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A COMBINED APPROACH TO THE DYNAMICS OF THEORIES

HOW TO IMPROVE HISTORICAL INTERPRETATIONS OF THEORY CHANGE BY APPLYING SET THEORETICAL STRUCTURES.

ABSTRACT. Although this article is self-contained, the ideas developed herein continue the discussions in the author's book *The Structure and Dynamics of Theories* (New York 1976). In an introductory section, a brief description is given of the structuralistic or non-statemental view of theories. Physical theories are introduced as nets of ordered pairs, each pair consisting of a sequence of mathematical structures and a class of domains of application. Following J. D. Sneed, a theory is thereby distinguished from its hypotheses or empirical claims. By virtue of the occurrence of terms which are theoretical with respect to the given theory, the whole empirical claim of a theory at a given time must be formulated as one single sentence. In a third step, it is explained what it means to say that a person holds a theory at a given time. This pragmatic concept can serve as a starting point of analyzing certain aspects of theory change. In particular, the idea of a 'normal science' in the sense of Kuhn, as well as the idea of a 'progressive research program' in the sense of Lakatos, can be rendered precise. In order to explain what 'revolutionary progress' means, a new intertheoretical relation is needed. It is suggested that explication of the progressive character of a scientific revolution be made with the help of the relation of approximative embedding of the displaced theory into the supplanting theory. The resulting concept of scientific progress thus stands in strong opposition to all teleological conceptions of progress. This becomes manifest by the fact that it admits of progress branchings, thereby justifying the picture of the 'evolutionary tree' and emphasizing important role of decisions and value judgements in scientific progress.

History and Philosophy of science can be brought into still closer contact by means of concepts referring to paradigmatic objects, paradigmatic dispositions and several aspects of scientific communities. Enclosed in the topics of the discussion are the so-called theory-ladenness of observations, the thesis of holism and the problem of scientific rationality. Furthermore, it is argued that the concept of a rational reconstruction in terms of methodological rules should be abandoned, and that the misleading term 'methodological rule' be replaced by the liberally interpreted phrase 'methodological recommendation'.

1. PRELIMINARY REMARKS

Phrases like "Formal Approach" or even "Systematic Approach" are nowadays generally considered synonyms for linguistic or semantic analyses referring to a text within a formal language. I share, at least to a certain degree, the

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view of J. C. C. McKinsey and P. Suppes that this attitude was "responsible for the lack of substantial progress in the philosophy of science".[1] Indeed, this kind of self-restriction forced philosophers to limit themselves to fictitious examples formalisable in primitive first order languages and to leave examples taken from real science to the historians.

In addition to my scruples against overestimation of formalizations I have reservations concerning the procedure of some philosophers, who, by drawing too simple a parallel between generalisations expressible in everyday language and laws of nature, arrive at a mutilated picture of a scientific theory. Primitive analogies are, although for quite different reasons, as bad as primitive languages.

If, e.g., one focuses one's attention on the fact that scientific claims are mostly formulated as strict universal sentences or as combined universal and existential sentences one easily overlooks another important aspect, namely that such claims make use of mathematical structures. As we shall see, this aspect is important enough to eliminate the common identification of physical theories with hypothetical assumptions or with classes of such assumptions.

Many philosophers of science, in my opinion, also make a third type of mistake. It consists of the erroneous presupposition that our mathematical and physical understanding of scientific theories is abundant and that only the adequate philosophical interpretation is lacking. Leaving aside the question of the mere existence of a sharp boundary line between mathematical and physical understanding on the one hand and philosophical understanding on the other hand, in most cases the former is simply not satisfactory. Furthermore, in the case of physical theories it is not sufficient to give precise and satisfactory axiomatizations, although they are very useful. They are important because by means of them one recognizes the basic mathematical structures of theories. Of equal importance, though, is the unresolved question of how to build a system of empirical hypotheses based on such a structure.

J. D. Sneed has contributed new and interesting insights to this last problem.[2] He has brought to light some completely neglected fundamental differences between mathematical and physical theories. Sneed's results offer additional evidence against the imitation of metamathematics when dealing with physical theories from a systematic point of view.

According to his original intention he devised the new set theoretical apparatus to solve a class of interconnected questions concerning the structure of physical theories and their empirical claims. Besides, his apparatus has proved to be a powerful instrument for clarifying some highly controversial aspects of scientific theories. I even dare to predict that at present his ideas form the best foundation in order to bridge the systematically oriented and the historically oriented philosophy of science. In the following sections I will try to substantiate this claim by explicating some notions of T. S. Kuhn and I. Lakatos. I also hope that these considerations will guard against the acceptance of a 'rationality monism' leading inevitably to a cliché of rationality.

2. THE STRUCTURALISTIC VIEW OF THEORIES

In order to sketch the new approach to theories as untechnically as possible, I will try to describe the most important stages in the historical development of this conception.

The first step in this direction was made when logicians, in particular P. Suppes, suggested axiomatization of a theory by defining a set theoretical predicate. A phrase, "x is a P" (e.g. "x is a group" or "x is a classical particle mechanics"), is introduced, whereby the so-called axioms are parts of the definition of "is a P". In the case of a physical theory the predicate P designates a mathematical structure which, for the moment, we will call the *fundamental structure* of the theory. This structure suffices in considerations of only the mathematical aspect of the theory. A non-mathematical theory however must contain, besides this 'formal' part, a 'non-formal' part to represent the applied aspect of the theory. For this reason E. W. Adams suggested in [1] a two-part reconstruction of an intuitive theory, namely the fundamental structure, called 'characteristic property', and the set of *'intended interpretations'*.

Sneed's approach represents, at least in a first approximation, the result of various additions to and modifications of this scheme. Let us first consider the second part. If we want to find out what applications the author of a work in physics assumes as existing, we must look at the examples and exercises he gives. Every such theory contains thereby *numerous applications* many of which may exist simultaneously. It is an important feature of these applications that they do not exist in splendid isolation but rather *partially overlap* each other. We shall see that this fact creates an additional problem concerning the 'cross-connections' of the various applications and complicates the mathematical structure as well as the empirical hypotheses formulated with help of the fundamental structure. Hence one must not speak of *the* application of a physical theory but of the whole *set I of intended applications*.

In Section 4 we shall encounter a curious property of this set I. In contrast to "domains" in logic and mathematics I is not a ready-made, extensionally closed entity but an 'open' set. Only a subset I_o of I, consisting of the *paradigmatic applications* of the theory, is extensionally given. One might visualize I as starting with I_o and continuously or stepwise increasing as the theory develops.

Let us now focus on the fundamental structure. It suffices here to look at it from an extensional point of view. Then we can identify the fundamental structure with the totality of those entities which satisfy it, i.e. with the *set M of models* of the axiomatized theory. By disregarding the axioms proper but retaining the full conceptual apparatus of the theory we obtain the usually much larger *set M_p of possible models of the theory*.

The theoretical/non-theoretical dichotomy implies another particularity of the Sneedian formalism. Because Sneed's view (which I accept in principle) differs fundamentally from the view of most other philosophers, in particular from that of the empiricists, we must dwell on this point for a moment. Usually one distinguishes between theoretical terms and observational terms. Y. Bar-Hillel seems to have been the first one to emphasize that this distinction resulted presumably from a confusion between the *observational/non-observational* and the *theoretical/non-theoretical* dichotomies. With this conjecture Bar-Hillel anticipated one aspect of Sneed's concept of theoretical terms. The general characteristic which must accrue to concepts in order to be properly called "theoretical" is implicitly contained in what could be called 'Putnam's challenge'. H. Putnam deplored that the decades of writing about 'theoretical terms' left untouched the problem "what is *really* distinctive about such terms", that is, in what way a theoretical term "comes from a scientific *theory*" (Putnam, [20a], p. 243).

It is exactly this question which Sneed tried to answer adequately. His *criterion of theoreticity* could be called 'functionalistic' because it refers to the use of concepts appearing in an applied theory. Very roughly speaking, if the question of whether a concept occurring in a theory T is applicable in a particular situation can be answered without presupposing that this very theory T has successful applications, then this concept is *non-theoretical with respect to T*. Otherwise it is theoretical with respect to T or *T-theoretical*.

In particular, a function is to be treated as T-theoretical if all methods of determining its value presuppose that *this theory* T holds true in some of its intended applications.

I will try to point out some important consequences of this new conception of theoreticity. Carnap has, on several occasions, underscored the conventional component by drawing a boundary line between the 'theoretical' and the 'observational'. It is, he said, like a slice in a continuum. Bar-Hillel accepted this basic idea of Carnap and transferred it to *both* dichotomies to be distinguished according to his view: to the 'observational/non-observational' dichotomy as well as to the 'theoretical/non-theoretical' dichotomy. Clearly *this can not hold true* of the second dichotomy if "theoretical" means "T-theoretical" in the sense of Sneed. We can even go one step further and claim that *no* characterization of theoreticity which meets Putnam's challenge is compatible with this image of a 'slice in a continuum'. The reason for this is very simple. Each scientific theory contains a finite number of basic concepts. In most cases this number is very small (e.g. three in the case of the Newtonian formulation of classical particle mechanics). We must be able to draw the boundary line between the theoretical and the non-theoretical ones only with respect to these very few concepts.

