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ACCIDENTAL ('NON-SUBSTANTIAL') THEORY CHANGE AND THEORY DISLODGEMENT: TO WHAT EXTENT LOGIC CAN CONTRIBUTE TO A BETTER UNDERSTANDING OF CERTAIN PHENOMENA IN THE DYNAMICS OF THEORIES

1. THE TENSION BETWEEN SYSTEMATIC AND HISTORICAL APPROACHES OF SCIENCE

The philosophy of science as initiated and developed in this century first and foremost by empiricists and then by philosophers of other persuasions as well was purely systematic in its orientation. Increasing attention to the history of science and to the psychological and sociological aspects of its practice should have, one might have thus expected, meant a welcome addition to the logic of science and furnished the experts of all these disciplines with a host of reciprocally fruitful suggestions.

Whoever entertained such hopes was, however, in for a bitter disappointment.

In particular, with the appearance of T. S. Kuhn's work on scientific revolutions it became dreadfully clear that the results achieved in the different branches not only did not complement each other, they did not even yield a *consistent* overall picture of science. The fledgling student of the philosophy of science appeared to be faced with having to choose between two *incompatible* (and not merely incommensurable) paradigms: the logical or the psychological-historical. Those, who for systematic reasons were interested in topics such as measurement, explanation and prediction, or confirmation, appeared to be forced to ignore the results of historical research. And it looked as though those interested in the topics treated in Kuhn's book would have to forget all that they had heard in lectures and seminars on general themes such as truth, objective knowledge, or scientific rationality, as well as the more specific material gleaned in courses on inductive logic, hypothesis testing, and theory construction.

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Indeed, the situation was even somewhat more aggravated; for the discussion was being conducted at two different levels with the pendulums swinging in opposite directions at each level. At the more concrete level of the philosophy of science the tendency was more and more toward Kuhn's way of looking at things, presumably because many found this approach more convincing, and because it promised to be much more interesting, for it furnishes a dynamic view of one of the most fascinating phenomenon to be found on our planet, whereas the logic of science can furnish, at best, only static snapshots. (Most of the time it did not even do this, since the systematically oriented philosophers normally contented themselves with simplified fictitious examples instead of considering examples taken from actual science.) At the more abstract level of *general* epistemological investigations things looked entirely different. A number of penetrating thinkers attempted to show that even if Kuhn himself did not so intend it, his conception of the natural sciences inevitably leads to a form of subjectivism as well as of irrationalism and of relativism, and thus, to positions which for philosophical reasons are untenable, or even absurd. Indeed, Kuhn himself often emphasized that these *supposed* consequences of his ideas must be based on misunderstandings. Since, however, it was apparently not possible to pin down the sources of these misunderstandings, these critics either did not believe Kuhn, or, at least, remained skeptical of his assurances.

Thus, young philosophers of science were driven into a sort of intellectual schizophrenia. On one hand they found the Kuhnian approach uncommonly attractive, on the other, if they took Kuhn's critics seriously, they felt forced to regard it as in need of fundamental revision.

It has been my firm conviction for a long time now that this represents a wholly impossible situation, and that it is absolutely imperative to bridge the gap between the historically and the systematically oriented approaches instead of damning one or the other. The attempts in this direction to date, including the so-called 'theory of research programmes', appeared to me unsatisfactory for many reasons. It seemed to me that the primary task was to discover the source of the misunderstandings. Since this eluded me, I could not, for a long time, form any precise notion about the nature of the bridge being sought. Then, as I came across Sneed's book, something happened which may aptly be described in Kuhn's words about a new paradigm suddenly coming into focus. It became abruptly clear that at the bottom of Kuhn's theses lies a *theory concept* which is totally different from the one then current among philosophers of science.

For a long time, indeed, for *too* long a time, metamathematics has furnished the model to which philosophers of science turned and on which they attempted to pattern their investigations of theories. For logicians and metamathematicians it goes without saying that theories are classes of sentences. This interpretation had proven so fruitful for handling all problems in these areas that it was never questioned. Philosophers of science adopted this view of theories as a matter of course, and with it, the tacit assumption that in their discipline, too, the logical reconstruction of theories as classes of sentences would prove fruitful. Today, I no longer believe this assumption to be correct. We will gain a better understanding of scientific theories if we give up this statement view. In this connection I should like to follow Bar-Hillel in referring to the new conception positively as the *structuralistic conception of theories* instead of using the negative appellation non-statement view (although I am doing this with some hesitation in view of a widespread different use of the predicate 'structuralistic'). I hope this short autobiographical excursus will prove helpful in understanding what follows.

In order to forestall false expectations I should like to make two observations before going any further: (1) the task of a logical reconstruction also includes indicating the *limits* of what can be logically comprehended and explained. In regard to the problems at hand, I am convinced that these limits must be much more narrowly drawn than most 'empiricists' and 'rationalists' believe. One of my tasks will be to explain why this is so. (2) Although I will at times be dealing with special phenomena, including those which Kuhn calls 'normal science' and 'scientific revolutions', detailed analyses are not my primary goal here. First and foremost I want to deal with the objections mentioned above, such as irrationalism, subjectivism and relativism, thus contributing to the clarification of questions belonging to the abstract epistemological level. In order to illustrate my conception I will also be making some short remarks about a number of other topics such as 'holism', 'research programme', and the 'theory-ladenness of observations'. Those of you who have studied these topics intensively, please keep my purpose in mind and excuse the briefness of my remarks.

Since there are a number of different formal and non-formal directions in the philosophy of science, I would like to specify more closely which systematic approach will be taken here. I have purposely elected the one which presumably lies the furthest from Kuhn's, and is so dissimilar to it that one can scarcely imagine how the two can be brought into touch, namely, the axiomatic method as developed by Suppes. For simplicity's sake I will henceforth speak of the 'Suppes-approach'. The only thing which I was able to discover *common* to the approaches of Kuhn and Suppes was that both were the target of the most bitter attacks from philosophical quarters, even if for wholly different reasons. While Kuhn reaped the protests already mentioned, Suppes' procedure drew objections primarily on grounds that it is so abstract and so general that it precludes a discussion of a host of problems central to the philosophy of science such as the problems of theoretical terms, the role of conventions, empirical confirmation etc.[1] Essentially, these objections culminate in the challenge to specialize Suppes' method in such a way that the epistemological problems in question can be discussed. It appears to me that Sneed has achieved this to a great extent. Most remarkable is the fact that every single step in this specialization is a step away from the 'philosophy of science fiction' which many philosophers have set in the place of a philosophy of science. Such a fictive idea is, for example, the assumption that a physical theory must have some such thing as a 'cosmic application'. In part, though, many inadequacies of the classical systematic philosophy of science are due to the proclivity for introducing the theory concept after the fashion of metamathematics. Each one of the realistic, pragmatic steps taken by Sneed in the process of specializing the Suppes-approach constitutes simultaneously a step toward the dissolution of this orientation and the erection of a new pillar for the bridge leading to the historically oriented philosophy of science.

This successive disengagement from the logical and metamathematical ideal might be one of the reasons that Sneed's ideas have seemed so difficult to grasp, or even alienating.

