INCOMMENSURABILITY IN THE STRUCTURALIST VIEW

Walter Van der Veken

In his 1969 Postscript Kuhn complains for being misunderstood by his criticists. He wrote "I have argued that the parties to such debates [on theory choice] inevitably see differently certain of the experimental or observational situations to which both have recourse. Since the vocabularies in which they discuss such situations consist, however, predominantly of the same terms, they must be attaching some of those terms to nature differently, and their communication is inevitably only partial. As a result, the superiority of one theory to another is something that cannot be proved in the debate. Instead, I have insisted, each party must try by persuasion to convert the other" (1). Kuhn sees mainly two difficulties, to be solved by "persuasion". First, if two researchers understand each other, i.e. when they belong to the same normal scientific tradition, they may apply shared values -- as fruitfulness, accuracy, simplicity, etc. — differently because they don't have the disposal of a neutral algorithm for theory choice, or of a systematic decision procedure that leads for each individual to the same decision. As a consequence it is not the individual scientist but the scientific community that takes the decisions. Secondly, a more important difficulty arises when they speak from "incommensurable" viewpoints. In this case the scientists cannot ressort to a neutral language that is adequate to state both their theories. In such a situation of "communication breakdown" the only thing they can do is to translate each other's standpoint in their own language, but this translation remains always imperfect. They can therefore only try to persuade each other, a process that leads if successfull to a gestalt switch by one of the two parties.

Conclusion, if we can agree on the fact that persuasion intrinsically is an irrational process, scientific change whether normal or revolutionary, proceeds along "irrational" lines.

A few pages further on Kuhn says "Later scientific theories are better than earlier ones for solving puzzles in the often quite different environments to which they are applied. That is not a relativist's position, and it displays the sense in which I am a convinced believer in scientific progress" (2). But this is at least a weak form of contradiction, if a theory A is better — whatever that means — than a theory B, there must exist criteria to compare A and B. When we leave "being true" or "being less false" out of scope, and we may very well do so because neither is involved in Kuhn's translation problem, i.e. incommensurability problem, the criteria to compare A and B can be used as commensurability standards. At first sight, two possibilities are open, either progress doesn't mean what it means,or the notion "incommensurability" — and perhaps the distinction normal vs. revolutionary scientific change — must be re-examined. But it is not that easy. It is sometimes very hard to interprete Kuhn correctly, and to appreciate his intentions.

For nearly fifteen years one particular approach to theory change, that concentrated on the views of Kuhn (and also Feyerabend), has been elaborated and provided us with and extensive and detailed formal framework. It is this view, the Sneed-Stegmüller approach, in which we will examine the problem of theory change and incommensurability, first of all because it gives the possibility of depicting in a fairly adequate way Kuhn's view, and secondly because it is very promising, notwithstanding it needs a lot of improvement, as a tool for future investigations on theory change, pragmatics of science, and so on.

In part I we will sketch in a very brief outline the most important concepts of the framework necessary for a good understanding, we also will try to give a short overview of recent developments in the structuralist approach — readers acquainted with the structuralist view can of course skip this part.

In part II we will examine the various concepts that were introduced to describe the different modes of theory change. In a third part we will concentrate on the problem of incommensurability. In the final part we will try to state some open problems and to indicate some tentative directions for further research.

Part I : Introduction

When Sneed, in 1971, published his *The Logical Structure of*

Mathematical Physics it was his intention "[to make] simply an elaboration of the rough characterisation of theories of mathematical physics" (3), an although he was aware of the possibility of applying his characterization to Kuhn's view (or vice versa), it was only in Stegmüller's (1973) book that Sneed's model was extensively applied to this view. In Sneed's (1971) book a lot of questions remained unanswered, it is interesting, however, to look at the most central structure which is unaltered until today. For an historical background of the rise of the Sneed-Stegmüller program see, e.g., I. Niiniluoto (1981).

The most central idea of Sneed was to describe theories as set-theoretic structures. In the same way as "P is a group" he wanted to formulate "P is a non-relativistic particle-mechanics", etc. This method was building upon the views of Adams, Suppes, and others. Only Sneed's view differs from earlier attempts to the point that he introduced constraints and an account of theoreticity. His view also differs from the so-called statement view that a scientific theory is not merely, a statement or a class of statements, but that it consists of a fundamental structure (a core) and of a set of intended interpretations/applications. It is on this twofold structure that all further elaboration is based.

