sintesi scientifica

# SCIENTIA

International Review of Scientific Synthesis

ESTRATTO / OFF-PRINT

# STEGMÜLLER ON THE STRUCTURE OF THEORIES

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The German philosopher Wolfgang Stegmüller, a long time disciple and scholar of Carnap, has come to see in Joseph Sneed's work on the mathematical structure of theories of physics[1] the correct approach to the reconstruction of scientific theories.
Among other merits, Sneed's approach allows, according to Stegmüller, a better understanding of Kuhn's controversial philosophy of science.

In the first part of SDT[2] Sneed's approach, which is called «the Non Statement View» in opposition to the standard neopositivistic approach, termed «the Statement View», in explained and defended by Stegmüller.

The second part of the book uses Sneed's approach with some minor modifications, as a standpoint for the explanation and evaluation of Kuhn's ideas on normal and revolutionary science[3].
Kuhn's theses are considered by Stegmüller historically correct. However they seem to contradict some of the most largely accepted views on science, and have caused numberless strong objections on a philosophical ground. When rephrased in the Non Statement View many of those ideas seem to lose their unacceptability and find their philosophical vindication; while others[4] can more easily be recognized as mistaken. This fact counts for Stegmüller as a further argument for the Non Statement View.

## I - SNEED

The NSV[5] is based on the particular stand taken on two crucial problems. First, the axiomatization of theories by first order predicate calculuses, as it was in the program of Logical Positivism, results to be practically, if not in principle, impossible; on the other side, it seems also scarcely informative on a number of significant features of theories. Instead, Sneed develops P. Suppes' method of semantic axiomatization in the language of informal set theory; this couples the advantages of a much richer language with those of more directly characterizing a theory through its models rather than through its axioms. A theory is axiomatized by formulating in set theoretic terms a predicate which describes the models of the theory. The *definiens* for this 'theory predicate' essentially employs the conjunction of what are commonly seen as the 'laws' of the theory. The theory's empirical claim lies then in the claim that the theory predicate is true of a certain set of objects, the 'intended applications'.
The second problem concerns the nature of the theoretical-non theoretical distinction. The solution offered by the SV, which identifies this distinction with that between observational and non observational, is defective for a number of reasons: because the observational-non observational distinction itself is vague and relative[6], and because of the difficulties it

faces *a)* in giving a satisfactory account of the meanings of theoretical terms and of the empirical content of a theory, and *b)* in justifying the role of theoretical terms (Hempel's 'theoretician's dilemma') and the name given to them (Putnam'a 'challenge').

Sneed overcomes these problems by considering theoretical for *T*, or *T*-theoretical, those functions (and the terms designating them) which cannot be measured without recourse to some application of the theory *T*, therefore without presupposing that *T* itself is true (at least for that application). This makes theoreticity relative, so that functions which are *T*-theoretical may appear as non theoretical in some other *T'* presupposing *T*. There may exist in this way entire hierarchies of theories and corresponding levels of theoreticity.

In this way however the empirical claims of a theory *T* cannot be about objects characterized by *T*-theoretical functions (that is, *T*-theoretical objects), since the test of *T* would be circular. Instead, Sneed constructs them in form of a (modified) 'Ramsey sentence' about non *T*-theoretical objects. They become the claim that for such and such objects if the measurements described by *T* as measurements of the *T*-theoretical functions are performed, the values of other functions will stand to the obtained values in the relations prescribed by the theory.

Such claim may be read either realistically, as the claim that 'there exist' those *T*-theoretical functions and objects and the laws of *T* are true of them; or instrumentalistically, interpreting theoretical measurements and theoretical laws together just as rules of inference about values of non theoretical functions. The empirical success of *T* in fact cannot be used as a proof of existential claims about *T*-theoretical functions and objects, since it would be circular.

The SV considers theories as sets of universally quantified statements (the laws); and simplifying, as single statements (resulting from the conjunction of the laws), whereby a (complex) predicate of the form '$Px \rightarrow Qx$' is claimed to be true of all the objects in the universe, characterized in theoretical terms. Thus, there is one universal application of the theory.

According to Sneed, instead, the theory applies to a limited number of non theoretically characterized objects, the set of intended applications; this is the set of objects about which is made the complexive claim that, when theoretically characterized, they instantiate the theory predicate (i.e. are models of the theory). Moreover, the theory is not the statement expressing that claim, but the structures (the sets of models and of intended applications) involved in it; whence the name of 'Non Statement View'.