At the risk of being repetitious I will again state the essential points. First, the idea of a single 'cosmic' application of a physical theory is scrapped in favor of the thesis that each such theory has *several partly overlapping applications*. Second, these intersections lead to the important *differentiation between laws and constraints*. While laws hold in some one, or possibly all applications, constraints establish more or less strong 'cross-connections' between the particular applications by ruling out certain combinations of theoretical function values. (The importance of this new concept of constraint has again been underscored by the investigations of C. U. Moulines in whose reconstruction both of the first two basic principles of thermodynamics prove to be constraints, not laws.[1a]) Third, it should be remembered that the *special laws* holding only for certain applications must be differentiated from the basic law which is to be incorporated into the core of a theory. (By means of his concepts of nets of theory cores, and theories, Sneed has shown that, and how, this differentiation can be iterated.) A fourth point appears to me to be of utmost importance, namely, the *theoretical – non-theoretical dichotomy*. I should like to remind you that this distinction is handled quite differently than it was within the framework of empiricism. The scientific language is not divided into a 'fully understandable observational language' and a theoretical language which is 'only partly interpretable' by means of correspondence rules. Instead, the theoretical terms of a theory T are distinguished on the basis of a criterion. The measurement of theoretical functions depends upon a successful application of just this theory T. Thus, one can say that these quantities are T-determinable, and one must henceforth speak of 'T-theoretical quantities', not simply 'theoretical quantities'. It appears to me that only in this way do we find an answer to what I have elsewhere called 'Putnam's challenge', namely, to show 'in what way theoretical terms come from theories'. The theoretical terms 'come from the theory' in the sense that their values are measured in a theory-dependent way. This leads to Sneed's problem of theoretical terms whose only known solution to date is the Ramsey-method. (For a simple illustration of this problem assume that the theory contains theoretical terms. Assume further that there is but one application of the theory to date and that we want to use the Suppes-method. Then, if S is the basic set-theoretic predicate axiomatizing the theory and the individual constant a designates this application, an empirical claim of this theory must have the form $a \in S$. Because of the particular nature of theoretical functions we must then *assume* this claim to be true should we want to test it. Thus, $a \in S$ cannot be an empirical claim, contrary to our intention.) The contrast to the traditional way of thinking becomes abundantly clear where two different physical theories T_1 and T_2 are formulated in the same language. One and the same term of this language can then be simultaneously theoretical and non-theoretical; i.e., theoretical in relation to T_1 and non-theoretical in relation to T_2.[2] Later I will consider a fifth point, *the method of paradigmatic examples.*

2. THE STRUCTURALISTIC THEORY CONCEPT: THEORIES, THEIR EMPIRICAL CLAIMS, HOLDING A THEORY

I would now like to examine more closely the new concept of scientific theories, the non-statement view, or positively stated, the structuralistic view of theories. Compared with traditional ideas identifying theories with classes of sentences, this structuralistic conception offers five important advantages: (1) With it a concept corresponding to Kuhn's notion of '*normal science*' can be introduced in an unforced natural way; a concept which dispels the appearance of irrationality surrounding Kuhn's notion. (2) With it a *concept of progress* can be introduced which also covers the revolutionary cases where one theory is dislodged by another theory having an entirely new conceptual apparatus. (3) The phenomenon of the *immunity of theories* to 'recalcitrant experience' can be made clear and understandable. (4) It permits an elegant simplification of what Lakatos intended his *theory of research programmes* to achieve while avoiding the difficulties and vagueness attached to this theory. (5) It removes the danger – and this is perhaps the main advantage – of falling into a rationality monism and, thus, into the *rationality rut* of assuming there could be but *one single* source of scientific rationality (e.g., by adhering to rules of inductive inference or of the falsification principle, or to certain methodological rules.)

Since, however, the structuralistic viewpoint has, to date, only been worked out for theories of mathematical physics, these five advantages can, at present, only be claimed in respect of such theories. But we can hope that these classes of theories prove to be a 'typical paradigmatic case', and that the structuralistic theory concept with all its advantages can also be extended to other scientific disciplines.

Our first explication aims at an interpretation of the concept of normal science. Thus, I would like to preface it with the remark that, indeed, as Sneed has already emphasized, the new theory concept may be used to elucidate Kuhn's idea. But this does not mean that Sneed and I originally intended to back up Kuhn against his critics, and that the new complicated conceptual apparatus was created solely for this purpose. I hope that Sneed's remarks have sufficed to indicate that this is not the case. All fundamental innovations stem from purely systematic problems and considerations. Thus, even if you should find the following considerations convincing, you should not conclude that the whole apparatus was invented for the benefit of a 'Kuhnian-hermeneutic'. Instead, you may take this as an additional indication of the importance and utility of this apparatus.

We now turn again to the central question of what may be understood as a *theory*. As you remember Sneed has already distinguished between

(1) a theory as an entity based on a theory element $\langle K, I \rangle$, whereby K is a theory core and I is a class of partial possible models, i.e. the set of intended applications, and

(2) the empirical claims of a theory, which have the form $I \in \mathbb{A}\,(N^*)$, which may be taken as an abbreviation for an array of claims of the following kind $I' \in \mathbb{A}\,(K^s)$.[3]

If it is asked whether (1) or (2) applies to *that which a scientist considers a theory*, the concrete answer in most cases will be neither. The scientist will be thinking of something much less abstract, something with flesh and blood involving people, their convictions and their knowledge. This third theory concept we will call *holding a theory*.

Perhaps the following analogy to the philosophy of language will help to make this somewhat clearer. Sneed occasionally characterized theories and empirical claims as *products*. Linguistic objects, words and sentences, can also be seen as products, and speech act theory has shown that the extremely important dimension of the performative modi is lost from this point of view. Similarly, theories and empirical claims stand in much the same relation to *acts of holding* a theory as do linguistic products to speech acts.

As helpful as this analogy might be in forming an initial approximation, it can be equally misleading if taken too literally. For example, a speech act is intrinsically something performed by only one person. Holding a theory, on the other hand, is rarely the act of one single person; normally it is a community act. Moreover, holding a theory must be characterized as a relatively complicated phenomenon involving numerous components each of which we would in turn call acts.

The concept of holding a theory can be introduced in a broader or in a narrower sense. Further, one can give it a more objective or a more subjective accent. Sneed and I have experimented with a number of different variants.[3a] For all these definitions one needs extra-logical concepts such as 'person', 'believes that', 'has supporting evidence for', as well as a variable t ranging over historical times. Instead of formal precision I will attempt here an intuitive gloss.[4] In order to introduce the *weak* objective variant, we will assume a theory T in the earlier sense to be given. The statement that a person p holds a theory T (in the weak sense) at time t (abbreviated $H_W(p, T, t)$) means that there is a net N based on spezializations of the core K of T such that p believes $I \in \mathbb{A}(N^*)$ at t, furthermore, that p has supporting evidence for this proposition, and finally that p believes N to be a *strongest* existing net such that $I \in \mathbb{A}(N^*)$. If one likes, one can also incorporate into this concept the person's belief that using this theory will yield progress. This can be rendered as follows: p believes at t that there is a specialization $K^{s'}$ of a K^s which is not yet in the net N, and which will yield a stronger net N' such that $I \in \mathbb{A}(N^{*\prime})$ and $\mathbb{A}(N^{*\prime}) \subset \mathbb{A}(N^*)$ (whereby '$\subset$' stands for *genuine* inclusion). The phrase 'has supporting evidence for' implicitly contains the confirmation problem. Since we are going to disregard this problem here, we will use the abbreviation 'p knows that Y' to mean 'p believes that Y and p has data supporting Y'.