The thesis about the conventional component can not be true either. Since one decides whether or not a term is theoretical by using a *criterion*, a statement of the form "term t is T-theoretical" is therefore not a philosopher's suggestion, but rather an *empirical hypothesis* about the actual use of the term t by the participants of T.[3]

Sneed's approach to theoreticity creates a problem, which he called *the problem of theoretical terms*: Can the mathematical apparatus of a theory containing theoretical functions be used to state *empirical* claims? The answer seems to be: No. For suppose the predicate "P" designates the fundamental structure of T and "a" names (or describes) one of T's intended applications. Then an empirical claim of T necessarily has the form:

(1) "a is a P"

But if T contains theoretical terms we could determine the truth-value of (1) only *if we already presupposed that a statement of this form (1) with the same predicate "P" is true*.

Attempts to check a sentence of form (1) for empirical truth lead, then, to an infinite regress or to a circle.[4]

Fortunately, one can avoid the sceptical conclusion that theories containing theoretical terms cannot be used to formulate empirical hypotheses. One can reject the presupposition that sentences of the form (1) adequately state such hypotheses. By replacing the theoretical function symbols with variables, and by putting the appropriate existential quantifiers before the result of this replacement, we transform (1) into its *Ramsey-substitute* (2). In order to find out if the Ramsey-substitute of (1) is true we need only investigate certain non-theoretical entities, thereby evading the restriction of producing values of the theoretical functions; we need not suppose that any other claim of the form (2) be true. Therefore the Ramsey view offers a possible solution to the problem of theoretical terms.

Actually, one can say even more than this: As long as no other solution to the above-mentioned problem is known, the transition from the traditional view of scientific hypotheses to the Ramsey view is not optimal but absolutely compulsory. Furthermore, this transition must not be interpreted as a philosopher's recourse but as a part of the description of the use of theories within empirical sciences. After this explanation the following passage in Sneed's paper [24] referring to the solution of the problem no longer sounds enigmatic: " ... it (i.e. this way of describing scientific theories) need not be defended against objections that the Ramsey method is in some way epistemologically suspect. That empirical scientists do not live up to someone's favorite epistemological credo is for them to defend, not me."

We must now ask if the theoretical/non-theoretical distinction affects the characterization of theories in terms of set-theoretical structures. Up to now only a twofold distinction has been made within the realm of entities called "models": the elements of M (i.e. those endowed with the full conceptual apparatus of the theory in question and also obeying the fundamental laws of the theory) and the elements of M_p (i.e. those of the same general kind, not necessarily obeying the laws). Suppose now that we 'lop off' from the M_p's all theoretical components. The remaining entities will be called "*partial possible models*" and their totality will be abbreviated by the symbol "M_{pp}". If the predicate "empirical" is used as a synonym for "non-theoretical", then the empirically describable objects, whose behaviour the theory is designed to explain, form a subset of M_{pp}.

For the sake of illustration let us take the example of classical particle mechanics as axiomatized by McKinsey et al.[5] Further, assume that *force* and *mass* are theoretical functions while the *position function* is non-theoretical. Those systems (i.e. those quintuples consisting of a set of particles, a time interval and the three functions: mass, position and force) which satisfy all the axioms of these authors, in particular the second law of Newton, form the models of this theory. Those systems which satisfy the same conditions, with the exception of the second law, form the possible models. Finally, those subsystems in which only the position function remains after removal of the mass function and the force function, form the partial possible models; intuitively they consist of purely kinematical descriptions of moving particles.

When using the predicate "empirical" as in the next to last paragraph, one must not overlook the relation of "non-theoretical" to a particular theory. The empirical character of the descriptions is entirely compatible with the thesis that these descriptions are in a certain sense 'theory-laden'. The theory which is thereby alluded to may be, e.g., a certain theory of space–time measurement. In any case theories more elementary than classical particle mechanics underlie the latter.

For the moment it suffices to keep in mind how the second revision affects the set theoretical characterization of theories, namely, in the case of a theory T containing T-theoretical terms, the essential separation of the three classes M_{pp}, M_p and M.

What is the 'real work' done by theoretical concepts? Within single and isolated applications of the applications of the theory this work remains concealed. And this, by the way, is the reason why the unamended Ramsey-view would not work. Theoretical functions can produce an interdependence between partly overlapping applications insofar as the value which such a function has in *one* application depends on the values of this function in *other* applications. The new concept of *constraint* renders this intuitive idea precise.

The distinction between laws and constraints is the third and perhaps the most important modification of the original scheme. While laws always exclude certain possible models from actually becoming models, constraints rule out certain *combinations of* possible models or of models. One therefore must distinguish between *two kinds of structures* used in a physical theory. The one type of structure is required to be valid in *single* applications. We have this kind of structure in mind when speaking of laws. The ruling-out effect of the second type of structure operates *across different applications.*

Let us designate the set of constraints by the letter "C". Then the first component of a theory, i.e. that component which at the beginning of this section was called the fundamental structure, has taken definite shape and can be represented by the quadruple $K = \langle M_p, M_{pp}, M, C\rangle$. Following Sneed we shall call this quadruple, consisting of the set of possible models, the set of partial possible models, the set of models and the set of constraints, in this order, the *core* of the theory in question.

At several points I used a plural, speaking of "laws", where actually only *one* law was mentioned, namely the basic law of the theory, which the participants in the theory claim to hold in *every* application. This law was extensionally represented by the set M. The special laws, which hold only in *particular* applications, have to be distinguished from this basic law. While the second law of Newton is a basic law belonging to the core of Newton's theory, Hooke's law, for example, holds only in some applications of this theory.

The distinction between the basic law and special laws necessitates a fourth modification of the entity called "theory". This revision suggests a rewriting of the items of what up to now was called a theory in such a way that the theory as a whole becomes a *hierarchical structure consisting of 'theory-elements'*.[6]

Before having discussed special laws I had identified a physical theory with an ordered pair $\langle K, I\rangle$, with I representing the applied aspect and K the mathematical aspect of the theory. The latter was reconstructed as the quadruple $\langle M_p, M_{pp}, M, C\rangle$. In order to avoid terminological confusions we must introduce a new and more general name for these entities. We shall call them theory-elements. More exactly, X is a *theory-element* only if X is an ordered pair $X = \langle K, I\rangle$ with a (theory-element) *core* $K = \langle M_p, M_{pp}, M, C\rangle$ and a set I such that $I \subseteq M_{pp}$. The last clause guarantees that only partial potential models are used as intended applications.

Now, in order to introduce the special laws 'holding in a theory' into this framework *I interpret a law as an entity having the same formal structure as the theory itself*. In other words, a special law is to be reconstructed as a 'mini-theory' of a certain kind.[7] I call the procedure for obtaining a special law from a given theory-element *specialization* of this element. The same symbols standing for the components of a theory-element, but with the addition of a stroke "$'$", will represent special laws. One first singles out a non-empty subset M'_{pp} from the set M_{pp} of the partial possible models in the given theory-element. In a similar way we obtain the subsets M' of M and C' of C. The set of intended applications of the special law can be defined as $I' = I \cap M'_{pp}$ and the set M'_p may be introduced so that its elements are the new partial possible models (i.e. elements of M'_{pp}), together with the theoretical functions.[8] The derived specialization $\langle\langle M'_p, M'_{pp}, M', C' \rangle, I' \rangle$ thereby reflects the formal structure of the original theory-element.

Theory-elements and their specializations can thereby be linked together in a hierarchical order, or more exactly, the specialization-relation forms a *partial ordering* of their whole set. Such a partially ordered set is called a *theory-net* N.[9] We thereby reduce the entity originally called "theory" to one of the many theory-elements, namely the initial theory-element of N. Only its position 'at the top of the net' reminds us of its distinguished role. We will give this initial element $\langle K, I \rangle$ a name and call it the *basic element* or the *basis* $B(N)$ of the net N.

This sketch of how to reconstruct physical theories and their components as set-theoretical structures temporarily suffices.

3. ON THE DISTINCTION BETWEEN THEORIES, EMPIRICAL CLAIMS OF THEORIES AND ACTS OF HOLDING A THEORY

Some readers may now expect that I am going to replace the common identification of theories with hypotheses by a bipartition. Instead, it turns out to be useful to replace it by a *threefold* distinction. Theories and theory-nets are structures of the kind described, but they are not sentences. The empirical claims or empirical hypotheses of theories must of course be reconstructed as sentences or propositions. Actually, the Ramsey-view forces us to interpret the whole claim of a theory as *one single* sentence rather than as a class of sentences. Set-theoretical structures as well as sentences can be taken as abstract entities having no pragmatic aspects. Such aspects enter into the concept of holding a theory. By this we mean certain human acts which, like speech acts, contain a reference to *historical times*, to *persons*, to the *beliefs* of these persons as well as to the *supporting evidence* for their belief.