In Sneed's formulation a 'strong' theory of mathematical physics is a pair $<E, I>$, where *E* is an 8-tuple $<Mpp, Mp, r, M, C, L, CL, a>$, called 'expanded core'. *M* is here the set of models of the basic laws of the theory, itself called the 'basic law'; *Mp* is the set of the systems of objects characterized by all the functions of the theory, theoretical and non theoretical: they are the entities the theory is about, its possible models (formally, *Mp* is a set of *n*-tuples constituted by a domain *D* of individuals, and by $(k + m) = n - 1$ functions from *D* into the set of real numbers); *M* is thus a subset of *Mp*. *Mpp* is the set of all entities similar to the elements of *Mp* except for lacking the *T*-theoretical functions: they are the 'partial' possible models of the theory, in other words its 'empirical basis'. *r* is the 'differentiating function' between *Mpp* and *Mp*; practically, it identifies the *T*-theoretical functions, which constitute the difference between *Mpp* and *Mp*. *C* is the 'constraint' on *M*, the set of those sets of its members characterized by the obtaining of particular 'cross-connections' among overlapping applications; intuitively, they are the sets of models in which functions assume the same values for the same objects when it appears in two different applications; for example, suppose *C* is the constraint for Newtonian gravitational theory, and the systems Earth-Moon and Earth-Sun are two models of the theory: if the Earth has a different mass in the two models, the set of these two models is excluded by *C*, and does not satisfy the theory. *L* is the set of models for the special laws of the theory. *CL* is the constraint on *L*. *a* is the 'application relation', matching each special law to its special domain of application within the general domain of the theory.

The second member of $<E, I>$ is the set of intended applications, and is defined by the following conditions: 1) *I* is a subset of *Mpp*; 2) each element of *I* is a physical system; 3) the domains of elements of *I* are 'linked', i.e. they may be arranged in a succession such that





the intersection of each with the successive is not empty; 4) *I* is a 'homogeneous' set of physical systems (i.e. two or more of them may be combined to yield another element of *I*).

A 5-tuple $<Mpp, Mp, r, M, C>$ is called a 'core', and a pair $<K, I>$, where *K* is a core, is a 'weak' theory of physics.

The claim of the theory has the form $I \varepsilon A(E)$, where $A(E)$ is the $\bar{r}$-image of the intersection of the power set of *M* with *C* and *CL*. It says that *I* is one of those sets obtained by the sets which satisfy the theory by dropping the theoretical functions.

Sneed believes his formalism can be used to explicate pragmatic notions and aspects of the dynamics of theories. To this end he defines the pragmatic notion of 'holding a theory'. The definition is intended to capture the concept of a theory as something which remains identical over time, while undergoing successive refinements. One and the same core can in fact serve as a basis for different expanded cores, as different (typically, stronger and stronger) special laws are introduced.

Since laws here are sets of models, a law which is a subset of another is stronger, or more informative; and an expanded core is stronger than another when it includes stronger laws. The definition of holding a theory expresses the fact that who holds a theory uses the strongest available expanded core for which there is empirical evidence that it can be applied to the set *I* of intended applications; moreover, the holder has faith that even stronger expansions applicable to *I* can be found in the future.

## II - STEGMÜLLER

Stegmüller own contribution focuses on showing how Kuhn's ideas can be accomodated in Sneed's formalism. Kuhn's central notion of 'paradigm' is a rather complex and far from clear concept. According to Stegmüller, except for those features which are object of psychology and sociology rather than philosophy of science, the concept of paradigm can be rendered by the pair $<K, I_o>$, where *K* is a core as previously defined, $I_o$ a 'paradigmatic' set of intended applications. *K* captures the aspect of a paradigm as a set of fundamental assumptions which all members of a scientific community put as a basis of their 'normal science' activity (the latter typically consisting in finding and testing stronger and stronger expanded cores). Stegmüller maintains that the only viable way to identify the set *I* of intended applications is intensional: precisely, by specifying a subset of it, given extensionally, to which all members of *I* bear a 'Wittgensteinian family' resemblance; this set is just the set $I_o$. It captures the aspect of paradigms as relatively smalls sets of 'classical' examples and successful applications which have been found by the founder of the paradigm and become exemplars for the work of normal scientists. Because of the 'open' nature of a Wittgensteinian family it is undetermined exactly which members should be included in *I*; therefore different sets of intended applications may be used, all however including $I_o$.

Stegmüller defines the notions of a 'Kuhnian-type theory of physics' and of 'holding a theory in Kuhnian sense', which presuppose the idea of paradigm. They are based on Sneed's corresponding notions mentioned above, with the introduction of the set $I_o$, of the set $I_p^t$ of the intended applications used by a person *p* at a time *t*, and of a person $p_o$ who first introduced the paradigm at time $t_o$. The person $p_o$ used an expansion $E_o$ of *K*, and $I_o$ as paradigmatic set of intended applications.

In this way scientists may be seen to use a common paradigm $<K, I_o>$, while working on different expansions of *K* and different sets of intended applications; their goal is indeed to find the largest set *I* and the strongest expansion *E* applicable to it, based respectively on $I_o$ and *K*. Whenever the application of an expansion *E* to a set *I* fails, either one or both must be given up (cores, expansions, intended applications sets and theories are not the kind of entity that may be falsified, since they are not statements or claims; the empirical claims in which they are used, however, are).