3. 'NORMAL SCIENCE' AND 'SUBJECTIVISM'

Before sketching other variants, I would like to indicate how the concept of holding a theory can be used to explicate the notion of normal science. The idea is this: *if several persons hold the same theory*, they will be said *to belong to the same normal scientific tradition*. This means that the persons in question do indeed use the same theory to construct their hypotheses, but couple it to a variety of different convictions and assumptions. Thus, the theory T remains unchanged, while the empirical claims attached to it may change at any time. I would like to propose calling all those changes not involving the theory itself *accidental theory changes*, since one can draw an illuminating comparison with the ancient substance-accident dichotomy: the core $K = \langle M_p, M_{pp}, r, M, C \rangle$ is the immutable substance underlying change, while the core specializations K^s represent the constantly changing accidents.

But now I am anticipating. To actually arrive at a viable concept of normal science in Kuhn's sense, several important factors must still be considered. To this end I must briefly say something about the concept of *paradigm*. What Wittgenstein had in mind and wanted to illustrate with the example 'game' was, if I may simplify a bit, as follows: neither an explicit extensional characterization of the predicate G for *game* via a listing *of all* games, nor a precise definition of G by the stipulation of sufficient and necessary conditions for membership in G is possible. Instead, we must limit ourselves to effectively specifying a sub-set G_0 of G, the *list of paradigms* (or *paradigmatic examples*) *of games*. Elements will then be admitted to the difference set $G - G_0$ only if they exhibit a significant number of properties common to most elements of G_0. This formulation underscores the irremediable vagueness adhering to a set determined by paradigmatic examples. But please note that this does not make the paradigm concept itself vague. Indeed, this concept enables us to make *precise* statements about a kind of vagueness characteristic of physical theories.

In our case, though, the general concept of a paradigm will only be used for a very special purpose: The set I of intended applications of a theory is not completely specified from the very beginning by means of a list or a strict definition; it is an *open* set for which the theory's creator has stipulated a subset I_0 of paradigmatic examples, and which can be changed (through additions and cuts) in the course of working with the theory provided the condition $I_0 \subseteq I$ is met. Thus, for instance, Newton specified the paradigms for the application of his theory by designating examples such as the solar system, certain parts thereof, the tides, pendulum motion, and free falling bodies near the earth's surface.

This idea can be utilized for our explication attempt as follows:[5] we expand the present theory concept to a concept of a theory in the strong sense, i.e. to what Sneed called a Kuhn-theory, and require that there once existed a person p_0 who at time t_0 successfully applied the core K of the theory, i.e., $I_0 \in \mathbb{A}(K)$. We then modify the concept of holding a theory by taking 'theory' to mean this strong theory concept and requiring that the person p holding the theory also choose the set I_0 as the set of paradigms for I. This establishes the *historical source* of the theory in its inventor, as well as the *paradigm* concept and the *historical continuity* between all those persons holding the theory. The resulting *concept of holding a theory in the strong sense*, $H_{st}(p, T, t)$, could be taken as an explication of the Kuhnian concept of normal science, at least of its objective variant.

This objective variant can be replaced by a subjective one. The only difference is that *some existential quantifiers and the epistemic operators 'believes that' and 'knows that' are switched around.* Whereas previously we always spoke of the existence of a net N and a set I about which the person p knows or believes something, we now say that p knows of a net N and a set I that $I_0 \subseteqq I$ and $I \in \mathbb{A}(N^*)$, and that as far as p knows N is a strongest net and I a largest set of its kind. In this definition the only remaining objective entity is the core K itself. If under 'subjectivism' nothing other is meant than a philosophical temperament which prefers this subjective to the objective variant, this would be a quite viable subjectivism.[6] That this represents a realistic interpretation can be illustrated by analogy to a remark made by Wittgenstein. Words and sentences, so said Wittgenstein, appear as such to be dead. Where, he asks, do they get life? And he answers: through use. Similarly, the question as to where a theory gets life could be answered by saying through the persons and communities holding it.

4. RATIONALITY AND PROGRESS BRANCHING IN NORMAL SCIENCE

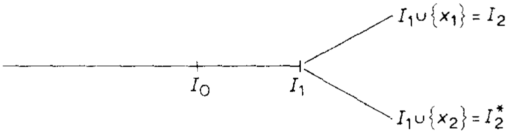
What about the rationality of the normal scientist? In principle this question is easy to answer; indeed, without having to go into the problem of whether, and how, criteria for scientific rationality can be formulated. For whether these criteria are inductivistic, deductivistic or something else, their satisfaction, or violation, pertains only to empirical claims, and thus turns completely on the word 'knows'. In any case the normal scientist, i.e., the scientist holding a given theory, *can* satisfy any of these rationality criteria.

In his well-known critical article about Kuhn, Popper says: "In my view the 'normal' scientist, as Kuhn describes him, is a person one ought to be sorry for. . . . The 'normal' scientist, as described by Kuhn, has been badly taught. He has been taught in a dogmatic spirit: he is a victim of indoctrination."[7] I hope I have made it clear why I take this to be a faulty interpretation. This kind of objection is no longer justified if one of the reconstructions sketched here is taken as a basis. One could underscore this by giving the concept 'holding a theory' the nickname 'normal science without dangers'.[8]

Normal science allows for *two sorts of progress*. One consists in expanding the set of intended applications, the other in further specializations of the core K. The first obtains when $I_t \subset I_{t+1}$, the latter, when $I_t = I_{t+1} \in \mathbb{A}(N^*_{t+1})$ with $N_{t+1} \sqsubset N_t$.[9]

Their counterparts are the corresponding types of *setbacks* which a normal scientist oftèn experiences; namely, being forced to retract an attempted expansion of the range of application or an attempted core specialization. Every such setback is the result of an empirical refutation of a hypothetical assumption. The crucial point is, though, that the theory itself is not adversely affected by such setbacks. Responsibility for these failures, if sought, is not to be found in the theory, but lies with the researchers who were not successful in attempting to strengthen the empirical claims formulated with the help of the theory.

I would also like to mention two interesting complications. Wherever we have used a superlative to characterize the concept of holding a theory, it was always with the *indefinite*, not the definite article. The reason lies in the following possibility:



The branching is intended to indicate that a given core specialization K^s is applicable either to I_2 or to I_2^* but *not* to $I_1 \cup \{x_1\} \cup \{x_2\}$; i.e., this set is not an element of $\mathbb{A}(K^{s*})$. In such a situation neither the theory, nor experience, nor logical reasoning can help. The scientist must *decide* on the basis of value judgements.