I begin with some remarks concerning the formal structure of empirical claims based on a theory. The discussion of the theoretical/non-theoretical dichotomy led to the conclusion that all the empirical hypotheses of physical theories must have Ramsey-form.[10] Within linguistic frameworks, such a Ramsey-sentence would have a very complicated structure if due care were taken for all the emendations mentioned in section 2. By using the set-theoretical apparatus already described we can formulate the Ramsey-substitute even as an *atomic* proposition. For this purpose we need a function A, "the application of", which is defined for cores of theory-elements and which assigns to a given core $K = \langle M_p, M_{pp}, M, C \rangle$ a certain class of subsets of M_{pp}.[11] A subset of M_{pp} is an element of $A(K)$, i.e. of the 'application of K', iff theoretical functions can be added to each element of this subset in such a way that (1) this set of 'theoretically supplemented partial possible models' becomes a subset of M (i.e. such that all its elements satisfy the basic laws) and that (2) the whole 'array' of theoretical functions occurring in these supplementations satisfies the constraints C. The *basic claim* of a theory with the basis $\langle K, I \rangle$ thereby becomes the sentence: $I \in A\ (K)$.[12]

In order to obtain the Ramsey-formulation of the empirical claim connected with a whole theory-net N the function A must be extended to apply to whole *nets* N^* of cores of theory-elements. The empirical hypothesis corresponding to a given net N will then be the proposition:

(i) $I \in A\ (N^*)$[13]

To reproduce the content of (i) in everyday language, consider first the net N of 'subtheories' (i.e. of special laws) $\langle K', I' \rangle$ under the basic theory-element $\langle K, I \rangle$. The empirical proposition (i) becomes a sequence of claims of the form $I' \in A(K')$, each one associated with the corresponding subtheory, whereby the first member of the sequence is the basic claim $I \in A\,(K)$. Now all of these claims can be interpreted as explained in the preceding paragraph.[14]

This sketch suffices to show how to distinguish a theory, or more exactly, a net of theory-elements, as a set-theoretical structure, from the empirical claims of this theory; but it is certainly not sufficient for explicating Kuhn's notion of normal science or Lakatos' notion of research programme. Presumably, our apparatus would not even be sufficient to reproduce what working scientists mean when they speak of theories. In all these cases something much less abstract than theory-nets or empirical hypotheses is meant, namely *a certain kind of knowledge, shared by the scientists belonging to a particular group*.

I introduce now a new concept as a starting point to account for these other aspects of science. In a similar way in which today's philosophy of language distinguishes between speech acts and words or sentences as products of these acts, I here differentiate between *acts* of holding a theory and theories as well as empirical claims as *products* of such acts.

In order to explicate the notion of holding a theory we must be able to differentiate between the *essential* ingredients of a theory and its *accidental* characteristics. The participants in a theory are united because they retain all the essentials of the theory, while differing from each other with respect to the hypothetical assumptions or empirical claims of this theory. For example, all Newtonians must accept the same fundamental principles of Newton's theory; but the special assumptions and convictions they connect with this theory may greatly vary from one Newtonian to another.

Hence the initial theory-element plays once again a role, for, as the basic element and starting point for all possible nets, it contains all the essentials of a theory. Further, I must analyse in some detail what I will call "the method of paradigmatic examples", which contributes to a better understanding of the concept of paradigm as used in Kuhn's philosophy of science.

4. PARADIGMS, HOLDING A THEORY AND NORMAL SCIENCE

The account of the 'non-statement view' or structuralistic view of scientific theories in section 2 was unilateral. I concentrated mainly on the analysis of first members of theory-elements $\langle K, I \rangle$, thereby neglecting the 'applied aspect' of theories. Concerning the dynamics of theories, however, the sets I of intended application differ from all other components of a theory-net in an important respect. While the other components, consisting without exception of mathematical structures and substructures, either appear or disappear, the set I always originates in an initially given set I_o of paradigmatic examples.

To characterize a set S with the help of paradigmatic examples means, roughly speaking, providing a list of the elements of S_o,[15] a subset of S. We never decide to remove an element from it. For membership in the difference set S–S_o an object must have a '*significant*' number of properties in common with '*many*' or with '*almost all*' elements of S_o.[16] The vagueness of these terms is irremovable. Of course, it applies to the concept of membership in S–S_o only, and not to the concept of paradigmatic examples. As a matter of fact, the latter concept is our tool *to render precise the kind of vagueness involved*.

In the present case I_o is the set of paradigmatic examples of intended applications, provided by the creator(s) of the new theory. All changes in the domain of intended applications I of the basic theory-element must satisfy the condition: $I_o \subseteq I$. In other words, the set of paradigmatic examples must be a subset of all *possible* intended applications. This does not preclude a violation of this condition. It means only that this requirement is part of the definition of the *identity* of the theory in question. Newton's set of paradigmatic examples for his theory included: the solar system and certain subsystems of it (e.g. earth–moon, Jupiter and its moons), the tides, free-falling bodies near the surface of the earth, the movements of pendulums. We would not call a physicist a "Newtonian" if he excluded some of these domains from the set of intended applications of *his* theory.

The vagueness mentioned before manifests itself in the impossibility of giving precise, necessary *and sufficient* conditions for membership in I. The flexibility gained by scientists working with a given core, however, far outweighs this drawback. This flexibility prevents the scientific working-day from issuing either in stiffness or in the need for superhuman efforts.

By the 'given' core we mean the first member of the basic element $B(N)$ common to all theory-nets N. Let us call this first member the *basic core* K_b of the basic theory-element T_b. The basic core forms the second part in the definition of the *identity* of a theory because it contains the fundamental laws as well as the general constraints.[17] Similar to a change in the subset I_o of intended applications, the (total or only partial) replacement of K_b by another core would amount to *the replacement of the given theory by a different theory*.

That a *person p holds at time t a theory T with intended application I* can now be explained approximately by satisfaction of the following conditions:

(1) There exists a person (or a group of persons) p_o who determined the basic core K_b and the set I_o of paradigmatic examples I_o;

(2) p_o for the first time applied successfully a net N under K_b to a set I with $I_o \subseteq I$;

(3) p accepts the set I_o of paradigmatic examples;

(4) p knows at t a theory-net N with basis $B(N) = T_b = \langle K_b, I\rangle$ and a set I with $I_o \subseteq I$ and he knows of I and of the core-net N^* belonging to N that $I \in A(N^*)$;

(5) p knows at t that expansion of I at t necessarily weakens the net N and that refinement of N at t diminishes the range of intended applications;

(6) p believes that there exist refinements of N applicable not only to I but even to sets including I.

Some expressions occurring in this description of the acts of holding a theory need clarification. To *weaken* a net N means to abandon certain specializations (i.e. certain special laws and/or special constraints) used in the construction of N. To *refine* a net means to dovetail additional specializations within the net. That a person *knows* something says that he believes it and that he has supporting evidence for his belief.

An extended or liberalized version of this concept of holding a theory improves the understanding of T. S. Kuhn's concept of normal science. Several persons belong to the same tradition of normal science only if they hold the same theory. Items (1) and (2) refer to the *historical origin* of the theory; item (3) expresses the *historical continuity* between the creator of the theory and all the other holders of it; item (4) describes the *empirical knowledge* and the *empirical claims* of the holders of a theory; (5) reflects the idea that a holder of a theory '*tries to make the best of it*', i.e. that his claim is as strong as possible; finally, (6) characterizes the 'normal scientist's' *belief in progress*, including *theoretical* progress, consisting of additional refinements of the net, as well as *empirical* progress, i.e. further expansions of the set I.

On the other hand, the definitions of 'holding a theory' and of 'normal science' do not contain any belief in *particular* hypotheses. This interpretation demonstrates, then, that blaming Kuhn for irrationality, as is commonly done, turns out to be totally unjustified. At the same time, the concept of holding a theory makes it possible for the participants in a theory to entertain conflicting hypotheses. Furthermore, it fulfills the elementary requirement of falsificationism, that empirical hypotheses are to be given up as soon as they are effectively falsified. The sharp boundary line between a theory and the empirical claims of this theory makes the views of Popper and Kuhn compatible, at least in one important respect. If, e.g., a researcher hypothetically accepts a special force-law and the law does not 'prove its mettle' because it fails to enable valid predictions, he must, as a rational being, give up this law. Nonetheless, if he makes the theory responsible for this failure, then he indeed behaves 'like a poor carpenter who blames his tools'.

It could be objected that the attempt to explicate the concept of normal science presupposes that the word "theory", within the context "holding a theory", has approximately the same meaning as Kuhn's term "paradigm", and that this synonymy does not hold true. Recently Sneed proposed in [24] to introduce a concept of Kuhn-theory. This concept handles many more aspects of what Kuhn calls paradigm. A Kuhn-theory is a quadruple $\langle T_b, I_o, N_p, \mathcal{N}\rangle$ consisting of a *basic theory-element* T_b with the *basic core* K_b; the *set* I_o *of paradigmatic examples*; the *paradigmatic subset* N_p (representing the substructure contained in *all* notes used in the course of the theory's history); and finally, the *class* $\mathcal{N}$ *of all theory-nets whose basic element is* T_b *and which include the paradigmatic net as a part.*

Would it then be possible to simplify the former approach by taking this concept as a starting point, defining "holding a theory" as an *act* having a Kuhn-theory as a *product*? Unfortunately, Sneed's concept of Kuhn-theory consists of a mixture of 'essential' and 'accidental' components. The latter are ingredients not used to *identify* the theory as a *particular* theory which accompanied the development of the theory 'by mere historical accident'. A special force law, e.g., may have to be incorporated into the N_p of T because during T's history it was never called into question. Refutation and replacement of this law would not force us to say that the scientists held a different theory after revision, but only that the development of *this* theory took another course.