If the failure involves a special law, this can be deleted or substituted, thereby producing a different expansion of the same core *K*, which in turn will be tested. Alternatively, one may take the experimental result as evidence that the particular physical system which failed to satisfy the law after all did not properly belong with the set of intended applications. In this case the expansion remains untouched, while the set $I$ is substituted by a new set $I'$, still characterized by the same paradigmatic set $I_0$. In either case the paradigm $\langle K, I_0\rangle$ is immune from rejection. The second alternative is the only possible when the failure concerns not an expansion, but the core itself; and the first when a member of $I_0$ is involved. Only in the exceptional case in which $K$ fails to be applied to $I_0$ the paradigm has to be rejected'. Except for this, even a prolonged series of failures to find a successful expansion $E$ or set $I$ for $\langle K, I_0\rangle$ is not a reason sufficient to reject it, unless a new and more promising paradigm is at hand: first because there are infinitely many expansions and sets of intended applications which can be tried, before any hope of success is lost; second, because $\langle K, I_0\rangle$ has at least proven successful with the limited set of phenomena to which it has been applied so far, and 'a broken oar is better than none at all'.

When explained this way in terms of the NSV Kuhn's immunity of paradigms to falsification loses much of that dogmatic flavour for which Kuhn has often been criticized.

In virtue of Sneed's solution to the problem of theoretical terms the NSV also accomodates Kuhn's ideas on theory-ladeness and theoretization. Partial possible models of a theory $T$ are in general characterized by theoretical concepts of some proceding theory. According to Stegmüller this is always the case: no function is non theoretical for all theories, because even the concepts of the basic theories of lengh, or of extensive quantities, cannot be reduced to an observational language.[8] In this sense the appearing of the first theories represents a 'revolution': there is no theoretically neutral way to describe what a theory is about. Therefore every observation is couched in terms which are theoretical for some theory.

The main target of Kuhn's critics has been, beside the concept of normal science, that of revolution, which threatens the traditional conception of science as a rational and progressive enterprise. Kuhn distinguishes two kinds of revolution: one is precisely the just mentioned passage from pre-theoretical knowledge to a theory, with the introduction of theoretical concepts. The second consists in the substitution of one paradigm by another. In terms of the NSV this amounts to the substitution of the core $K$ or the set $I_0$ or both. It may happen, as we have seen, in case of failure of $K$ to apply to $I_0$, or in case of repeated failures to formulate new satisfactory theory claims, if another more promising paradigm is available.

Kuhn's claim is that a paradigm shift involves a change of meanings of the terms of theories; theories before and after the shift are thus 'incommensurable': the choice between theories of opposite paradigms is not decidable by logical or empirical considerations, but by subjective features like simplicity, elegance, or even by psychological or sociological factors. In this claim the critics have seen the definitive proof of Kuhn's irrationalism, manifested already in his picture of normal science.

This time around, however, Stegmüller too criticizes Kuhn: meaning variance may cut deductive relationships, so that from one theory neither the other nor its negation may be deduced. But Kuhn is able to argue from this to incommensurability only because of the mistaken SV, which he shares with his opponents. Since theories are not sets of statements but mathematical structures, comparison and choice on the basis of their content does not require linguistic, but mathematical relations. In particular, Sneed's RED relation, holding between theories independently of any relation between their linguistic formulations, offers a basis for a fully rational choice.

The mere obtaining of RED between theories $T$ and $T'$ would ensure, according to Stegmüller, that $T'$ has at least the same empirical accomplishments (i.e. predicts, explains the same facts) as $T$. As a matter of fact, we shall see that this is not the case. A weaker requirement is possible, namely that $T'$ accomplishes at least as much (i.e. accounts for at least as many facts) as $T$; but it is dubious that RED ensures event that.

Since the empirical accomplishments are expressed by the theory sentence $I \varepsilon A(E)$, RED is intended to guarantee two conditions: *a)* that $A(E')$, the 'predicate' in the theory sentence of $T'$, contains at least the same information as the 'predicate' $A(E)$ of $T$ (or, in the weaker requirement, at least as much information); *b)* that the 'subject' $I'$ of $T'$ includes at least the same objects (in the weaker requirements, at least as many) as the subject $I$ of $T$.





Three reductions relations between expanded cores are proposed as alternatives guaranteeing condition *a)*: WRED, IRED, RED. WRED is defined so to ensure that $E'$ be empirically as strong as $E$ (and therefore $A(E')$ as $A(E)$; IRED is defined so that $E'$ be theoretically as strong as $E$; and RED so that both empirically and theoretically $E'$ be as strong as $E$.

According to the definition, any relation $\varrho$ is a RED iff:

1) It is a relation from $Mp$ into $Mp'$, and its reverse is a function; thus, for any member of $Mp$ there is at least one different $\varrho$-correspondent member of $Mp'$: $Mp'$ is at least as 'rich' as $Mp$; the idea that is in the back of the author's mind, however, is that of a 'same object as' relation.

2) For any subset $Y'$ of $Mp'$ which fulfills $E'$ (i.e. belongs to the intersection of the power set of $M'$ with $C'$ and $CL'$) also its $\bar{\varrho}$-image $Y$ fulfills $E$ ($\bar{\varrho}$ is a relation obtaining among sets whose members are $\varrho$-images of each other).