Another kind of branching is also possible. The scientist can at a given time be faced with the choice of either expanding his current range of application I at the expense of additional expansions of his net, or leaving I unchanged in order to gain a further core specialization.

Typical situations of this kind could be called *progress branching in normal science.* In these branches we have located a juncture where value judgments are unavoidable in deciding which way to proceed. Should someone regard *this* as subjectivism, the *only* correct reply is that *this is a species of subjectivism which we can not evade.*

5. HOLISM OF EMPIRICAL CLAIMS. RESEARCH PROGRAMMES. THE THEORY-LADENNESS OF OBSERVATIONS

In connection with this reconstruction sketch for the concept of normal science, I should like to make a few short remarks about three concepts which appear often in the current literature. The expression '*holism*' can be used in relation to theories as well as empirical hypotheses. The question of the justification for using it in relation to theories we will put aside for a moment. In relation to empirical content the holistic standpoint can be formulated as follows (and this is a true statement): *the empirical content of a theory at a certain time is not exhibited by numerous special hypotheses constructed on the basis of this theory, but by one single big empirical claim*: $I_t \in \mathbb{A}(N_t^*)$.

It is a bit difficult to say something brief as well as substantial and correct about the concept of *research programmes.* As a *normative* concept it represents the result of a reaction, namely, Popper's critical reaction to Kuhn's notion of normal science. Since in our estimation this reaction rests on a faulty interpretation, we can disregard this normative aspect. If we then purge the characterization of this concept of certain contradictions and obscurities, we get something generally quite similar to our explication of holding a theory. But something is missing here; namely, the method of paradigmatic examples. On the other hand Lakatos certainly intended to include the confirmation aspect, which we decided to leave out, and also to account for certain other features which might be summed up under the heading 'scientific strategy'. Had history been such that the words 'research programme' were used in this way before the expression 'normal science', it would have been quite natural to propose a slightly modified version of the concept of holding a theory as an explication for 'research programme'.[10]

These rather incomplete remarks might be better understood in the light of some observations about the relationship between Popper and

Lakatos. For I see this relationship from a quite different perspective than most philosophers. Very often one reads something to the effect that *Lakatos has improved and extended Popper's methodology*. In my opinion, though, these two theories can scarcely be compared with each other. In order to see this, one must keep in mind the very different problems to which these two philosophers addressed themselves. Popper was out to cut down the identification of empirical science with inductive science.[11] To this end he attempted a purely deductive solution to the Hume-problem. The concept of the inductive confirmation of a hypothesis was to be replaced by a concept of deductive confirmation called *corroboration*. And non-inductive *test rules*, whose key concept was *falsification*, were to take the place of inductively motivated rules governing the acceptance and rejection of hypotheses. In the language of our present terminology, Popper was not at all concerned with *theories*, but with the evaluation and testing of empirical *hypotheses* based on theories.

Lakatos, on the other hand, was concerned with *theories* themselves – the word 'theories' being taken here in our sense. But he inherited the Popperian interpretation of the phenomenon 'normal science' and with it the shock which this interpretation produced. This finds its expression in the well-known allegation that in Kuhn's hands the philosophy of science becomes 'mob-psychology'. His '*methodology of research programmes*' was designed to help give these apparently irrational processes a rational interpretation. An 'extension' of Popper's ideas could at most be spoken of in the sense that Lakatos employed notions stemming from Popper's theory, as witnessed by the use of phrases like 'excess-corroboration',[12] and, thus, accepted their explications as satisfactory. To put it briefly, Popper was concerned with a deductive theory of corroboration and testing, Lakatos with a 'rationalization' of the concept of normal science. Popper wanted to solve the Hume-problem, Lakatos wanted to tow us out of the mob-psychology swamp. The normative components which Lakatos built into his theory stem entirely from the Popperian reaction to the phenomenon of normal science, a reaction which in our estimation rests on a faulty interpretation. And with them, incidentally, Lakatos inherited the unsettling task of *formulating the rules of this normative methodology*, which was actually never done.

Of special interest to us here is the fact that a new concept of rational reconstruction comes to light, namely, the *philosophy of the as-if* already mentioned at the outset. It construes the scientist's behavior as '*rational*' by viewing it *as if it were dictated by methodological rules.*

Such an appended philosophy is for me superfluous, since I contest the presupposition on which it is based. This is also the deeper reason behind the wholly different reconstruction concept I employ. A logical reconstruction in the present case should not, in my opinion, have to give a rational account of certain behavior by resorting to norms or '*methodological* rules', whatever that might be. Its sole purpose is to contribute to the understanding of this behavior by bringing *logical* concepts to bear.[13]

My rejection of the notion of reconstruction based on methodological rules could be easily misunderstood. I do not mean to deny that one can attempt to compile and order a list of *sensible methodological recommendations*, whether these recommendations are thought for the most part to be actually followed, or whether they should serve to guide scientists. Such a list might suggest, for example, the following:

(I) evaluation criteria for judging empirical hypotheses (confirmation criteria plus the value standards mentioned by Kuhn plus possibly still other dimensions for judging);

(II) seeking new special laws as specializations of the basic predicate when applying (I) results in a rejection;

(III) seeking new special constraints in case (II) does not work;

(IV) a change in individual domains and/or functions where these are not extensionally fixed and (II) and (III) do not help;

(V) varying the set $I{-}I_0$ when (II) and (IV) do not suffice;

(VI) starting 'as low as possible' by all recommended manipulations of the theory net. (This represents a certain interpretation of Quine's idea of making changes between the center and the periphery of a theory before revising the center itself.)

Concerning the last item, the so-called *theory-ladenness of observations*, I would like only to point out an equivocation which has caused much confusion. It is maintained, for example, that the description of the facts relevant for a theory, itself requires a theory. This is in most cases true. But it is also harmless, and creates no special problem. For the theory required is, of course, not the same as that *for which* the facts are being described, but a more elementary, underlying theory. This situation does, however, raise a difficult problem concerning the *inter-theoretical relation of the hierarchal ordering of theories.* Some authors want, though, to maintain something much stronger than this, namely, that the facts for a theory T are determined *by this theory itself*. This appears to be the meaning of such phrases as 'theories define their own facts'. Yet even this stronger version of the thesis is not only intelligible, but correct *if confined to T-theoretical terms*. And in this case it creates another serious problem, namely, the problem of theoretical terms as formulated by Sneed for which only the Ramsey-solution is currently known.[14]

6. THEORY DISLODGMENT WITHOUT FALSIFICATION. THE THREEFOLD IMMUNITY OF THEORIES. THEORY CHOICE AND RATIONALITY

The concept of holding a theory, which served to define the notion of normal science, or at least an important aspect of this notion, was only the first step toward de-irrationalizing the current image of the Kuhnian conception of science. Now what about scientific revolutions? Can the logician also contribute to a better understanding of *this* phenomenon? Here I should like to begin with a confession so that you will not be too terribly disappointed with the following remarks: the logician can actually accomplish far less in this case than in the case of the phenomenon which Kuhn called normal science. This is not because, as Kuhn's critics think, scientific revolutions are in fact thoroughly irrational processes in the sense that they can only be comprehended with the help of concepts similar to those used to describe religious and political upheavals, but simply because many aspects of these phenomena, and indeed the most interesting ones, lie outside the competence of the logician. In particular, I am convinced that it is impossible to define a concept which would stand in the same relation to the phenomenon of scientific revolutions in Kuhn's sense as does the concept of holding a theory to the notion of normal science.