Thus, which one of the two concepts one prefers depends on what one is more interested in. The concept of 'Kuhn-theory' gives a good survey of several components of Kuhn's concept of 'paradigm', which are distinguished from a logical point of view. On the other hand the concept of holding a theory seems to explicate better the concept of normal science.

5. THEORY-LADENNESS OF OBSERVATIONS, HOLISM AND RATIONALITY

The phrase '*theory-ladenness of observations*' conceals a fundamental ambiguity. To clarify this, suppose that within a given context one refers to a *particular* theory T as well as to *particular* observational data which are of some relevance for T. We ask the holder of the thesis that these data, like *all* empirical evidence, are theory-laden, *which theory* he refers to. Suppose he answers that observational data are never 'theory-neutral', for *some* theoretical and hypothetical components enter into these data. In the present case these components would differ from T. Let us call this the *weak version* of the thesis of theory-ladenness. In this weak version the thesis, if accepted, creates no specific difficulties *because in relation to our theory T these data are still neutral*. The 'theory-ladenness' comes from a theory (or from several theories) *different from T* and *underlying T*. Of course, even in this weak sense, the thesis is connected with the important problem of how to reconstruct theories in the appropriate 'hierarchical order'. Only partial answers are known to this challenge.

But it seems that the adherents of the thesis have something quite different in mind. In order to understand what they mean we have to interpret the thesis in the *strong* sense. According to it the data refer back *to the very theory itself* for which they are relevant. Phrases like "theories define their own facts" demonstrate that at least some authors believe in this strong version. Whenever this thesis holds true we are confronted with a difficulty: *How can we test a theory by means of data which we can not understand unless we accept the theory, or part of it, as true*? Replacement of "theory *T*" by what ought to be said according to the present view, namely: "*empirical claims of* theory *T*", does not reduce this difficulty. However, this is only one version of *Sneed's problem of theoretical terms*. Therefore the answer actually amounts to stating that the only known way out of the difficulty is the *Ramsey-solution* of theoretical terms.

The "*holism of empirical claims*" follows immediately from this solution. It states that the empirical content of a theory is, at any given time, one *singular* big claim and not 'a class of numerous empirical hypotheses'.

The holistic view applies not only to empirical hypotheses, but also to theories. The "*holism of physical theories*", then, is a consequence of the former. Whoever wants to formulate a physical claim must decide which particular core he will use. Therefore he can not accept (or reject) a theory piece by piece, but only as a whole.

It now suffices to show why one need not state '*criteria of rationality*'. Whether an empirical scientist, qua scientist, is a reasonable man or not depends on his attitude towards hypothetical assumptions. The concepts of holding a theory and of normal science are to be reconstructed by disregarding the attitude towards empirical hypotheses. Therefore, *the concept of normal science is completely neutral with respect to the question "rational or not*?". The normal scientist can satisfy or violate any given 'criteria of rationality' formulated by a theory of confirmation (or corroboration) and by a theory of testing.

Only if the 'refutation of a theory' existed could a normal scientist who continues to hold a refuted theory be called unreasonable. As will be shown in section 7, such a situation is not possible, at least not in physics.

It is not difficult to understand why, in the opinion of Popper, Watkins, Lakatos, et al. normal scientists, as defined by Kuhn, are kinds of '*epistemic monsters*'. Without a differentiation between theories of mathematical physics and physical hypotheses the concepts "holding a theory" and "accepting a hypothesis" become synonyms. Popper's conclusion soon follows, that the normal scientist, as defined by Kuhn, "is a person one ought to be sorry for" because "he is a victim of indoctrination", rejecting all criticism of his beliefs and adhering to refuted claims.

6. RESEARCH PROGRAMMES

I will here interpret Lakatos' concept of research programme as I interpreted Kuhn's concept of normal science. Before doing this I must remove an inconsistency in the long article [14] by Lakatos. On p. 118 he takes theories *as members of* such *sequences* which on p. 132 he calls research programmes. Nevertheless, on p. 124 et passim he speaks of *Einstein's theory* and *Newton's theory*. This way of speaking is inconsistent insofar as no human being could ever design a research programme containing whole theories like these *as members*.

I remove the inconsistency by taking the expression "theory" in the writings of Lakatos as ambiguous. In all places where theories are considered as members of research programmes I interpret them as strong theory-propositions in the sense of section 3. A '*progressive research programme*' can then be considered *a sequence of strong theory-propositions* $I_j \in A\ (N_j^*)$ such that for every k later than j either I_j is a *proper* subset of I_k or N_k is a refinement of N_j such that the partial possible models belonging to $A\,(N_k^*)$ form a *smaller* class than those belonging to $A\ (N_j^*)$.[18]

The concept of '*holding a progressive research programme*' can then be introduced analogously to the concept of 'holding a theory'. I thereby presuppose that the second meaning of the concept of research programme, i.e. the sense in which Lakatos speaks of Einstein's theory, can be explicated with the help of Sneed's structuralist view of theories.

Note that I speak of *mere analogy to* the concept of holding a theory: For the sake of simplicity I disregarded the confirmatory aspect, whereas Lakatos did not; Lakatos seems to have had in mind only cases of progress, whereas the concept "holding a theory" is more general, including also *setbacks;*[19] furthermore Lakatos mentions no equivalent to my 'method of paradigmatic examples'.

Despite these differences there exists a much greater similarity between the Kuhnian concept of normal science and Lakatos' concept of research programme than is normally recognized. Lakatos himself was partly aware of this similarity when he characterized his idea of research programmes as "reminiscent of Kuhn's 'normal science' ".[20]

7. REVOLUTIONARY SCIENCE AND THEORY DISLODGEMENT

At the end of section 4 I stressed the different roles played by the items which identify a particular theory, like I_o and K_b, and by those parts of a theory-net which may change while the theory remains constant, like specializations of K_b, special constraints and elements of the difference set $I–I_o$. Using a colourful terminology, one could call all the properties of the first kind the *essentials* of a theory and the characteristics of the second kind its *accidentials*.

I have discussed up to now only *accidental changes*. Because of the occurrence of mere accidental changes the decision not to give up a theory is compatible with the rejection of refuted empirical hypotheses.

In keeping with this terminology *revolutionary changes* are *changes of the essentials* of a theory. In the course of a 'revolution', some items, which define a theory, are replaced.

What was felt as most provoking in Kuhn's description of scientific revolutions was his interpretation of how such revolutions come about. According to the empirists and the Popperians, an old theory must *founder on experience* in order to give way to a new theory. According to Kuhn the old theory is *supplanted by* the new one: the decision to reject a theory concurs simultaneously with the decision to accept another.

I shall call this aspect of revolutionary theory change as emphasized by Kuhn "*theory dislodgement*", which is actually an abbreviation for the longer phrase: "dislodgement of a theory by a replacing theory."[21] Many found in Kuhn's description of this phenomenon a second reason to accuse him of imputing irrational behaviour to scientists. The charge was mainly based on two of his claims: first, on the thesis of the immunity of the 'paradigm-theory', and, secondly, on his insistence on the alleged fact that the new theory disseminates not by arguments but by other means, like persuasion, value judgement, propaganda and even power.

Although formal logic can not deal with all the details of this point because some touch on psychology and sociology, a brief analysis with formal logic can contribute fundamentally to an understanding of what Kuhn means to say. Actually, only such an analysis can destroy the prevailing impression that scientists doing 'extraordinary research' are *epistemic monsters of a second kind*, like religious or political fanatics.

In sections 3, 4 and the end of 5, I alluded to one aspect of the immunity or steadfastness of a theory. Suppose a scientist contrives an empirical hypothesis $I \in A(N^*)$, whereby the basic core-element is K_b, and this hypothesis then founders on experience. If previous work with the mathematical structure K_b succeeded, there is no reason to blame the theory for the inability to account for the experimental results; the scientist might have exhibited a lack of skill and ability in handling his mathematical tool. Only the withdrawal of an attempted refinement of the net, or the abandonment of an intended expansion of the set of applications of the theory would follow *objectively* from the failure.

From a logical point of view we can say even more than this: No finite number of *unsuccessful* attempts to use a theory with the two 'paradigmatic components' K_b and I_o to state an empirical claim $I \in A(N^*)$ can be taken as a proof for the *nonexistence* of a successful claim of this form.

Even if *all* the competent scientists are convinced that a theory does not apply to a particular area in $I–I_o$, which in earlier times was believed *to be* an application, they still may retain the theory and throw out the area from the alleged range of intended applications.