3) If $y'$ is a 'theoretical enrichment' (i.e. the $r$-image) of an $x'$ belonging to $Mpp'$, if it belongs to $Mp'$ and has a $\varrho$-image $y$, any other eventual enrichment of $x'$ belonging to $Mp'$ has a $\varrho$-image, and this is an enrichment of the same $x$ member of $Mpp$ as $y$ is.

Point 3) is introduced in order to allow that, by dropping theoretical functions, from $\varrho$ we get a relation $\varrho^*$ which is a WRED, i.e. which 1) is a relation from $Mpp$ into $Mpp'$ whose reverse is a function, and 2) is such that for any subset $X'$ of $Mpp'$ fulfilling $A(E')$ also its $\bar{\varrho}^*$-image $X$ fulfills $A(E)$.

WRED, IRED and RED respectively are defined for cores as a relation $\varrho$ which obtains between two cores $K$ and $K'$ whenever for any expansion $E_i$ of $K$ there is an expansion $E_i'$ of $K'$ such that $\varrho$ is a WRED (or IRED, or RED) between $E_i$ and $E_i'$.

The condition *b)* is guaranteed by requiring that $\langle I, I'\rangle$ be a member of $\bar{\varrho}^*$ and that for every $x$ member of $I$ there exists a $\varrho^*$-image $x'$ in $I'$ such that both have enrichments in $Mp$ and $Mp'$ respectively[9]. Conditions *a)* and *b)* appear together in the definition of the reduction relations WRED, IRED and RED for theories.

## III - CRITICISMS

I shall present now some of the criticisms which Stegmüller's book has drawn. Some refinements or outright changements of the NSV posterior to SDT have been summarized by Stegmüller in his subsequent SVT[10], and they seem to answer part of those criticisms.

A number of criticisms is advanced by Feyerabend in his *Changing Patterns of Reconstruction*[11].

A – *Kuhn-NSV relation*. Stegmüller derives ans argument for the NSV from the fact that it allows to appreciate the rationality of Kuhn's picture of science; this argument is proven to be invalid by two kinds of considerations:

1) Kuhn's ideas are largely, or far a significant part, wrong: for example, normal science in Kuhn's sense just doesn't exist for Feyerabend.

2) Those of Kuhn's ideas which reflect real science can be captured by the SV just as by the NSV:

2.1) On the topic of immunity of theories to rejection even for Stegmüller such immunity is not complete: there is a possibility of rejection, even if not of falsification. On the other side, even the SV can account for the resistance of theories, which makes rejection difficult and rare: such an account is given for example through Lakatos' 'research programmes'.

2.2) The separability of theory and belief can be accounted for «by showing how the theory remains constant, while its probability changes in different conditions».

2.3) Nor does the introduction of Sneed's concept of theoreticity force to abandon the SV. It certainly gives rise to rather complex structures, but there is no reason why the concept of theory should be tied to these structures rather than to the statements about them[12]. In other words, the theory might be identified with the theory sentence '$I$ belongs to $A(E)$' instead than with the pair $\langle I, E\rangle$.

B – The NSV has plausibility problems of its own, and the SV correspondingly seems to have some advantages:

1) There are in scientific practice cases in which the core is modified, but the paradigm left intact; early quantum theory is an example of this, for Feyerabend. Stegmüller's view cannot deal with this, while the SV can: for example, if the core is conceived as a cluster of statements,

characterized by a wittgensteinian predicate. In this way, few statements can be added or dropped, thereby changing the composition but not the identity of the core.

2) The application of the theory does not always involve the entire structure: this instead is required by the NSV (but not by the SV).

Thomas Kuhn also, in his *Theory Change as Structure Change*[13], has some criticisms, concerning the identification of cores, theories, paradigms:

3) He suggests that constraints might be given more weight, introducing as primitive just applications and constraints among them (instead of first non theoretical, then theoretical functions and finally constraints), and watching to which extent the successive research will determine the theory identity and the demarcation between theoretical and non theoretical concepts. For example, classical mechanics and the electromagnetic theory, which seem two distinct theories, might be just one theory, with some common applications and many different ones. Whenever in fact they appear together, neither one is primitive or presupposed by the other.

Equally, classical particle mechanics might be divided into statics and dynamics, so that the force function would be theoretical only within statics, whence it entered dynamics *via* constraints. Considering some functions theoretical in given applications and non theoretical in others would not be a problem, once the presupposition of a neutral observative language is abandoned. Moreover,

4) a clearer distinction between core and expansion is needed. Lacking a precise demarcation, there is risk of circularity between the definition of theory identity through core invariance and the definition of the core as invariant with respect to the theory. For example, we think of Newton's second law as belonging to the core; but what is it of the third?

C – A more general criticism is brought by Feyerabend to the enterprise itself of reconstructing theories by one overall frame, or model, regarded as exhaustive and exclusive. Just the fact that Stegmüller's approach fails in some cases, while faring well in others, should teach that the plurality of models of reconstruction is better.