In order to avoid the confusion of different notations of Sneed (1971) and Stegmüller (1973) we will try to use as much as possible the notation of Balzer and Sneed (1977) and (1978).

Cores together with their application form *theory-elements.* These theory-elements constitute a theory and are used to express empirical claims. A theory may consist of one or more theory-elements. Let M_p be a m+k - matrix consisting of m+k - tuples of the form $<n_1 \ldots n_m, t_1 \ldots t_k>$ ($m < 0$, $k \leqslant 0$), n_i and t_j are sets, relations or functions; the n_i are the *non-theoretical* components and the t_i are the *theoretical* components. M_{pp} is a m+0 - matrix obtained from M_p by lopping-off the theoretical components of M_p. This can be done by introducing a restriction function $r : M_p \rightarrow M_{pp}$, $r(n_1 \ldots n_m, t_1 \ldots t_k) =_{df} <n_1 \ldots n_m>$. M_p and M_{pp} are called respectively the set of all *potential models* and the set of all *partial potential models.* Be $M \subseteq M_p$, then $F = <M_p, M_{pp}, r, M>$ is called a *frame.* $K = <F,C>$ a *core* and C a *constraint* for M_p. A constraint is a set of restrictions that rule out certain combinations of components in different potential models and such that (i) $C \subseteq Pot(M_p)$ (the powerset of M_p), (ii) $\emptyset \notin C$, (iii) if $x \in M_p$ then $\{x\} \in C$, (iv) if X and $Y \in Pot(M)$, $X \neq 0$ and $Y \neq 0$, $X \in C$ and $Y \subseteq X$, then $Y \in C$. T is a *theory-element* if there exist a core K and a set I, $I \subseteq M_{pp}$, such that $T = <K,I>$. Though the intuitive content of the definitions above is clear, they seem general for several authors. Niiniluoto (1981) summarizes the required improvements to the notion of a core. An other important point is the distinction between theoretical and non-theoretical terms (see also Niiniluoto 1981).

Sneed and Balzer consider three different relations among theory-elements, sufficient to express all other intertheoretical relations of some interest. These three relations are : theoretization, specialization and reduction.

Theoretization consists of adducing new theoretical components to the matrix of T. *Specialization* of e.g. a core K is intuitively spoken the assignment of certain special laws to some subset of M_{pp}. Formally, if T' and T are theory-elements then T' is a specialization of T iff (i) $M'_{pp} \subseteq M_{pp}$ (M'_{pp} is a non-empty set), (ii) $Pot(M'_{pp}) \cap A(K) \neq \emptyset$ (A(K) is the class of the sets of the possible non-theoretical applications of core K), (iii) $M'_p = \{x | x \in M_p$ and $r(x) \in M'_{pp}\}$, (iv) $M' \subseteq M$, (v) $C' \subseteq C$, (vi) $I' = I \cap M'_{pp}$.

The third relation, the *reduction relation*, says that every application of the reduced theory corresponds to at least one application of the reducing theory and everything the reduced theory says about a given application is entailed by what the reducing theory says about the corresponding application. One can make a distinction between weak and strong reduction, intuitively weak reduction only requires "translation" between the non-theoretical concepts of the theories in question, while strong reduction requires "translation" between the theoretical concepts as well.

II. Theory-dynamics.

Within the structuralist account, several proposals were made to describe theory-dynamics. We try to summarize these proposals in a chronological order. In Sneed (1971) a first approach to describe theory-change was made by way of defining "person p has a theory at time t". When several persons have a same theory at time t, they belong to the same scientific tradition. Another approach consists in employing theory-nets relativized to a scientific community SC at time t (e.g. Moulines (1979)).

People that have a theory share two things (i) a core mathematical formalism, (ii) a commitment to use that formalism in dealing with the same class of physical systems. In addition, they share the same "starting point" in attempting to use the core formalism as much as possible within the characteristic range of intended applications. (Sneed pp. 249 ff.) In Kuhn's terminology, we might say that this account is that of "normal" scientific practice.