First we will try to characterize that aspect of the phenomenon described by Kuhn which again shocked many readers and led to charges of irrationalism, subjectivism and this time relativism as well. All empiricist philosophers, and the modern rationalists too, agreed until quite recently that a theory which founders on experience must be discarded. It seemed impossible to dispute this elementary fact. Establishing an empirical refutation or falsification also seemed to present no fundamental difficulties. It merely required a certain modicum of intellectual honesty on the part of the scientist such as, for example, not to challenge the data and not to take refuge in *ad hoc* hypotheses designed solely to rescue a theory.

As opposed to this, Kuhn's thesis, to put it somewhat simply, is that even a theory plagued with ever so many anomalies, i.e., a theory caught in a crisis, is not discarded because it has foundered on experience. Instead, it is jettisoned only when another theory is available to take its place. This prima facie curious phenomenon may be called 'theory dislodgment by a superseding theory', or briefly 'theory dislodgment'. As I already mentioned, it cannot in this case be the logician who, in analogy to the previous case, furnishes a precise explication for this notion. He must instead content himself with investigating *whether, and how, such a phenomenon can be reconciled with our conception of science as a rational enterprise.* As shown by the discussion up to this point, it is not easy to steer our thinking between the Scylla of a teleological metaphysics and the Charybdis of relativism. I will, however, attempt to show that in this case, too, the structuralistic view of theories enables us first, to gain a basic logical understanding of the situation; second, to produce a plan for a viable concept of scientific progress in revolutionary changes; and thus, third, to deliver something like a logical test for the correctness of Kuhn's thesis (limited at present to theories of the kind being considered here). Concerning the first point we must focus our attention upon a certain particular aspect of theories, namely, their *steadfastness in the face of 'recalcitrant data'*. This is expressly emphasized by Kuhn and felt by many to be especially shocking.

Such an immunity does actually exist and, indeed, in three different respects. The by far most important of these, the *first kind of immunity*, arises when one considers the relationship between the *core* of a theory and the *specializations* of this core. As you will remember, the empirical claims of a theory with the core K have the form: $I_t \in \mathbb{A}(N_t^*)$ (whereby 't' is a historical time index and 'N_t' denotes the net under K used at t). As an empirical claim this 'central empirical statement' of a theory (or Ramsey-Sneed-claim as I call it) can be refuted by experience. This refutation does not, however, directly effect the theory itself, for the falsification of the empirical claim only proves that *certain* specializations of the core (in the original version, *a certain* core expansion) are not suitable. Indeed, *the* *same holds for every finite number of such unsuccessful attempts.* We can, by paralleling Popper's argument proving the non-verifiability of strict universal quantifications, obtain the following proof for the unrefutability of a theory on the basis of a finite number of refuted empirical claims: *since the number of possible specializations of a theory-element is potentially infinite, no number, be it ever so large, of unsuccessful attempts to specialize a given theory-element can be considered conclusive proof that a successful specialization of this element is impossible.* Thus, we are not forced to give up the theory; there might just be a still undiscovered specialization which would prove successful when discovered.

We need only one more factor, which in most cases represent an elementary historical truth, in order to give Kuhn's metaphor about the poor carpenter a completely unstrained natural interpretation. If we place ourselves in the 'normal scientist's' situation, i.e., in the situation of one who already holds a theory, we will realize that this scientist is always working with theories whose cores have in the past repeatedly served well. When, therefore, a member of the community is not successful in working with the core, it is natural and understandable that he, and not the theory, be blamed. If he himself then chucks the theory without being able to offer a better alternative, he is in fact behaving 'like a poor carpenter who blames his tools.' The above argument in support of the first type of theory immunity simply serves to analyze logically the state of affairs by virtue of which this metaphor aptly describes our situation. Kuhn's remark to the effect that in normal science *persons*, not *theories*, are tested, also loses its paradoxical appearance in view of these circumstances.

At this point some will object that an *instrumentalistic conception* of scientific theories underlies this line of thought. In principle I have nothing against using the label 'instrumentalism'. But it should be pointed out that the expression refers here to something quite different than is usually so referred to (and is described, for example, in a well-known book of Prof. I. Scheffler's[15]). For according to the present conception theories are not instruments for creating fictitious pictures of reality, but tools for making empirical claims about reality.

I hope that you are gradually starting to get the feeling that the theory concept being offered here is more adequate than the traditional identification of theories with classes of sentences. The proposed reconstruction of the notion of normal science was *not only* intended to contribute to the interpretation of Kuhn's analyses. For quite aside from what a historian of science or historically oriented philosopher of science might tell us, *it appears the most natural thing in the world to say that the Newtonians held the same theory although they allied it with wholly different convictions and assumptions.* That the present notion of a theory is also simpler than others is shown, for example, by the fact that no complicated additional constructions such as Lakatos' 'protective belt of auxiliary hypotheses' is needed for demonstrating the immunity of theories to potential falsification. All too long philosophers have been influenced by the analogy to everyday generalizations when it came to reflecting about the output of science. With this analogy one cannot even formulate the simple idea mentioned above. We ought, therefore, to be prepared to admit that this analogy is no longer adequate as soon as science has reached the *stage of theory construction.*

Many philosophers have thought that the description of the phenomenon of theory dislodgment indicated a rationality gap. In particular, Kuhn's thesis that the decision to scrap a theory *is always simultaneously a decision in favor of a new theory* was thought to imply something irrational and, thus, logically incomprehensible.[16] To support this, their second charge of irrationalism, they argued that there must be something like a *critical level* at which a theory must be rejected regardless of whether a new one is available or not. Here again we clearly discern the influence of the statement view, namely in the attempt to put scientific theories in the same category with *statistical* hypotheses, if not deterministic laws, and consequently demand an analogue to the critical level of a statistical test.[17] Even Lakatos' attempt to stipulate criteria for the 'degeneration' of a research programme can be seen as an effort to establish such a parallel. And with it, incidentally, he was saddled with the problem of setting a time limit – a problem first seen by P. Feyerabend and, in my opinion, unsolvable within Lakatos' conceptual framework. Our conception faces no such difficulty.

All of these liberalizations of the original falsificationist programme are ideal. As the brief logical analysis has shown, there is no critical level at which a theory must be discarded or a research programme starts to degenerate. Stipulating such a level would be a purely arbitrary act.

Concerning the second half of Kuhn's thesis we need only add a psychological truism to the immunity already established in order to comprehend this situation, namely, the elementary insight that people do not throw away tools which have served well as long as they possess no better substitutes. Or to take a more drastic example which better depicts the situation of a crisis-ridden scientific theory: would someone who was freezing not seek shelter in a hut simply because it was awfully ramshackle? Were he not to, this would mean he prefers sure death to mere danger.