But couldn't it happen that a theory *must* be rejected because the fundamental law M was falsified? For theories of mathematical physics the answer is: No, *this can not happen*. In the case of classical particle mechanics, for example, one would have to falsify the second law of Newton. *No one has ever told us what empirical data would have to look like in order to falsify Newton's second law*. 'Empiricists' as well as 'critical rationalists' may consider this a challenge. Although the basic core of a theory is not totally empty, like a tautology, it is 'almost empty'. This 'almost-emptiness' precludes the empirical falsification of the fundamental law of a theory.[22]

The 'three-dimensional' immunity of theories explains why revolutions take place as described by Kuhn and why nevertheless the scientists involved need not be acting 'irrationally'. Neither the proponents of the old theory nor those of the new one can be reproached for using no arguments. For how can you blame people for not using something which does not exist? The participants of the old theory may *hope* that the success of these 'paradigms' will come up in time. And the adherents of the new theory as a rule can do no more than *believe* and *hope* that the partial success of their theory will turn out to be 'success all along the line'. An old theory cannot disappear due to refutation or foundering on experience; the community of its holders simply *dies out*. Similarly it follows from the immunities mentioned, that revolutionary changes are not primarily based on theoretical insights but on *value-judgements* and on *decisions*.

I reserved the phrase "theory dislodgement" to refer to the displacement of the basic core K_b, either alone or together with the set I_o. One can generalize this notion so that it becomes applicable to other changes *within* the net N as well. The 'higher' the position within the net of the supplanted specializations, the 'stronger' one realizes the change. This accounts for some of the 'mini-revolutions' which were admitted by Kuhn. To take an example: Suppose the known 'anomalies' of Newton's theory T_1, i.e. of classical particle mechanics in Newtonian formulation, could have been dealt with by replacing the third law by a different one. In this case no revolution of classical particle mechanics would have taken place, but a mini-revolution of a high order, i.e. the *Newtonian* classical particle mechanics T_2, consisting of T_1 and that specialization of T_1 in which the third law is introduced, would have been replaced by a theory-specialization $T_2{}^*$ of T_1.

This kind of relativisation of the concept of revolution works in the opposite direction as well, provided one has a sufficiently clear notion of the principles (even more general than those included in K_b) which M. Bunge includes under the heading "protophysics". Recently, C. U. Moulines rendered precise the protophysical framework presupposed in classical equilibrium thermodynamics.[23] The protophysical principles consist of fundamental assumptions concerning concepts like *system, state of* a system, *combination of* states, *transition of* states, *equilibrium*. A 'protophysical revolution' in this field would consist in a change of the algebraic and topological properties of the five notions mentioned.

Quantum mechanics developed similarly to such a 'protophysical revolution'; perhaps for this reason many experienced its rise as a kind of 'hyper-revolutionary cataclysm'. In this case, fundamental protophysical concepts which in the past had survived physical revolutions, like "state", "probability" and "law", or even "being identical with", underwent change as well.

8. REVOLUTIONARY PROGRESS AND INTERTHEORETICAL RELATIONS

Within the present framework it was relatively easy to talk about progress of 'normal science' or of progress 'within a research programme'. The situation becomes much more difficult when we try to say anything substantial about progress in case of radical theory change called "theory dislodgement".

In my opinion, almost all contributions to this problem, beginning with the famous Popper–Lakatos–Kuhn discussion, totally missed the point. They discussed, e.g., whether there is 'real progress' in scientific revolutions and whether these events are at least in some sense, 'rational'. But before entering in such debates a deeper problem must first be solved, namely *what the word "progress" in cases of theory dislodgement could mean*. I'll try to clarify this question by distinguishing between five different historical conceptions.

The position called "*falsificationism*" is the least satisfactory. It does not even do justice to empirical claims of mixed quantificational form or to statistical hypotheses. Theories are treated as if they were deterministic laws of the simplest kind.

Lakatos' idea of *research programme* moved to disentangle the common identification of scientific theories from empirical convictions. Unfortunately, in another respect he retained Popper's view, namely with respect to what Kuhn calls "normal science". Perhaps he was even more shocked than Popper by this sad monotony of science. In any case, his endeavour to tow the philosophy of science out of the swamp of Kuhnian 'mob psychology' (i.e. mob psychology applied to the unreasonable masses of people doing science) culminated in the attempt to define an analogy to falsification on the more general level of research programmes, so to speak a 'second order falsification'. The displacement of an old research programme with a new one needs justification; this is given not by arguments in support of the new programme, as 'justificationism' would have it, but by criteria of 'degeneration' of the old programme.

In my opinion, there is little hope that this metascientific research programme will ever successfully render precise the criteria of degenerating research programmes. First, the confused situation of 'first order falsification'[24] makes a successful solution of this problem an unpromising business. Secondly, all the arguments of section 7 for the immunity of theories can easily be translated to argue for the immunity of research programmes. Thirdly, no satisfactory answer has been given so far to Feyerabend's challenge that standards of the kind Lakatos was looking for "have practical force only if they are combined with a *time limit*".[25]

The third position, usually ascribed to Kuhn, claims that *historical facts alone* decide about progress in extraordinary science. However, a historical process is always *potentially symmetric* in the following sense: under suitable circumstances the actual course of events *would* run in the opposite direction. To let history decide about revolutionary progress would mean to interpret the latter as a potentially symmetric relation. Whatever happens, it *is* progress by definition.[26]

There certainly *are* places where Kuhn gives the impression that he did indeed hold this position, e.g. on p. 166 of [8] where he says: "Revolutions close with a total victory for one of the two opposing camps. Will that group ever say that the result of its victory has been something less than progress? ... To them ... the outcome of the revolution must be progress." Interpretations of such passages as attempts to *define* or to *explicate* the notion of progress would certainly justify the *charge of relativism* because of the potentially symmetric character of the resulting concept.

But what Kuhn really wants to say concerning a definition of 'progress' comes to light on the last three or four pages of his essay. There he underscores that he does not claim to give a positive solution to the problem of scientific progress, but only rejects all variants of a teleological metaphysics explaining progress in terms of 'coming closer and closer to the truth'.

It seems to me that Kuhn is correct in this respect. The 'true constitution of nature' should not be taken as more than a metaphor. I am the last to deny the importance of the semantic concept of truth. But an abyss separates semantics and the notion of increasing verisimilitude. In the next section I shall argue that the introduction of the correct concept of truth is compatible with the view that there is not 'one specified goal' of science because progress branchings may occur at different levels. It then follows that the use of Tarski-semantics in order to explicate the idea of coming closer and closer to the *one* truth leads to a cul-de-sac. Actually, this is a misuse of a notion, fruitful in various areas, for the purpose of developing a teleological myth of progress.

If we call the teleological view the fourth position, then Kuhn's positive attitude to the problem can be considered the *fifth* one. I derive this position from private conversations and correspondence, and also from the striking similarities between particular ideas of Kuhn and thoughts found in Wittgenstein's last posthumous book *On Certainty*. (I will return to this relationship in section 10.) I therefore try to find Kuhn's answer by asking how Wittgenstein might have answered. Roughly speaking, he would have said: "No particular, simple criterion of progress exists. There can be only *good reasons* for theory change. These reasons are numerous and they vary from one cause to another. If we can spell out a sufficient number of them in a particular situation, then we feel entitled to speak of progress."

For certain types of situations, to be specified later (see section 9.), this really seems the most satisfactory general answer, although a little bit more can be said about the nature of these reasons and of the way they determine the theory choice. However, for a systematic investigation of the question: "What does 'scientific progress' mean in case of theory dislodgement?" a *sixth position* is needed. I suggest that the answer to our problem be given *in terms of intertheoretical relations*, if such relations are available.

With this suggestion I also admit, right at the outset, that no simple and no nontechnical solution can be given. Actually, rather than speak of *the* solution to the problem of scientific progress, one should make the distinction that the general framework be built up on the abstract level, where *arbitrary* theories come under consideration; in each *particular* case, then, with a pair of theories T_1, T_2, where T_2 displaces T_1, the progressive character of the dislodgement must be established by reconstructing the appropriate intertheoretical relations in such a way that they fit into the general framework.

As a starting point, choose the particular kind of intertheoretical relation called "*reduction*" by Sneed.[27] I cannot enter here into the rather difficult technical details of this concept. Therefore I emphasize only that feature of it which is most important in the present context: The Sneedian concept of reduction does not presuppose that the two theories use 'the same apparatus' or that they are formulated 'in the same scientific language'. Reduction applies even to such cases where Kuhn speaks of 'incommensurability' and Feyerabend of 'incomparability'.