Much less formally elaborated, Sneed discusses two cases of "revolutionary" scientific theory-change. First, the case where the theory of mathematical physics is dealing with phenomena which had never before been dealt with by any theory that might properly be called a theory of mathematical physics (this distinction is made by way of lacking theoretical functions). Secondly, the case where a theory replaces another theory of mathematical physics (e.g. relativity and quantum mechanics). But how are theoretical functions discovered in the first case ? "There is one pat answer to all these questions : we simply pick theoretical functions, constrain their values and relate them to non-theoretical functions in any way that works. That is, the only important aim in constructing the theory is that claims made with the theory are true and, perhaps, that the formalism is as simple as possible ... [Thirdly] [t]he theory must, in some way, allow us to "understand" better the phenomena it accounts for." (4) The second case, where a theory replaces an old one, is explained by Sneed in the following way. By discovering new data about the paradigm set of intended applications one could abandon the claim of the, so far, "successful" application of the core. Or, the theory should be given up, especially when a new theory, that seems better to handle the problem, is at hand, or, when no such theory is available, a more conservative position may be choosen because there are parts of the theory that remain successful. But in which way does the new theory and its predecessor relate to each other ? Sneed is not sure that the following must be true in general, — he maintains that it holds for classical particle mechanics and special relativity theory as well as for "classical" special relativity and relativistic quantum mechanics —, in that the new theory must be such that the old one reduces to (a special case of) the new theory. The reduction requirement thus seems crucial in Sneed's approach of revolutionary theory-change, but he did not elaborate further on this subject in his (1971) book.

Stegmüller was the first author within the structuralist tradition to concentrate upon this question. In his (1973) *Theorienstrukturen und Theoriendynamik* he introduced the concept of theory dislodgement. He takes a particular theory to consist of two important items, first I_o (the set of paradigmatic examples) and K_b (the basic core of the theory) that identify a theory, and, secondly, those parts of a theory-net which may change while the theory remains constant (such as specializations of K_b, special constraints, etc.). The elements of the first kind form the *essentials* of a theory, that of the second kind the *accidentals*. Accidental changes occur in normal-science periods. Revolutionary changes are changes of the essentials of a theory. This supplanting of a theory by a new theory is called *theory-dislodgement*. In order to fill in Kuhn's view completely, Stegmüller calls a scientific revolution progressive if the displaced theory can be partially and approximatively imbedded into the supplanting theory. He uses the concept of reduction to formalize this imbedding. Reduction of theories can be interpreted in a very broad sense, reducing and reduced theories may have completely different theoretical superstructures (a different apparatus, a different scientific language, they may be even incommensurable in Kuhn's sense), the only presupposition to be made it at the non-theoretical level, i.e. at the level of the partial potential models, is that of comparability.

In Balzer and Sneed (1977) a technical framework is given for describing the logical structure of empirical science, it is a more elegant integration of both Sneed (1971) and Stegmüller (1973). Thus far the treatment of special laws was done by a procedure that consisted in the construction of so-called expanded cores. As Stegmüller himself granted on the disadvantages "the main theoretical shortcoming was that laws and constraints could not be analysed separately, because they were lumped together into the two big classes L and C_L respectively. Therefore, for example, no systematic distinction could be made between laws of different degrees of generality. A great practical disadvantage was the clumsiness of the 'application function' which was needed in order to formulate empirical claims." (5) In Balzer and Sneed (1977) a new procedure was developed. The original "theory" became "theory-element" (no further distinction is made between theories and laws), and the notion of theory-nets was introduced to replace core-expansions. The intuitive idea behind the introduction of theory-nets is to grasp the hierarchical structure of a theory, parts of a theory are built upon other parts, some are 'deeper' than others. Three features are inherent in the hierarchical relation : (i) the relation is transitive, (ii) there are no loops in an hierarchical sequence, (iii) every part of a theory is more general than itself. The definition of a net becomes: X is a *net* iff there exist N, $\leqslant$, $\sim$, such that (i) X = $<$ N, $\leqslant$, $\sim$$>$,

(ii) N is an non-empty and finite set, (iii) $\leqslant$ and $\sim \subseteq (N \times N)$ such that for all x, y, z, $\in$ N (a), $x \leqslant x$, (b) $x \sim y$ iff $x \leqslant y$ and $y \leqslant x$, (c) if $x \leqslant y$ and $y \leqslant z$, then $x \leqslant z$. X is a *theory-net* iff there exist N, $\leqslant$, $\sim$, such that (i) $X = \langle N, \leqslant, \sim \rangle$ is a net, (ii) for all $x \in N$: x is a theory element, (iii) for all $\langle K,I \rangle$ and $\langle K',I' \rangle \in N$: if I = I' then K = K'. We can not elaborate further on the technical formalization of all applications of the theory-net concept and of the reduction notion here. We refer the interested reader to Balzer and Sneed (1977) and (1978).