Reconstruction for its own sake may be good, but is not sufficient: as in Aristotle's example, philosophy of science should also add to our knowledge of nature; now, the reconstruction enterprise so far has proven barren. Moreover, the monopoly of one scheme of reconstruction shows the persistence at the level of metascience of those ideals of absoluteness, provability and universality that have already become outadated in science itself.

Finally, the method of science is determined by its object and since science is a human enterprise, the 'science of science'[14] should have, if any, the method of human sciences (history, sociology, etc.).

D – Incommensurability, the one topic on which Stegmüller criticizes Kuhn, is one of those on which he himself has been widely criticized. On this point too I shall introduce criticism by various authors.

1) First, Feyerabend points out that there are two distinct concepts of incommensurability: Kuhn's and his own.

*a)* Kuhn's concept consists of three factors: (roughly) i) conceptual incommensurability; ii) world incommensurability; iii) methods incommensurability. Stegmüller registers only the first one, and this is all his reduction relation is intended to provide for; therefore his claim to have solved the problem of incommensurability, or 'filled the rationality gap' is unjustified.

*b)* Feyerabend's incommensurability concerns only concepts. In any case, for Feyerabend incommensurability means only deductive disjointness, and nothing else. In particular, not incomparability. That we cannot establish deductive relations between theories implies that we cannot compare them with respect to 'content', or 'verisimilitude'. But there are other criteria of comparison: formal criteria (e.g., a linear theory is preferable to a non linear one) and non formal criteria (e.g., conformity with basic accepted theories, or metaphysical principles). To Feyerabend, therefore, simply there is no rationality gap to be filled.

2) Stegmüller accuses Kuhn and Feyerabend a 'logical mistake', because they use the 'metatheoretic paradigm' of their opponents, i.e. the SV, to argue for the incommensurability thesis[15]. Stegmüller thus seems to imply that holding a paradigm conception is logically incompatible with the SV; but this, Feyerabend remarks, is obviously not the case.

3) The RED relation, according to Feyer-





abend, makes theories comparable, not rival. Obviously, reduction would be of no interest to the end of progress and rationality if we weren't even able to establish that reduced and reducing theories are about the same entities. Now, as it is, RED might be used for the 'reduction' of (e.g.) electrostatic to general hydrodynamical theory. The same objection is raised by Tuomela[16]. If it is correct, RED does not, of course, guarantee that $T'$ predicts, explains, etc. at least the same facts as $T$. That claim is based on the implicit presupposition that the correspondent empirical objects of the two theories (i.e. the $\varrho^*$-correspondent objects) are the same real world physical systems. But if we admit meaning variance, there is no way of showing that this is the case.

RED does not even ensure that $T'$ accounts for at least as many facts as $T$. It only ensures that the facts for which $T'$ accounts are, from the point of view of $T'$ itself, as many as those $T$ accounts for are from the point of view of $T$ itself. In fact, the extension of the subject and the predicate in the two theory sentences are measured, to the effects of RED, with respect to the respective partial possible models. But in case of paradigm shift what counts as a fact, or as a relevant fact, for one theory, might not count for another. Therefore there is no guarantee that $T'$ accomplishes as much as $T$ from the point of view either of $T$, or $T'$, or some other theory, or of 'objective reality' itself. In conclusion, it seems that the linguistic factor is involved in the search of an interesting reduction relation, in spite of Sneed's and Stegmüller's hopes.

4) A closely related criticism is brought by Kuhn[17]: the relation from *Mpp* into *Mpp'* that RED presupposes may not exist, or be impossible to find in case of paradigm shift. An example might be the inexistence of such a relation between the objects of chemistry in the 18th and 19th century; basic vocabularies are so different that one couldn't identify an 18th century substance in 19th century terms, ans *vice versa*.

Actually, Stegmüller's point was just that one could do without such identification: any relation matching randomly members of *Mpp* and *Mpp'*, if just fulfilling the formal condition of RED, would be a reduction. Kuhn's objection should be read then as the claim that this is not the case, that such a purely formal relation is not interesting for the task of rational theory choice.

5) We may also notice that RED is a very strong relation: there may be a lot of cases in which it fails to obtain either way, so that even if it did ensure the 'at least the same accomplishment relation' we would be left without a 'rational' way to choose. It may be that weaker relations obtain, like WRED or IRED: but would they be enough for a choice? Also, it may turn out that one theory sentence has a stronger predicate, but the other a stronger subject. It may be that most of the expansions of a core $K$ can be reduced to those of core $K'$, but for the rest of them the relation obtains in the opposite sense. It may be that all of them are reducible, but not all by the same relation $\varrho$; etc.

There are probably ways of comparing theories as to their content even when RED (or WRED, or IRED) does not obtain: requiring that the new theory duplicates 'all' of the achievements of the superseded theory is, according to Feyerabend, an excessive rationalism; it might be rational to choose a theory over another if it accomplishes more, even if not all the same things. But even if RED did ensure the 'as much accomplishment relation', there would remain many cases of theories accomplishing as much as a superseded theory not covered by RED.