To be sure, much difficult work is still to be done in order to develop a formal method for dealing with two different theories, like relativistic and nonrelativistic mechanics. The reason is that the concept of reduction, whose basic idea goes back to E. W. Adams and which Sneed then improved and expanded to apply to the present more complicated conceptual structure, originally was not intended to improve our understanding of the revolutionary theory change. Rather, it was devised to prove that a 'reducing theory' (like classical particle mechanics) which historically developed *before* the 'reduced theory' (like rigid body mechanics) can be interpreted as a 'parent theory' of the latter 'offshoot theory'. But the point mentioned before already comes into play: Reducing and reduced theories have completely different theoretical superstructures. Comparability must be presupposed only at the nontheoretical level, i.e. at the level of *partial* potential models.[28]

In the case of revolutionary theory change some additional aspects supervene. The fact that the reducing theory comes later into existence than the reduced theory is irrelevant from the logical point of view. It is relevant, however, that in revolutionary theory change, the new theory need not be able to reproduce *all* the unmodified empirical claims of the old one. The new theory is expected to be *better* than the old. As a consequence of this, certain empirical hypotheses formulable on the basis of the old theory will become *invalid* within the new theory. Actually, Sneed's concept of reduction implicitly makes allowance for this case. For if K is a core-element of the old theory and K^* is the corresponding core-element of the new one, then there may exist partial potential models lying in *$A(X)$ whose counterparts are not in $A(K^*)$*. What for the general case is a mere possibility will in the revolutionary case become necessary. It is not difficult to make this requirement part of the definition. Let us speak in this case of an *explicitly partial* reduction.[29]

In most cases there will be still another difference. Even those empirical claims of the old theory which are not rejected by the new theory will be, as a rule, not *exactly*, but only *approximately* reproducible by the displacing theory. G. Ludwig, in [16], took an interesting new approach to the problem of approximation. Although the conceptual frame of this book looks very different from the present one, Ludwig's basic ideas can, as Moulines shows in a yet unpublished manuscript, be 'transcribed' into the Sneedian apparatus and further developed within it.

In view of the two differences mentioned I now replace the word "reduction" by the expression "partial and approximative imbedding of one theory into another". Roughly speaking, my thesis then runs: *to attribute the property "progressive" to a scientific revolution means to claim that the displaced theory can be partially and approximatively imbedded into the supplanting theory*.

This claim would be highly speculative, if it could not be supported by the work done on reduction on the general as well as on the concrete level. Even so, it certainly *is* speculative in the way that a new 'paradigm' (in Kuhn's sense) is speculative at the time of its appearance. I therefore frankly admit that the success of this idea is, to borrow Kuhn's words, for the moment "largely a promise of success discoverable in selected and still incomplete examples".

Even in case the work in process finally ends in 'real success', I should like to warn against possible overestimation, because a successful reconstruction of the concept "progressive theory dislodgement" will deviate in two important respects from the notion of 'progress' within the teleological metaphysics of increasing verisimilitude. First, a notion of progressiveness based on intertheoretical relations will satisfy only internal criteria, no external ones. Therefore it will not guarantee that knowledge evolves linearly *toward a particular goal*. (For a more detailed account see section 9.) Secondly, this notion must satisfy the basic requirement that 'the unrefuted empirical content' of the old theory must be reproducible within the new theory. Here, 'content' refers to the *actual* empirical claims made by the adherents to the old theory, not to *all possible* claims.

An opponent may object: "Knowing that a theory dislodgement has resulted in progress means something much stronger, for it must imply *definite* or *conclusive epistemic progress* which at the same time *ultimately dooms the old theory*". To this I would answer: Such knowledge does not exist, at least not as a *human* knowledge. I do not deny by this statement that it is possible to *define* such concepts as "ultimate success" and "ultimate defeat" of theories. But concepts like these can only be the object of speculative belief, not of knowledge.

Remember: The intertheoretical relation sought for must be strong enough to prevent the *inverse* process of a 'progressive theory dislodgement' from becoming progressive in another possible world. It need not be strong enough to guarantee *progressiveness* of the dislodgement *'in all possible worlds'*. That the displacement of nonrelativistic mechanics by relativistic mechanics was 'progress' in *this* world does not necessarily mean that in no possible world (e.g. one *in which time needed and costs count for nothing*) a Super-Newton would follow the 'counterpart' of our Einstein and reproduce all the achievements of the Einstein-counterpart and much more.

This result does not conflict with the earlier remarks about the immunity of theories, for a theory's immunity with respect to falsification means actually that there exists no ultimate defeat of a theory. And this, of course, does not mean that we are better off than the fallibilist thinks we are, but that we are worse off. If "objective superiority of a theory" were to mean, say, "superiority in all possible worlds", then there is no way to prove, or to give a good reason for, the objective superiority of one theory with respect to another.

On p. 171 of [8] Kuhn remarks that a number of vexing problems may vanish "if we can learn to substitute evolution-from-what-we-do-know for evolution-toward-what-we-wish-to-know". I hope that I gave in this section some constructive hints *how* to make this substitution.

9. CUMULATIVITY AND LINEARITY, PROGRESS BRANCHING AND THE ROLE OF VALUE-JUDGEMENTS

Critics of Kuhn very often characterize his position by using the slogan that science develops neither cumulatively nor linearly. From a logical point of view these two characteristics are independent of each other. "*Cumulative*" refers to a relation between the present and the past, and conveys the idea of preservation of past achievements. "*Linearity*", on the other hand, refers to a relation between the present and possible futures; it designates the goal-directedness of human knowledge toward final truth.

Those who admit that revolutionary changes destroy, at least sometimes, cumulativity but who maintain the necessity of goal-directedness and linearity of scientific growth, grant that these two features are to be distinguished. In my opinion, these critics go in precisely the wrong direction. Cumulativity *can* be justified in the diluted sense of the last section. Linearity, on the other hand, can *not* be defended because it remains as a vestige of teleological phantasy.

Actually, the unfoundedness of teleological metaphysics does not form the base of my argument. I proceed from the fact that *progress branching is always possible, in the case of normal science as well as in the case of theory dislodgement*. Scientists working in a given tradition encounter a possible branching situation if they must choose either to refine their net, while leaving the range of intended applications unchanged, or to increase the set of intended applications at the cost of the net. In such circumstances (and they may arise at any time) the holders of the theory must *decide* which course to follow. Whatever the outcome, the basis of this decision will have been *value judgements*.

The revolutionary case necessitates further differentiations. Value-judgements will always be necessary when a new theory 'tries to supplant' an old one, for the success of the new core will at first be very limited, clearly recognizable perhaps in only one particular application. At a later time, the supplemented new core becomes the basis of a whole net of core-elements and the old theory may perhaps be reducible to or approximatively imbeddable into the new one. Whenever the latter takes place *the original decision* in favour of the new core *has been objectively justified*. The value-judgement on which the decision was based becomes, so to speak, *provisional* in retrospect, since objective superiority now replaces its subjective component.

This situation must be strongly distinguished from a case in which a theory has at least two different successors each of which is superior to the given theory in the sense explained, but which are partly incomparable, having only partially overlapping applications. Let us call this "*theory dislodgement connected with possible progress branching*". Again, one must decide whether some, all or none of the new possibilities are to be pursued. But this time the underlying value-judgements are not provisinal but *final*.

Some hold that such a possibility is incompatible with the semantic conception of truth. But such a claim is simply incorrect. Suppose that there exist theories T_1 and T_2, incomparable with each other (for the reason mentioned before) but both potential superseding successors of an earlier theory T_0 (i.e. T_0 is at least approximately imbeddable into both T_1 and T_2). Given suitable formal languages, S_1 and S_2, one can introduce in a precise way the concept "true in S_1", applicable to empirical claims of T_1, as well as the concept "true in S_2", applicable to empirical claims of T_2. Of course, this technical maneuver does not help discover the preferable theory; it duplicates the branching situation, or better, it retains branching on the intuitive level, which has now become the meta-level, and reproduces it on the formalized object-level. In any case Tarski-semantics does not exclude progress branching in case of theory dislodgement.

Presumably, some critical readers would call my position "subjectivistic".

In answer to this I would only point out that to leave room for decisions on the basis of value-judgements is not to surrender to subjective arbitrariness. As I have argued at length elsewhere,[30] many kinds of investigation which have been running under headings like "inductive logic" do not belong to the field of 'theoretical reason' at all, but to the field of 'practical reason', to use Kant's terms. Similarly, in the present case one leaves behind the realm of theoretical reasoning and enters the domain of practical deliberation. *In every possible branching situation (in our world of limited lifetime and scarce resources) theory choice ceases to be the object of theoretical justification and becomes a case for rational decision theory.* This is my interpretation of, and concession to, what in section 8 was called the Kuhn–Wittgenstein position of 'good reasons.'

10. ON POSSIBLE ADDITIONAL IMPROVEMENTS OF THE INTERPLAY BETWEEN HISTORY AND PHILOSOPHY OF SCIENCE

Recently, T. R. Girill has pointed out in his review of the Popper–Kuhn–Lakatos discussions that Popper "treats science entirely in terms of knowledge-that", while "Kuhn's 'paradigmatic' interpretation is equally pre-occupied with science as knowledge-how".[31] The remark about Popper can be generalized insofar as it applies to *all* kinds of *logical* reconstructions. All the set-theoretical entities I have been treating, like theory-elements, cores, theory-nets, intratheoretical as well as intertheoretical relations, are means to grasp something of the 'knowing-that'-aspect of scientific research.

On the other hand, when introducing the concept of holding a theory, I interpreted some of these entities as *products* of certain human *acts*. To handle these notions in terms of 'knowing-how' means to concentrate on the underlying *dispositions* manifested in acts and their products. These dispositions constitute a complicated network of personal ingenuity and skills, and of social habits and capacities. Kuhn has called the latter the "*disciplinary matrix*".[32] Although he intended merely to remove an ambiguity from the term "paradigm", Kuhn's step goes farther. In section 4 I explained several entities, to which "paradigm" refers in its first sense. Let us call them collectively "*paradigmatic objects*". I now deal with "*paradigmatic dispositions*"; the word "paradigm" refers to them in its original second use, now being replaced by the new word quoted.