Moulines (1979) suggested to dynamize theory-nets. He first introduces some pragmatic concepts in order to modify the Balzer-Sneed notion of theory-nets. A relation "... *is historically previous to* ... ", H, and the concept of a "*scientific community*" SC_i, a group of people that communicate with each other in a specific scientific language and that share particular measurement techniques and observational and calculating procedures for testing hypotheses, may both be fuzzy objects. Two other concepts are the "*acknowledged paradigm set*" and the epistemic relationship "*SC_i admits proposition p*" (the latter means that most members of SC_i consider p as a proposition well-confirmed and corroborated by testing procedures typically used by SC_i). The notion of theory element is then modified as : T is a theory-element only if there exist K,I,SC and h such that (i) $T = \langle K,I,SC,H \rangle$, (ii) K is a theory element-core, (iii) $I \subseteq Pot(M_{pp})$, (iv) SC is a scientific community, (v) h is a historical interval, (vi) SC intends to apply K to I during h. Introducing N is a "tree-like theory-net" iff (i) N is a theory-net, (ii) there is a $T_o \in N$ such that for all $T_i \in N$ T_i is a theory-specialization of T_o, he defines E is a "theory-evolution" iff E is a finite sequence to theory-nets $\{N_i\}$ such that for any two N_i, N_{i+1} belonging to $\{N_i\}$: (i) N_{i+1} immediately follows N_i, (ii) for every $T_{i+1} \in N_{i+1}$ there is a $T_i \in N_i$ such that T_{i+1} is a theory specialization of T_i.

Another distinction introduced by Moulines is that of the epistemic relationships of SC_i towards the intended applications of its theory-nets. Some applications of I will be admitted by SC_i during h as well-confirmed applications of K. Moulines calls this secured subset the "firm domain of applications", F_i. E is then called a progressive theory-evolution iff : (i) E is a theory-evolution, (ii) for every pair N_i, N_j in E, if i<j, then $F_{I(N_i)} \subsetneq F_{I(N_j)}$. This concept reminds one of Lakatos' "progressive research program".

Niiniluoto (1981) makes some interesting comments on the subject of theory-change, that go even much further than Moulines, e.g. that normal scientific theory is much more dynamic than is allowed within the structuralist framework, and that the dichotomy between normal science and scientific revolutions should perhaps be rejected and replaced by a degree of radicality of theory-change.

To close this section it may be interesting to look at Kuhn's reaction on the structuralist interpretation of his theory. In general, I think we can say his reaction is rather enthusiastic, although he sees, beside a lot of advantages, some difficulties. As a formal representation of scientific theories it provides us with a primary technique for exploring and clarifying ideas and opens fruitfull perspectives on interdisciplinary communication. But the importance of this advantages is "dwarfed" by an other aspect, namely the "circular" representation and explication of scientific revolutions. The concept of reduction does not solve the problems arising when theories ought to be compared. "[T]he problem of comparing theories becomes in part a problem of translation, and my attitude towards it may be briefly indicated by reference to the related position developed by Quine in "Word and Object" ... Reference and translation are two problems, not one, and the two will not be resolved together." (6). We will return to this subject in the next section.

Part III. Incommensurability.

In this section we will give the definition of incommensurability of W. Balzer, and dwell upon the discussion of Stegmüller vs. Kuhn and Feyerabend.