That the philosopher has overstepped his competence here by resorting to *empirical generalizations* is a charge I could not accept. The situation seems to me similar to that of the ordinary-language philosopher. He does not base his analyses on statistical surveys concerning the use of language, but on his own linguistic competence. In respect of psychological truisms like the one just mentioned, we have at least the same degree of competence. As a *human being* I am competent to evaluate certain reactions as *typically human* without having to resort to generalizations, which properly ought to be called 'hypothetical'. And I am not faced with a contradiction should I at some time learn of beings whose behavior is just the reverse of the hypothetical wanderer in the last example. For I would not then conclude that I must change my opinion about psychological truisms, but rather that the incomprehensible behavior of these beings must rest on the fact that they are either insane, or influenced by an ideology incomprehensible to me, or not human beings at all.

These two forms of understanding, based on the linguistic competence of the native speaker and on the competence of our ('non-hypothetical') judgments concerning spontaneous human reactions respectively, could be called *elementary hermeneutic understanding*. For it must well be these which are meant when hermeneutics so strongly emphasizes the uniqueness of the methods employed in the humanities vis-à-vis the methods of the natural sciences. If we accept this choice of terminology, we can say that in order to gain an accurate grasp of the phenomenon of theory dislodgment, logical and hermeneutic understanding must work together (whereby the former is complicated, the latter elementary).[18] This point is also important in another respect, for here we have a typical case where acquiring understanding remains superior to any attempt at historical explanation.

I have dwelt on this point at some length because it is exemplary of how logical analysis plus elementary historical observations plus psychological truisms (elementary hermenêutic understanding) contribute to the clarification of a type of situation which we find over and over again in the history of science.

All of these considerations do *not*, of course, preclude the possibility that at some time a scientific community might agree to suspend work with a current theory as a 'hopeless undertaking' even though no new, more promising theory has been found. Our reflections should also serve to indicate why this seldom or never happens.

Such cases must, however, be carefully distinguished from those situations in which a negative decision is made in relation to certain parts of the range of application of a theory. In order to see this clearly we must take a look at the *second kind of theory immunity*. To this end we recall once again the relationship between I_0 and I. I_0 is the explicitly given extensional subset of I consisting of the paradigmatic examples. This subset can never be changed. With the exception of this minimal requirement, I is an open set. Should the scientific community in trying to apply the theory to some $a \in I - I_0$ experience fundamental difficulties, stretching perhaps over generations of research, and thus, conclude that this application is not possible, it need not, *contrary to falsificationism*, hold the theory responsible. Instead, it can decide to deny a's membership in the theory's range of application, i.e., to maintain $a \notin I - I_0$.

Here we come in sharp conflict with the demands of the 'critical rationalists'. For according to their notion of critical attitude, a scientist should make his theory *as sensitive as possible* to potential refutation. In our present case that would mean stipulating sharp criteria for membership in I and, consequently, rejecting the theory. I would like to counter this demand with the following observation: it appears that no physicist (and presumably no scientist in general) has ever been willing to assume the risk of falsification involved in explicitly defining I's extension, i.e., in stipulating sufficient and necessary conditions for membership in I. To oppose this reluctance on the part of scientists does not mean making their practice more rational. It would merely represent an attempt to remold scientific practice to fit a preconceived and extravagant rationality cliché. When optical phenomena could not be explained with the help of Newton's particle mechanics, as he hoped they could, his work was not pronounced invalid; instead it was concluded in accord with Maxwell's conception that light did not consist of particles. As I have not approached science via a preconceived overall conception of scientific rationality I am unable to perceive anything irrational in such a decision.

But even those willing to accept in principle what has been said so far might still raise the following objection: we have continually assumed that a theory's difficulties concern only *genuine* specializations of the core, not the core itself, or that they appear in connection with applications not belonging to the paradigmatic set I_0. But what happens when the fundamental law of the theory fails in I_0? Here we meet the *third kind of immunity of theories*. It is a consequence of two particular features of physical theories: first, the occurrence of theoretical terms (in Sneed's sense) in the fundamental law and, thus, in the core, and second, what I have called the holism of empirical claims. Instead of analyzing the general case we will illustrate with a simple example. Assume we agree to take Newton's second law as the only fundamental law of his theory. Then accepting this law means nothing more than being committed to promise that suitable force and mass functions satisfying this law exist in all intended applications, functions, which take special forms in certain applications and are connected across these applications by certain constraints. It is the near vacuousness of this wide-ranging promise, and not the supposed 'tautological' or 'purely definitional' character of this law, which precludes its empirical refutation.

7. THEORY HOLISM AND 'PROPAGANDA'. THE ROLE OF VALUE JUDGMENTS

In view of these three forms of irrefutability it is understandable that even a theory caught in a crisis will almost always be retained until a promising new theory is constructed. Here another viable form of *holism* takes its place alongside the holism of empirical claims. This '*theory holism*' could be briefly formulated as follows: the decision to accept a theory is always *an all-or-nothing-decision*, and it *cannot be replaced by any rules nor dictated by a so-called experimentum crucis.*[19]

That the value judgments, persuasion, and 'propaganda' play a decisive role in spreading new theories also becomes understandable, when one remembers that due to the limits of human intellectual and experimental capacities only *partial* achievements may be expected from each new theory at its inception. In this respect I would like to add the following to Kuhn's observations: (1) It is above all the goal that is decisive in determining the rationality of an endeavor. And the goal of those seeking to spread a new theory is to replace an old theory with a more serviceable one. Thus, their efforts may be regarded as rational even though they have only incomplete arguments or none at all. In the beginning *they themselves* draw the main energy for their work from nothing other than the *hope*, by no means, however, the guarantee, of attaining the sought after goal. What can be said against the attempt to transmit the ray of hope to others so that with combined effort the goal may be more quickly reached? (2) The phrase 'to influence with propaganda' should not be used indifferently. Between political propaganda via emotional agitation and psychic manipulation of the masses, and 'propaganda with the help of experiments' there is a difference similar to that between the former and the efforts of a music connoisseur to win another music lover over to an appreciation of certain compositions in which he has shown scarcely any interest. It is only the first member of this pair that can be treated as the subject-matter of 'mob psychology'. (3) The following point appears especially important to me. Even though the new theory initially has only partial successes to show (as, e.g., the oxygen theory once did vis-à-vis the phlogiston theory), and a decision in its favor is made on the basis of individual or shared value judgments as well as more or less vague intuitions, this cannot be seen as a symptom for the 'hopeless subjectivity' of theory choice. If the new theory proves successful, all of these value judgments and intuitions prove in retrospect to have been *provisional* and are 'de-subjectivized' to the extent of these successes.[20]

We may even go a step further. The *rationality* of a group of people no more rests on their actual accomplishments than does the *morality* of a person depend on actual success. In both cases the intention is what counts. And when those striving to establish a more serviceable theory are very often, perhaps even most of the time, not rewarded with success,[21] when, that is, *failure is much more frequent than success*, this does not prove in retrospect the irrationality of their action; at most it is a reminder of how great the professional risks are for those engaged in extraordinary research.