To some extent, the study of these dispositions falls into the domain of individual and social psychology, and sociology. However, the writings of G. Ryle and the later works of Wittgenstein show the role of philosophical investigation in analyzing these dispositions. Wittgenstein's *On Certainty* especially contains many similarities to Kuhn's ideas.[33] A successful systematization of these thoughts of Wittgenstein could throw new light on Kuhn's ideas as well, and could bring psychology of research, general epistemology and history of science closer together.

I have primarily in mind the problem of the temporal change of the above-mentioned network of individual and social dispositions. Much of what Kuhn says about non-continuous paradigm change deals with a *change of 'knowing-how.'* And in this respect, presumably, the idea of 'cumulativity' must be completely abandoned.

Part of the resistance against a new physical theory-element or a new 'paradigm', as described by Kuhn, develops because the scientists working within a particular tradition are expected not only to acquire new knowledge, but also to change that extremely complicated disposition sometimes called "*physical intuition*". They are expected to change this disposition *as a community*, in such a way that they can continue to do teamwork. Many of them will, perhaps only subconsciously, feel this to be an inhuman and unreasonable demand. When this is the case the old theory does not disappear because its adherents are convinced or persuaded by the new one, but because they *die out*. There is nothing 'irrational' about this fact, just as it is not surprising that the success of the new theory depends to a great extent on waiting for those young men who, free of conflicting dispositions, will be able to form a particular new kind of physical intuition.

'Real life' could amend and fill in these logical reconstructions in still another dimension. The Sneedian formalism consists mainly of set-theoretical and algebraic structures and substructures, the two most important concepts being the theory-net and the core-net. We could try to breathe life into these concepts by relating their key notions to scientific communities and to historical time-intervals.[34]

Concerning the given set of *historical intervals* h_i, suppose that they are ordered by a transitive, connex and antisymmetric relation "is (historically) not later than". By a *scientific community* SC_i we understand a group of people who (1) share the relevant dispositions mentioned in the previous paragraphs, (2) communicate with each other on scientific matters during a historical period, and (3) avail themselves of the same technics of measurement and the same test procedures.

A "*core-realization* k_i *of the theory-element* T_i *by the scientific community* SC_i *at time* h_i *on the marked partition* J_i *of* I_i" is, then, a quadruple of the four items $T_i = \langle K_i, I_i \rangle$, SC_i, h_i and J_i such that there is a non-empty subset I_i^* of I_i, and the community SC_i accepts at h_i the proposition that $I_i^* \in A(K_i)$ while a subset SC_i' of SC_i believes of the whole set I_i during that $I_i \in A(K_i)$. While "to believe" has here the usual meaning, the verb "to accept" is used in the following, more technical sense: that SC accepts proposition P means that the ('overwhelming') majority of members of SC considers P as a proposition secured by the confirmation- and test-procedures characteristic for this scientific group SC. We may call I_i^* the '*secured domain for* SC_i' of intended applications (of the core K_i of the theory-element T_i) and the difference set $I_i - I_i^*$ the '*conjectured domain for the subgroup* SC'' (possibly consisting of one single man).

Since the historical time is discrete we can easily define the phrase: "the core-realization k_j *follows immediately* (in the historical sense) the core-realization k_i". Finally, we may call D "*the development of a theory according to the paradigm* T_0 *for the scientific community* SC iff D is a sequence $\{k_i\}$ of core-realizations, with SC remaining the same throughout the whole sequence, such that: (1) each k_{i+1} follows immediately k_i; (2) there is a theory-element $T_0 = \langle K_0, I_0 \rangle$, (3) every K_i taken from k_i is a specialization of K_0; (4) every I_i taken from k_i is such that $I_i \cap I_0 \neq \phi$ and for each member J_i^k of the marked partition J_i of I_i there is a subset I_0^{ik} of I_0 such that I_0^{ik} is a paradigm set for J_i^k, recognised by the community SC.

Some terms in this definition require explanations: The idea of a *marked partition* J of I is that every member J^k of J consists of a set of 'homogeneous' applications. I^k is a *recognized paradigm set* for J^k iff I^k contains as elements such entities which the scientific community recognizes as paradigmatic examples for the whole (homogeneous) set J^k.

It can be shown that the development D of a theory (according to T_0 for SC) has the structure of a theory-net in the Sneedian sense. But D is at the same time, because of the twofold relativisation to historical times and to human groups, a '*dynamized*' and '*sociologized*' version of a *liberalized variant* of a theory-net. Speaking of a liberalized variant is justified because the 'basic theory element' T_0 need not be identical with the historically first one, introduced by the theory's creator. In particular, the paradigmatic set I_0 need not be a subset of all I_i's; it suffices that suitable subsets of I_0, called I_0^{ik} in the definition, serve as paradigmatic examples for the partitions of the I_i's.[35]

If, for each k_i of D, $I_i^* \subset I_{i+1}^*$, I_j^* being the secured domain of k_j, one has an *'ideal'* development of the theory. This special case could serve as an alternative for explicating the notion of a research programme.

Several philosophers have deplored that no systematic pragmatics is available. The sketch given, together with some earlier definitions and remarks, could perhaps lay the foundations of a systematic pragmatics for a *special* field.

11. METHODOLOGICAL RULES AND RATIONAL RECONSTRUCTION

I am not sure whether in my earlier remarks on research programmes I did full justice to Lakatos. My uncertainty arises because I do not know what he meant by "*rule*" in the context "methodological rule".

It seems to me that the word "rule" should be used only in case where a violation amounts to a *mistake*. With this convention one can hardly make a difference between logical rules and methodological rules. On the other hand, Lakatos presumably did not want to claim that whoever violates a methodological rule does nothing better than logical nonsense. This being so, the expression "rule" ought to be avoided and be replaced by a more neutral term, like "recommendation" or "advice".

Methodological recommendations can either be formulated on a general level or on a more specific level, referring to a particular branch of science. I have tried elsewhere to give a list of general methodological recommendations for such theories and empirical hypotheses to which the Sneedian formalism applies.[36] I do not want to enter into the question of the value of such recommendations to a scientist. Suffice it to say that they *may* be, but *need not* be profitable. Certainly, in the days of crisis the more specific they are, the less they benefit. In that case they presumably consist of such methods which have helped fill in the details of the old theory; but there is no guarantee that they will benefit the (replacing) new theory in a similar way. This follows trivially, because 'methodological rules' in my sense are merely rules of thumb whose observance may be reasonable in some cases and unreasonable in others.

All these remarks imply a rejection of Lakatos' notion of rational reconstruction, for according to his conception, a rational reconstruction is an interpretation in terms of methodological rules. Such a project would amount to the recommendation that historical phenomena should be studied on the basis of a 'philosophy of as-if', or even worse, of a philosophy of wishful thinking: we should interpret the behaviour of people *as if* they would have been guided by methodological rules which they *ought to* have followed.[37]

The conception of rational reconstruction which I have in mind is very different. It is free from the ambiguity of methodological rules as well as from the dubious kind of philosophy mentioned. Fortunately, I need not enter into an analysis of my conception because it very much coincides with the one elucidated by J. Hintikka, although in a different context.[38] According to this view, *a rational reconstruction is neither a mere descriptive nor a mere normative enterprise*, but an attempt to construct an *explanatory model* of certain aspects of human knowledge. While 'rational reconstruction' in Lakatos' sense is in permanent danger of forging historical phenomena, the main objective of my notion of 'rational reconstruction' is to improve the understanding of these very phenomena. If, instead, one seeks an interpretation in the light of *'criteria of rationality'* acquired by deep philosophical *'meditation'* one is forced to judge real processes by a *cliché of rationality* and will then be horrified by the magnitude of irrationality discovered in these processes.

I left untouched the problem of the *means* to a rational reconstruction. I do not even want to postulate that every rational reconstruction must, eo ipso, be a *logical* reconstruction. Thus, I would, e.g., grant without hesitation Kuhn's characterizations of normal science and of extraordinary research the status of a rational reconstruction. What I tried to do, here and elsewhere, was *to supplement* this way of rational reconstruction by analysing those aspects of these and similar phenomena which can be dealt with by formal logic. Taken together, these analyses constitute a *logical reconstruction*, or a 'rational reconstruction' in the narrower sense.

A 'mere historian' may object: Why not forego these logical aspects altogether? Because a satisfactory *general* answer to this question would require a wide-ranging justification of the logic of science (which can not be given here), I give only a *special* answer in terms of a fact and a hope. The *historical fact* consists of the incredible number of competing misinterpretations of Kuhn's book [8], from the first reviews through the recent 'paradigm debates'. Much of this could have been avoided by a proper use of logic and set theory. It is my *hope* that some of the logical reconstruction sketches given in this paper will contribute to a better understanding of the dynamic aspect of theories and to the erection of a stable bridge between the systematically and the historically (as well as psychologically) oriented philosophy of science.

NOTES

1 [7], p. 292.