W. Balzer treats extensively the problem of incommensurability in his (1976) paper from two examples. Some conventions are stated by :

Let T be a theory.

a) x is a paradigm intended application of T iff $x \in I_p$.

b) if $y = \langle z, \dots x_n \rangle$ is a (partial) potential model of T then Ob (y) = $\cup \{x_i / i \leq n$ and x_i in the description of T is not required to be a relation$\}$ is called the set of objects of y.

c) if y is a set then a structure over y is an entity $\langle y, x, \dots x_n \rangle$ where x_i is a relation on y for $i \leq n$.

d) the language of T, $\mathcal{L}(T)$, is the set of non-logical symbols obtained by describing T in a system of higher order predicate logic. (Balzer

p. 331). Two different forms of incommensurability can be discerned.
First, if T and T' are theories then T and T' are *incommensurable*$_1$ iff
(i) $\mathcal{L}(T) \cap \mathcal{L}(T') = \emptyset$
(ii) $\exists x \in I_p \; \exists x' \in I'_p : \forall z \, (z \in Ob(x) \rightarrow$ z is a structure over $Ob(x'))$
In order to introduce a second type we need some further definitions.
(i) If $y = <x, \ldots x_m>$ and $y' < z', \ldots x'_m>$ are m-tuples of sets then
(1) $y \cup y' = <x_1 \cup x'_1 \ldots x_m \cup x'_m>$
(2) $y \subset y'$ iff $Ai \leqslant m : x_i \subseteq x'_i$
(ii) if T and T' are theories and y, y' are models of T and T' respectively, then y and y' are compatible in their common part (yCCy') iff : $\exists z \in M_{pp} \cap M'_{pp} : r(y) \cup r'(y') \subset z$
Now we can state : if T and T' are theories then T and T' are *incommensurable*, if there are $x \in M$ and $x' \in M'$ such that
(1) xCCx'
(2) $r(x) \in I_p$ and $r'(x') \in I'_p$
(3) there are theoretical components t in x and t' in x' of the same type such that $x\,(t \cup t') \notin M$ or $x'\,(t' \cup t) \notin M'$
(4) $Ob(x) \subset Ob(x')$ or $Ob(x') \subset Ob(x)$ ($\subset$ denotes the proper subset relation).
Combining both definitions one obtains a more general definition of incommensurability. Without discussing Balzer's two examples thoroughly it is worth mentioning that he succeeds in establishing a reduction relation in his second examples of two incommensurable theories (namely impetus theory and Newtonian mechanics) and thus seems to reject Feyerabend's thesis.

When Stegmüller in his (1973) spoke of the problem of incommensurability using the reduction notion, Paul Feyerabend (1977) reacted vigilantly "He (Stegmüller) gives a misleading account of the phenomenon, he lumps together what different authors have said on the matter, he misrepresents them, and suggest a solution that is hardly satisfactory, both from a logical and from an historical point of view." And "Apparently every one who enters the morass of this problem comes up with mud on his head, and Stegmüller is no exception." (7).

The main problem in Stegmüller's theory of reduction is, according to Feyerabend, that it makes two paradigms comparable but does not succeed in turning them into rival paradigms, and "it is only between potential rivals that reduction on the proper sense can be said to obtain". The criteria of comparison that can be used to decide between two rival paradigms remain arbitrary, non-objective, and changing criteria, according to Feyerabend.

Kuhn's reaction on the structuralist approach of the problem of incommensurability is much less negative that Feyerabend's. "I concede at once that, if a reduction relation could be used to show that a later theory resolved all problems solved by its predecessor and more besides, then nothing one might reasonably ask of a technique for comparing theories would be lacking. In fact, however, the Sneed formalism supplies no basis for Stegmüller's counter-revolutionary formulation. On the contrary, one of the formalism's main merits seems to me to be the specificity with which it can be made to localize the problem of incommensurability." (8) He insists that his position on the problem of the incommensurability of theories is not that they cannot be compared, but that there exist no formal language in which both could be fully expressed and which may be used as a point-by-point comparison between them.

In (1979) Stegmüller "admitted" that his account in (1973) was not intended as a final solution that covered all aspects of the problem of incommensurability. "It was only a very restricted philosophical thesis, but even this thesis ought to be revisited". In his *The structuralist view of theories* (1979) he gave in a reply both to Feyerabend and Kuhn a revision of his view on incommensurability. Summarizing his earlier view as "theoretical incommensurability", inc_t, he introduced a second type, "empirical incommensurability", inc_e. This inc_e occurs when one investigates "classical particle mechanics" OPM with "relativistic particle mechanics" RPM, in the following way.