8. OVERCOMING THE RELATIVISM CHARGE. PROGRESSIVE REVOLUTIONS

Following this attempt to de-irrationalize and de-subjectivize the current picture of Kuhn's conception of science, I must now take up the *charge of relativism.* It seems to contain a grain of truth. (In my book[22] I chose an unfortunate expression when I spoke of 'rationality gaps' in Kuhn's portrayal of scientific revolutions. I now find this misleading because it created the impression that I was joining the chorus of those raising the irrationality objection. But this was precisely the opposite of my intention. What I meant might more aptly be called a 'missing link in the portrayal').

For the sake of illustration I will briefly formulate the relativism charge against Kuhn as sharply as possible: "An actual change of theories is described by Kuhn in sociological-psychological language. In this way one gets the impression of a complete parallel with religious and political power struggles. On p. 166 of his book the 'progressives' are even expressly identified with the 'victors' in a paradigm struggle. Assume this is to be taken literally. Assume further that to this picture be added the Kuhnian thesis of the incommensurability of the old and the new theories. All this adds up to *relativism.*"

We want, now, to illustrate this objection with the following possible-world picture: Let T_1 and T_2 be two theories designed to solve the same kinds of problems but having different theoretical terms. Initially theory T_1 prevails in the possible world w_1, but is subsequently dislodged by theory T_2 after having run into a crisis. Exactly the opposite transpires in possible world w_2. Here T_2 prevails initially, is beset by crisis and subsequently dislodged by T_1. One can well imagine this happening given different psychological and sociological conditions suitable to each case. In both worlds the proponents of the new theory are convinced they have brought progress. Assuming the incommensurability thesis is right, we cannot compare the two theories with each other. Thus, left alone with the psychological-sociological progress criterion, we must admit *that in both worlds there has been, by definition, progress.*

This appears fully unacceptable to us. 'Revolutionary progress' *cannot designate a symmetrical relation.* Whoever denies this and is prepared to maintain that progress occurred in both worlds may rightly be labeled an epistemological *relativist*. Presumably even those would be right who insist that were this possible and were we not able to differentiate between two cases of this type, then we must cease calling science a rational enterprise.

It is important not to misunderstand my point. I do not deny that two such events could *happen*, nor that in both cases those involved *believe* progress has been served. My thesis is simply that in at least one of these two worlds the agents of the *alleged* progress must be mistaken; real progress can only have taken place in one of the two worlds.

Thus we have located the heart of the problem. The 'missing link' by Kuhn is not a 'critical rejection level';[23] it is the *introduction of an adequate concept of scientific progress for the case of theory dislodgment*. Only in this way, it appears to me, can we avoid what I above called the Scylla of teleological metaphysics and the Charybdis of relativism.

Today I believe that Kuhn himself wanted to point out this difficulty. The last pages of his book can be understood as a challenge to the systematic philosophy of science to come up with a viable concept of scientific progress not infected with a teleological metaphysics as is, for example, the concept of increasing verisimilitude. In my opinion Sneed's reduction concept offers the best start in this direction to date, if not yet the final solution. It also appears to me, as I have elsewhere attempted to show,[24] that the late Prof. Lakatos had something similar in mind with his concept of sophisticated falsification. Hence, I consider this latter concept, apart from its somewhat misleading label, much more important than the concept of research programme in its normative sense.

Sneed's reduction concept is especially interesting because it permits *a comparison of theories with fully heterogeneous conceptual apparatus*. Must not at least the incommensurability thesis be sacrificed? No, it suffices to relativize it to the statement view, in which case it becomes not only a plausible but, indeed, for most interesting cases a demonstrably correct statement, since T_1-theoretical terms cannot be expressed in the language of T_2-theoretical terms and vice versa.

In cases of radical theory change a complete reduction is presumably not possible. Here one must be satisfied with the notion of the *approximative imbedding* of one theory in another. Sneed's reduction concept must be correspondingly expanded and liberalized. Concerning this extraordinarily important approximation problem there is, as far as I know, only one single interesting preliminary work. It appears as yet to have attracted little attention in the philosophy of science although it stems from an internationally known quantum physicist. I mean a book from Günther Ludwig.[25] Ludwig works with '*blurred*' functions and relations, indeed, even with '*blurred*' objects. This idea may be successfully incorporated into the Sneedian conceptual apparatus as my collaborator and former student Moulines has shown. In this way it becomes possible to work with *blurred* models and possible models, even with *blurred* partial possible models.[26] Nevertheless I must admit that this is not yet a fully developed theory. It is at the moment merely a 'metatheoretical research programme' *in statu nascendi*.

I just mentioned that in my estimation Lakatos' concept of sophisticated falsification is more important than his concept of research programme. In introducing this concept he must have realized for the first time that the 'missing link' lies in the interpretation of 'revolutionary progress' and not in the specification of a critical level, be it of rejection, or be it for determining when a research programme is degenerating.[27]

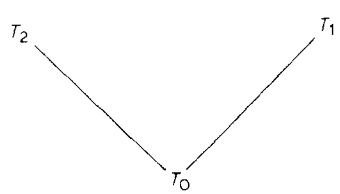
I must append two qualifying remarks to this discussion of theory dislodgment. First, I have for simplicity's sake considered only the case of '*radical*' theory change; i.e., the case where the core K is replaced by another. The construction of theory nets makes it possible to give an analogous account of a 'mini-revolution' such as Kuhn calls attention to in the postscript to the 2nd edition of his book. We have this kind of 'small revolution' when the 'pyramid point' K remains unaltered but a specialization K^s *at a relatively high place in the pyramid* is replaced by another. On the other hand it is also possible that during the course of the development of a theory not only the 'pyramid point' K but also certain specializations K^s remain unaltered. The foregoing reconstruction of the normal science concept would have to be correspondingly modified. Suppose, for example, history tells us that all traditional Newtonian physicists had used the third law without hesitation. The concept of holding a Newtonian theory could then be reconstructed so that all core specializations containing this law remain unaltered although it was not incorporated into the core itself for systematic reasons.

In this way it also becomes possible to exhibit the gradual increase in the sensitivity of specialized cores to empirical test the further down the pyramid one goes. This remark is only meant to point out that the portrayal of the *fine structure of a theory* by means of nets (of cores and of theory elements) permits subtler distinctions than my foregoing unrefined sketch might suggest.