6) As a final remark, a choice based on RED is neither definitive nor rationally inevitable. New developments of a theory $T$ reduced to $T'$ can after a while yield an expansion and a set of intended application which can in turn reduce $T'$. In such cases, holding $T$ in spite of its *prima facie* reduction to $T'$ would prove *a posteriori* to be best move. *A priori*, therefore it was not necessarily irrational.

E – Some important criticisms are advanced in Tuomela's *On the Structuralist Approach to the Dynamics of Theories*[18].

1) Concerning the clamis of superiority of the NSV over the SV, Tuomela observes that since the set-theoretic predicates of Sneed's formalism are defined on the basis of axioms, the two approaches are equivalent. To be sure, the axioms are typically expressed in informal set-theoretic language, and Tuomela acknowledges that there are serious problems in translating them into the first order predicate logic of the traditional approach (this can be

done at the price of tolerating non-standard models); however, he points out that, given any bunch of structures, we may define a language to speak about those structures. Now, it is cheap for Stegmüller to claim that his notion of theory is a different sort of entity, since he just calls 'theory' the structures instead of the axioms, which are what in his treatment corresponds to the ordinary notion. It is, as we can see, the same kind of objection as Feyerabend's A.2.3).

2) Next Tuomela proceeds to show that not only we can, but also need to define a language, on top of the structures. It is needed, among other reasons, in order to distinguish the semantic from the empirical content of theories; to offer a better account of the meaning of theoretical terms; in connection with the problem of reduction, as it was noticed above D.3); in order to define $I_o$, which on Tuomela's opinion cannot always be given extensionally; for a better account of the laws of nature, of explanation, of Ramsey-eliminability.

3) I noticed above that the NSV does not impose, but neither prevent, an instrumentalistic interpretation of theoretical claims. Tuomela has a number of comments intended to show that at any rate it is titled on the instrumentalistic side. On his view, theories should be conceived as about theoretical, rather than non theoretical entities; therefore, instead of claiming that a given set of partial possible models can be enriched to a set of models, one should claim that a given set of models finds a 'non theoretical enrichment', i.e. is identifiable with given empirical structures. The ontological content of the theory would thus be independent from its applications.

4) Other criticisms are concerned with the adequacy and precision of the rendering in Stegmüller's formalism of Kuhn's views on the one side and of actual scientific practice on the other.

## IV - NEW DEVELOPMENTS

SVT contains a number of considerations already found in SDT, new considerations in defence of the NSV and some important modifications of it. Moreover, it presents a modification of Sneed's reconstruction of theories, proposed by W. Balzer: each special law of a theory, together with its constraint, is now viewed as a core specialization $K'$ of the core $K$. A theory specialization of a theory $<K,I>$ is a pair $<K', I'>$, where $K'$ is the core specialization, $I'$ its particular set of intended applications, and where $M'$ is a subset of $M$, $C'$ of $C$ and $I'$ of $I$. If they are 'proper' subsets then $<K', I'>$ is a 'proper' specialization of $<K, I>$[19]. Both the original pair $<K, I>$ and each theory specialization are called 'theory elements', and the system of all the theory elements connected to an original element forms a theory-net $N$. The net of core specializations, or core-net $N^*$, is now the equivalent of the old expanded core $E$, and the empirical claim, corresponding to '$I \varepsilon A(E)$', is now '$I \varepsilon A(N^*)$'. A net $N'$ obtained from a net $N$ by one or more specializations is called a 'refinement' of $N$, and a 'proper refinement' if at least one of those specializations is a proper specialization.

Reacting to many of his critics, in SVT Stegmüller takes a more modest approach to the possibilities of philosophy of science: general philosophy of science cannot give but necessary conditions for metascientific concepts; accordingly, all the 'iff' in the definitions in SDT have now to be read as 'only if'.

Only in special philosophy of science, because of its more limited scope, more complete reconstructions are possible. Even there, science involves a number of pragmatic factors, which are not amenable to logical reconstruction. This might represent a compromising answer to Feyerabend's general objection C., i.e. the insufficiency of one only form of reconstruction.

Further, Stegmüller accurately defines the scope and nature of his claims of superiority of the NSV, by distinguishing three senses in which it may be intended: $NSV_1$, $NSV_2$, $NSV_3$. First, he agrees that there is no superiority 'in principle' of the NSV over the SV intended as informal and formal set-theoretic axiomatization ($NSV_1$, and $SV_1$) respectively: his claim is rather that the latter has proven practically unfeasible (a claim which his critics have not really challenged).

He also insists that, given the need of informal set-theoretic axiomatization ($NSV_1$) and of representing the empirical content of the theory as one global claim ($NSV_2$), theories cannot be conceived as classes of statements. However, for particular purposes one could choose (in general philosophy of science) to talk of theories as if they were classes of statements (and this would be the $SV_3$). The inferiority of such approach to the talk of theories as structure ($NSV_3$) is again a practical, and particularly psychological matter: it gives an image of theories too simple, even seriously misleading; an image which does not allow a proper insight of aspects as the nature and role of theoretical terms, the uniqueness of the empirical claim, the distinction between laws and constraints, scientific change and various pragmatical aspects of theories[20].