To CPM, the elements of M_{pp} are systems of moving particles as described by the position function. The underlying geometry, taken into consideration, leads to a undeterminacy with an imbedding into the vector space, that is mirrored in the set of admissible transformations (all Galilei transformations). The law of CPM are Galilei-invariant, whereas those of RPM are Lorentz-invariant. This leads to two different equivalence classes, E_G and E_L. The two theories to be compared are no longer about the "same" empirical systems for CPM, M_{pp} has to be restricted to the quotient set M_{pp}/E_G, analogously for RPM to M_{pp}/E_L. A solution to this problem can be found by going back to the underlying physical geometrics "It turns out that they are not incommensurable but comparable. More exactly, they are two competing, incompatible geometrics, the one of which may be taken as empirically refuted"

(9). Stegmüller introduces a tree-level hierarchy, (i) mereology (in S. Lesniewski's sense, a part-whole-theory), (ii) topology, (iii) physical geometry, to "get down to earth" without, however, reconstructing the transition step from (i) to (ii). Nevertheless he concludes : "(1) Within the structuralist approach some important concepts of incommensurability can be explicated; (2) A particular alarming kind of incommensurability is the empirical one, inc_e.

In cases of radical theory change we shall encounter such cases of incommensurability again and again. But this should not disturb us. In all such cases our further research may be guided by the conjecture that the theories are incommensurable because they are based on incompatible underlying theories.

To push the speculations one step further. An additional clarification of this kind of incommensurability, of the difficulties caused by it and of the possible ways to surmount it will depend on our future better understanding of the hierarchical structure of theories and the presupposition relations holding between them." (10).

Let us summarize the main topics of the discussion. If we take the problem of incommensurability as a problem of translation, as is emphasized repeatedly by Kuhn, two questions arise. First, can the reduction relation between two successive theories solve this question or only help to locate it ? Second : what is the importance of the incommensurability problem to epistemology, does the distinction between normal/revolutionary change remain desirable ? The structuralist approach highly enlightened the problem of theory-change. It helped to clarify Kuhn's position and forced him to make hard his notion of incommensurability. But this results are only partial, because, as Feyerabend mentioned correctly, Kuhn did not restrict his notion of incommensurability to the fact that incommensurable theories use concepts that cannot be brought into the usual logical relations (conceptual level). He also mentioned that researchers in different traditions see things differently (observational level) and that they use different methods for setting up research and for the evaluation of their results (methodological level). So, it became clear that incommensurable theories need not to be incomparable (see, e.g., Balzer 1976) and that the reduction relation may be a useful tool to compare theories, but only on the conceptual level. And secondly, one can ask, from a pragmatic point of view, how important incommensurability, interpreted as the impossibility of full translatability, actually is for the problem of theory-choice. Kuhn himself says "If I were now to rewriting *The Structure of Scientific Revolutions* I would emphasize language change more and the normal/revolutionary distinction less". In short, the question of incommensurability as it appears today is far from being solved, but its importance seems much less than both Kuhn and Feyerabend originally suggested.

Part IV. Concluding Remarks

At the end of this very short and incomplete overview of the structuralist approach we want to phrase some problems, however sketchy, and indicate some directions for further research. Concerning the structuralist approach itself :

— A formal-linguistic version of concepts of the theory (as sketched by Niiniluoto 1981) can possibly clarify a lot of problems, and perhaps offer a semantical counterpart (R. Tuomela 1978)

— Introduction of inductive and probabilistic concepts might be useful (see for a discussion Sneed 1981, pp. 98—100).

— The investigations concerning the hierarchical structure of a theory and its relation to theory-change should be intensified.

— The question of the progressiveness of theory-change should be reinvestigated, Stegmüller's account seems too poor and too general to grasp the full complexity of this matter.

Some more general problems :

— Can the structuralist framework be used to formalize other theories of scientific development, such as Lakatos' or Laudan's ?

— Is a decision theoretic account (see, e.g., Levi, 1980) compatible with the structuralist approach ?

NOTES

[1] Kuhn (1970) p. 198.

[2] Ibid., p. 206.

[3] Sneed (1971) p. VII.

[4] Ibid., p. 297—298.

[5] Stegmüller (1979) p. 26.

[6] Kuhn (1976) p. 301.

[7] Feyerabend (1977) p. 363.

[8] Kuhn, p. 300.

[9] Stegmüller (1979) pp. 72—73.

[10] Ibid., p. 77.

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