2 J. D. Sneed [22]. I have tried to give somewhat simplified account in [25], English translation New York 1976.

3 This, by the way, is one of the reasons why I personally would prefer a semantic criterion of theoreticity to the present one if it were available. A semantic criterion would free the philosopher from making empirical claims.

4 For a more exact formulation see Sneed [22], pp. 38 ff., or Stegmüller [25], pp. 63 ff.

5 [7], in particular sections 2 and 3.

6 Sneed, in his book [22], puts special laws and special constraints together in one whole class, and uses them only in the description of the dynamic aspect. The "expanded core" is the result of the addition of these two items to the core. The stability of a theory with varying hypothetical laws is formally depicted as a process in which the core remains constant while the core expansions are permanently changing. The present reconstruction with laws interpreted as specializations of theory-elements goes back to a suggestion of Dr. Wolfgang Balzer, Munich.

7 This, by the way, is in accordance with the physicist's usage of language. They do not only speak of the *law* of gravity but of the *theory* of gravity.

8 For precise definitions see Sneed [24]. The definitions *D2)–D6)* contain the formal equivalents to the intuitively explained concepts of the present paper. Within the formal equivalent to the concept of *specialization* explicit care is taken not to postulate laws already excluded by the given core K.

9 The readers who are interested in the technical details should not overlook the fact that, as a rule, a net has *not* the structure of a tree. This is so because a 'downward closure' of the net arises in the case of different laws which are not specializations of each other but hold in the same domain.

10 Of course, this strong claim holds only if (1) the criterion of theoreticity creates what was called "the problem of theoretical terms" and (2) we don't know a way out of the difficulty other than the Ramsey-method. But observe that this presupposition is much weaker than stating Sneed's characterization of theoretical terms being correct!

11 Precise formal definitions of this function A are given in Sneed [24], *D3), B)* and *D11)*. The latter corresponds to a more complicated earlier definition making reference to what was called "expanded core".

12 Typically, the basic claim will be empty or 'almost empty'.

13 Since this very long and very complicated Ramsey-sentence can be written down *as if it were an atomic proposition*, I tentatively used the predicate "macrological", being applicable to set theoretical formulations of this kind and of similar kinds.

[14] In [25], p. 98 and p. 102, I tried to give an immediate translation of the *linguistic* Ramsey-version into ordinary language.
[15] For a more detailed analysis see my [25], chap. II, sec. 4: "What is a paradigm?"
[16] I can not enter into an analysis of what factors determine the kind of similarity referred to. It is sufficient here to point out that the decisive factor in the present case is not so much the intuition about similarity of things in general, but rather the assumptions of scientists, working with the core K and its specializations, concerning what features physical systems must have in common in order to become applications of these mathematical structures.
[17] I characterized the identity of a theory with the help of its components K_b and I_o. But what determines the identity of K_b? Kuhn has asked this question in his critical remarks on the new approach in [13]. The boundary line to be drawn between changes of normal science and scientific revolutions depends on what we include in or exclude from K_b. My general attitude to this question takes the following form: *First*, from a logical point of view one must often differentiate between *different* theories, where physicists as well as historians speak of one and the same theory. *Secondly*, in each case one does not have just *one* reason but many reasons for characterizing a specific theory in a particular way.

As an illustration of the first point, consider the example of Newton. It is necessary to avoid the definite article, speaking of *the* theory of Newton. Rather, one must distinguish between three Newtonian theories N_1, N_2 and N_3.

N_1, which I call *Newton's basic theory*, is the same as *classical particle mechanics in Newtonian formulation*. The additional clause "in Newtonian formulation" is necessary because what from the physicist's point of view is just 'another formulation' of the 'same' theory is, in truth, a different theory. (The Lagrange version, e.g., is a different theory because its fundamental theoretical concepts are different.) N_2, *Newtonian classical particle mechanics*, is obtained from N_1 by adding the actio-reactio-principle, i.e. the third law. Finally, N_3, the *theory of gravity*, follows from N_2 by adding two well-known additional features which together make up the so-called 'law of gravity'.

Concerning the second point, I distinguish between the following reasons: (1) *logico-mathematical*, (2) *empirical and/or pragmatic*, (3) *general epistemological*, (4) *specific epistemological reasons concerning the basic core of a theory*, (5) *historical*. The reason, e.g., given by McKinsey et al. in [7], why it is not necessary to introduce the third law into the axiomatization of classical particle mechanics falls in the class (1). Because the third law is not used in every application of classical particle mechanics, and because the law of gravity is not used in every application of Newtonian particle mechanics, all the three theories N_1, N_2 and N_3 should be kept apart from each other according to (2). Since the basic core of a theory must achieve some of the performances of the 'correspondence rules' in the framework of the earlier empiricists, the fundamental laws ought to be *junctures* of all occurring theoretical and non-theoretical concepts. This forms a type-(3)-argument in favour of distinguishing between N_1 and N_2, for only the second law, but not the third one (containing the force function only), is a juncture of the kind required. Furthermore, as I have already pointed out, exactly the fundamental laws of physics are 'almost empty'. This, again, applies to the second, but not to the third law, and can therefore be considered a type-(4) argument for separating N_1 and N_2. Finally, as we know from history, the 'Newtonian physics' came into trouble because of the incompatibility of the third law with the theory of electromagnetism. This does not prevent us from calling the physicists who became aware of this "Newtonians". So there exists even a type-(5) reason for distinguishing between N_1 and N_2. Taken together, various reasons may call for identifying a given theory in a certain way. In particular, as we have seen, many reasons point to making *the second law and only it* part of the basic theory of Newton's.

[18] For more details see Stegmüller [25], pp. 254–258. The reader should note that in order to represent the strong theory-proposition the original Sneedian formulation in terms of so-called expanded core was used and not the new version in terms of nets and their refinements.

[19] This concept of setback embraces the two kinds of events opposite to those described in the last but one paragraph.

[20] [14], p. 132.

[21] In a recent review of my book [25] in [3], W. Diederich raised the objection that the concept of theory dislodgement does not do justice to Kuhn's notion of scientific revolution. This criticism is based on the following misunderstanding: While the concept "holding a theory" was intended to contribute to an explication of the concept of normal science in Kuhn's sense, the phrase "theory-dislodgement" was not at all devised as a means to *explicate* the concept of scientific revolution. I am fully aware that presumably more than 95% of what can be said about this phenomenon does not belong to the domain of a logician. By the slogan "theory dislodgement" I only wanted to localize that aspect of the phenomenon which gave offence to so many critics of Kuhn and which, therefore, needed a particular analysis and elucidation.

[22] Apart from the requirement that the mass- and the force-function stands in a formal relation to the second derivative of the position function with respect to time, the second law does not contain any restrictions. It does not say anything about the form of the force-functions and force-laws in the various applications. Nor does it contain a clue to the constraints 'cross-connecting' different applications. Finally, it leaves completely open how it may be specialized and what can be included into its range of application.

[23] [17], part 1 and 2.

[24] On this point, see the article of A. Grünbaum: 'Is Falsifiability the Touchstone of Scientific Rationality? Karl Popper versus Inductivism', to appear.

[25] See P. Feyerabend [4], p. 77.

[26] I have elaborated this idea in more detail in [28] by using a possible-world picture.

[27] See Sneed [22], pp. 216–248; for a simplified version Stegmüller, [25], pp. 144–152; and for an improved version Sneed [24], section 5. Although Sneed's article contains the most elegant solution to the problem of reduction known up to now, the reader should not ignore the fact that my above-mentioned reservation to the concept of Kuhn-theory is transferred to this concept because in his article Sneed renders precise reduction *between Kuhn-theories*.

[28] If, say, it should turn out that not even the partial possible models are comparable (e.g. because the theories of space and time underlying the reduced theory and the reducing theory radically differ) the problem of reducing would first have to be tackled 'one level deeper' than now.

[29] This sketch must not be understood as if one would compare the two theories from an 'external Archimedian point of view'. Rather, I tacitly presupposed that everything is judged *from the vantage point of the new theory*.

[30] See [29], [30] and [31], first half-volume.

[31] [5], p. 256.

[32] See [8], Postscript to the second edition.

[33] The similarities between Kuhn and Wittgenstein are often reflected in identical words, although Kuhn had no knowledge of Wittgenstein's manuscript at the time of his book's publication.

For the readers interested in the topic I give some paragraphs of [33] in which some similarities of the kind mentioned appear: 92, 94, 105, 298, 336, 341–344, 609–612, 669.

[34] The basic idea of the following sketch goes back to Moulines. But as I do not know to what extent he would agree with my formulations I am alone responsible for whatever mistakes may be found in the next paragraphs dealing with core-realization and theory-development.

[35] By this additional requirement those expansions of the secured domains are excluded which are considered 'senseless' or 'unreasonable' because they have no relation to elements of the paradigmatic set I_0.

[36] See [28], expanded version.

[37] In this respect I am, as far as I can see, in agreement with Kuhn; see his criticism of Lakatos in [11], in particular on p. 142 and p. 143.

[38] See J. Hintikka, 'Epistemic Logic and Philosophical Analysis', in [6], pp. 3–19, particularly p. 5f.

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