It is possible in my opinion to accomodate such claims with Tuomela's arguments for the necessity of supplementing the NSV with a language. Thus, even without indulging in any essentialism for what concerns the nature of theories, one might conclude that really it is more informative, coherent with our linguistic use and conceptual patterns, etc., to identify theories with non linguistic entities, rather than with their linguistic formulation; acknowledging at the same time that there are important aspects of science which cannot be properly understood except by reference to the linguistic formulations of theories[21].

Concerning the relation of the NSV to Kuhn's theses, Stegmüller stresses that they are reciprocally fully independent. The NSV shows the inadequacy of epistemological 'falsifications' of Kuhn's theses; however, they might find a historical confutation, and this is not excluded by the NSV. In this way he makes sure that Feyerabend's point A.1) does not represent an objection. The new structure of theory nets also provides a more accurate picture of actual science: it seems to allow for applications of the theory which do not involve the whole structure (thus answering objection B.2), and for the existence of mini-revolutions, as those Toulmin has claimed to exist[22]; Kuhn's request for a clearer distinction between core and expansion B.4) is also answered: in the new picture there is no more a center-periphery (core-expansion) dychotomy, but a more or less central (peripheral) relation, with respect to which each element is clearly defined.

New pragmatic notions are defined on the basis of the new formalism: a 'pragmatically enriched' (= p.e.) theory element is a quadruple $<K, I, SC, h>$, where $h$ is a period of time in which the scientific community SC intends to apply $K$ to $I$. Along the same lines is defined the notion of a p.e. theory-net; and a temporal sequence of theory-nets such that each is a proper refinement of the preceding is called a 'theory evolution'.

Given a set $I$ of intended applications, a subset $I'$ of $I$ is called 'secure' when there is unanimity in SC that it is a well confirmed application of $K$. A theory evolution will be 'progressive' whenever the range of secure applications increases from one net to the following[23]. Given a theory evolution $E$ a paradigm can now be viewed as the theory element $<K_o, I_o>$ such that for any theory element $<K_i, I_i>$ in $E$, $K_i$ is a specialization of $K_o$ and $I_i$ has as a paradigmatic set a subset of $I_o$[24].

This notion of paradigm is much more flexible of the previous one: it has dropped the requirement that there be a 'founder' of the paradigm; $K_o$ needs not to be the first core, and $I_o$ needs not to be the paradigmatic set for all sets of intended applications. Thus, it provides an answer to Feyerabend's objection B.1), since in this way the core-net can be modified, and even the original changed, without affecting the paradigm.

In spite of these improvements, Stegmüller acknowledges that the notions introduced are just a first step of «a transition from a mathematical skeleton of a possible theory to a real empirical theory»[25]. In this transition any general systematic investigation will always be left behind by historically and pragmatically oriented descriptions, like Kuhn's[26]. He grants thus, at least partially, Feyerabend's objection D.la) and Tuomela's E.4).

Stegmüller by now has also come to believe that paradigms, as reconstructed within the NSV, are completely immune to rejection by experimental results (contrary to what he previously thought). He thinks that the claim based on the fundamental law, appearing in all claims made through the theory, necessarily maintains on 'open' character, which makes it unamenable to direct empirical test, although not analytic[27]. If so, the NSV steps closer to a complete vindication fo Kuhn's ideas, while further differentiating from the SV: this would be a partial answer to Feyerabend's objection A.2.1).

With respect to the problem of incommensurability Stegmüller at the outset recognizes that it is more complex than he previously thought. The hope of solving the whole problems was indeed excessive, and the goal is now just to enlighten an aspect of it: what is scientific progress in case of paradigm shift. This is a decisive task, since if it fails the cause of rationality in science is lost. It is, also, an essentially logical task; however, it leaves completely open whether such progress exists, in general and in each particular case, and whether its existence may be ascertained, and in which cases. These problems need much further work in special philosophy of science, history and pragmatics. Even the simple explication of progress, within general philosophy of science, cannot go beyond giving necessary conditions. All this amounts to granting a number of the criticism raised against the NSV on these topics.

Also on the very topic of the reduction relation Stegmüller grants crucial objections [D.3), D. 1*a*) ii), D.4)]: he admits that it was a naive view of the partial possible models which made possible to believe that the appropriate relation from *Mpp* into *Mpp'* could be found without problems. Actually, in the case of paradigm shift, the partial possible models will be different, and an appropriate relation impossible to find. This happens because in a theory hierarchy the non theoretical objects of a theory at level $n$ turn out to be the theoretical objects of theories at level $n - 1$, and different paradigms may involve different theories at each lowers level.

If this is the case, however, the problem becomes just that of establishing a reduction relation on the lower level, and further down the hierarchy until a decision on non theoretical objects is reached. Stegmüller reverses now his contention in SDT, and claims that it is possible to descend the hierarchy right down to a ground theory, which has 'real' objects as non theoretical objects; studies by Sneed have shown that we get from particle machanics to 'real' objects through three theoretical levels: physical geometry, topology, mereology. Thus, even if two paradigms had different lower level theories down to the ground level, at that level a reduction relation might be found, since non theoretical objetcs are the same for both (the 'real' objects). Deciding about lower level theories in turn would allow to decide about the higher level theories based on them. This new more sophisticated way of looking at the partial possible models would still therefore allow the explanation of the notion of progress. What is claimed here is that «two *prima facie* incommensurable theories can be traced to a common 'observational' basis». If this claim is granted, there is still the question whether it solves completely the problem of incommensurability. Even if the observational basis is the same, if the languages in which the two theories are formulated are incommensurable wont'we ignore again whether an established RED reduction is a same object relation or not? In that case RED would probably ensure that $T'$ accounts for 'as many' facts, but not 'at least the same' facts as $T$.

Finally, Stegmüller thinks that more qualifications have to be made to his previous ideas about progress. First, the possibility of an approximate reduction should be allowed. Second, progress admits of 'branchings', i.e. occasions in which two form of progress (two possible refinements of the core-net $N^*$, or enlargements of a set of intended applications) are alternative. In these cases a logical comparation, even if carried out, does not decide a choice; in fact, even if alternative 1 is 'less progressive' than alternative 2, in the future it might yield refinements which in turn are 'more progressive' than those alternative 2 will yield. The decision is definitely left to a 'practical rationality' which goes beyond the mere reasons of logic. Since this problem concerns net refinements as well as paradigm choices, Stegmüller admission amounts to granting objection D.6) and, on the other hand, Kuhn's and Feyerabend's claim that not all not logical methods used in paradigm choice are irrational.

Summing up, not all the objections reported here have been considered in SVT, aimed principally at answering Feyerabend's criticisms. Stegmüller however has met most of the objections considered, either by answering them or, often, by softening his position. The end result is somewhat of a compromise, and certainly a more accurate picture of science.

*M.A.*

Translation: I. McGilvray - Roma

*I am thankful to Professor F. Suppe, of the University of Maryland, for his valuable guidance in studying the structure of scientific theories.*



## BIBLIOGRAPHY AND NOTES

1 See *The Logical Structure of Mathematical Physics*, Dordrecht, 1971; moreover, *Philosophical Problems in the Empirical Science of Science: A Formal Approach*, Erkenntnis, 10 (1976); *Invariance Principles and Theoretization*, Acta Philosophica Fennica, 1979.

2 *The Structure and Dynamics of Theories*, New-York-Heidelberg-Berlin, 1976 (1st edition Berlin, 1973).

3 See T. KUHN, *The Structure of Scientific Revolutions*, 1st ed. Chicago, 1962.

4 More exactly, one of them.

5 I will use frome here on this abbreviation NSV for 'Non Statement View', and 'SV' for 'Statement View'.

6 Criticisms to this effect are advanced by Putnam, in *What Theories are not*, in NAGEL, SUPPES, TARSKI (eds), *Logic, Methodology and Philosophy of Science: Proceedings of the 1960 International Congress*, Stanford (Ca.), 1962; and ACHINSTEIN in *The Problem of Theoretical Terms*, American Philosophical Quarterly, 2 (1965).

7 This possibility, which ill fits Kuhn's ideas, is later rejected by Stegmüller.

8 *SDT*, p. 208.

9 Stegmüller has '$\bar{\varrho}$' and '$\varrho$', but it does not seem to make sense, since '$\varrho$' is between *Mp* and *Mp'*, while *I* is a subset of *Mpp* and *I'* of *Mpp'*.

10 *The Structuralist View of Theories*, New York-Heidelberg-Berlin, 1979.

11 In The British Journal for Philosophy of Science, *28* (1979).

12 See p. 360.

13 In Erkenntnis, *10* (1976).

14 In Sneed's terminology: see note 1.

15 See *SDT*, p. 20 and pp. 215-216.

16 In *On the Structuralist Approach to the Dynamics of Theories*, Synthese, *39* (1979), p. 220.

17 In *Theory Change as Structure Change*, in Erkenntnis, *10*, No. 2 (1976).

18 In Synthese, *39* (1979).

19 That *Mpp'* is a subset of *Mpp* is omitted because of the method of selfdetermination of applications.

20 See *SVT*, pp. 45-49.

21 Such for example is the position of F. SUPPE: see e.g. *Theories, Their Formulations and the Operational Imperative*, in Synthese, *25* (1972).

22 See *Does the Distinction between Normal and Revolutionary Science Hold Water?* in LAKATOS and MUSGRAVE (eds), *Criticism and the Growth of Knowledge*, Cambridge, 1970, pp. 25-38.

23 It would in this way fulfill Lakatos' criterion of progressivity for a research programme: increased confirmed empirical content. In fact by definition a refinement of the laws is a condition for the existence of a theory evolution. See *Falsification and the Methodology of Scientific Research Programmes*, in *Criticism and the Growth of Knowledge*, cit., pp. 91-196.

24 See *SVT*, p. 56. This definition is by U. Moulines.

25 *SVT*, p. 27.

26 *SVT*, p. 57.

27 *SVT*, p. 53.