The second point concerns an open problem. It might turn out, at least in certain cases, that an adequate interpretation of radical theory change calls for something like the reconstruction of an '*intermediate theory*' combining features of both the old and the new theories. This intermediate theory is subsequently rejected by the new theory. For example, the proponents of the so-called ignorance interpretation of quantum physical measurement appear to concede a special status to the measuring instruments by continuing to consider them as *classical* systems. Those rejecting such an interpretation do so on grounds that quantum physical descriptions hold for *all* systems and, therefore, the measurement process must be considered an *interaction between quantum physical systems*. Supposing that quantum mechanics can be formulated independent of classical physics, the new theory first assumes an exclusive position in this second view.[28]

9. IS PROGRESS BRANCHING POSSIBLE IN REVOLUTIONARY THEORY DISLODGMENTS? THE 'EVOLUTIONARY TREE'

Is there such a thing as revolutionary progress branching? Concerning this question I would like to present a provoking thesis. According to teleological conceptions of scientific progress there surely can be no such thing. But the concepts of reduction and approximative imbedding were intended primarily to serve in formulating an *immanent* progress criterion which dispenses with all such 'metaphysical conceptions'. Assume now for the sake of argument that such progress concepts are available. It is then quite conceivable that we run across situations of the following kind:



This diagram is to be interpreted as follows: a theory T_0 can be dislodged by either T_1 or T_2, whereby T_0 is either *reducible to or approximatively imbeddable in both.* T_1 and T_2 are nevertheless neither equivalent, nor is either one reducible to the other because although both T_1 and T_2 explain the phenomena explained by T_0, the totality of phenomena which the one accounts for only *partially overlaps* those explained by the other. In perfect analogy to the case of normal scientific progress branching, we have here a juncture at which *ultimate*, not provisional, *value judgments* must decide which route to take, or whether both (or possibly several) such paths should be pursued.

Add this possibility to the two forms of normal scientific progress branching already described and we see that from a logical point of view nothing can be urged against the picture of a branching '*evolutionary tree*' which shocked so many. Nevertheless, many philosophers will object that the mere belief in the *conceivability* of such a situation implies a scientific relativism. To this I would reply: if by definition any interpretation of scientific progress not logically producing linearity and uniqueness is to be called 'relativism', indeed, even when an adequate progress concept is available, *then this is presumably a form of relativism which we must swallow.* My initial position must then be re-formulated. It is true that I tried to de-irrationalize and de-subjectivize the picture of Kuhn's conception of theory change shared by philosophers of quite different persuasions. On the other hand, though, I have not only not furnished the means for overcoming a certain variant of what some call *relativism*, but I have attempted to show that this form of relativism is *defensible*. If, however, it is asked whether, and how often, such branching has actually occurred, the logician, *qua logician*, is unable to say anything and must pass the question along to his colleagues in other fields, especially to the historians of science, since its answer does not lie within his competence. But it must not be overlooked that no matter what the answer, there would still remain a problem to be solved. For even if progress branching has occurred, it was presumably quite rare. But why? I know of no general answer. We are faced with a somewhat paradoxical situation: the prima facie shocking idea of progress producing a branching evolutionary tree proves under closer analysis to be epistemically harmless. The *real* problem here is to explain *why such branching is much more rare than one would expect.* I have only the vague idea that an adequate answer will involve peculiarities of human nature as well as internal and external factors.[29]

Let me make a concluding remark. I cannot hope that the material presented by Sneed and myself will suffice to convince you. On the one hand you may have the impression of having been bombarded with too many novelties which are difficult to digest. On the other, we have for the time being only a meagre basis, namely, the theories of mathematical physics. And there, too, we had to begin with the most simple. Whatever a logician may have to say about a topic like 'theory change', it is sure to be dry as bones compared with the fascinating vividness and colorful richness of Kuhn's writings.

Concerning my own talk in particular, it must be said that although it is based on analyses and arguments, it represents primarily an attempt to persuade you of something. It contains more propaganda for, than conclusive proofs of the superiority of a new 'theory paradigm'. The success of a new paradigm is, as all of you know, 'at first largely a promise of success discoverable in selected and still incomplete examples'.

Since the logic of science is still in its infancy compared with metamathematics, very much remains to be done before the bridge between it and the history and psychology of science can be completed. Future success depends, however, not only on our efforts and skills, but on something else too. In those fields of knowledge dealing with human affairs one can observe in recent times an unfortunate trend. The representatives of various schools of thought do not even listen to one another anymore.[30] This trend has begun to catch hold in the philosophy of science too, and seems to gain momentum the more opinions diverge. This need not be, since it has not always been so. Ancient philosophers differed in their opinions no less than philosophers today. But no matter how vigorously they attacked each other, they never refused to talk it out. The future situation in the philosophy of science will depend to a great extent on whether we succeed in regaining this virtue of the ancient Greeks, namely, of listening to each other.

NOTES

[1] Cf., e.g., Bas C. van Fraassen [5], especially p. 310f.

[1a] Carlos-Ulises Moulines [18] and [19].

[2] The concept 'pressure' offers an example if we take mechanics as T_1 and phenomenological thermodynamics as T_2.

[3] 'K^S' stands for some arbitrary specialization of K, and is, thus, an abbreviation for '$\langle K, \mathscr{K}, \sqsubseteq \rangle$'.
[3a] J. D. Sneed [25]; W. Stegmüller [28] and [29].
[4] For formal definitions cf. Sneed [25], p. 266, and Stegmüller [28], p. 194.
[5] For technical details cf. Sneed [25], p. 294, and Stegmüller [28], p. 221ff. The deviation of the latter from Sneed's definition is due to the attempt to eliminate the 'platonistic' character of the set I. This was done by way of an auxiliary definition (D15, p. 221) in which the applications of a theory T accepted by a person at a given time were defined.
[6] For a more exact formulation of this *subjective variant* cf. Stegmüller [29].
[7] Karl Popper [20].
[8] Of course we do not mean to deny that normal scientists can, and often do, violate such rationality criteria. But in this they do not fundamentally differ from other professions. There is scarcely a historian today who would seriously maintain that Alexander Borgia met the minimal requirement for becoming Pope, namely, being a good Catholic.
[9] '$\sqsubset$' symbolizes the partial order defined by Sneed for expansions.
[10] The expression 'research programme' is perhaps misleading insofar as it suggests the idea of a *specific* research goal when merely the overall goal of an optimal application of a theory is meant. This fact is sufficiently reflected in the stipulations of our explication. For breaking the overall goal down into component parts the Kuhnian expression 'puzzle-solving' offers itself.
[11] Whether or not he was *successful* will, of course, not be discussed here.
[12] The concept 'excess-corroboration' plays an important part for Lakatos in determining the superiority of one 'research programme' to another. Obviously one can speak of such an additional or excess corroboration only, when the concept of corroboration itself is assumed *to be antecedently available*, i.e., adequately introduced in a deductive confirmation theory.
[13] When I said that the concept of holding a theory could 'by and large' also be used to reconstruct the concept of research programme, it would be these aspects that must be considered. In particular, an adequate clarification of this concept would entail the following modifications: (1) For the reasons just mentioned the normative methodological standpoint could be disregarded. (2) Certain inconsistencies in Lakatos' presentation, resulting from an ambiguity in his use of the word 'theory', must be weeded out. According to Lakatos, theories are, on the one hand, per definition *elements* of research programmes; on the other hand, he speaks of Newton's *theory* or relativity *theory*. What being, other than perhaps Hegel's world-spirit, is supposed to have a research programme including such theories *as elements*? In the first case theories are to be understood as empirical claims; in the second, as theories in our sense or research programmes. Consequently Lakatos ought to speak of the Newtonian research programme, the relativity research programme, etc.
[13a] But we should be mindful of the fact that no scientist not following these recommendations or other advices of a similar kind makes a mistake. 'Physical intuition' may tell him to violate the recommendations and his success will prove that he was right in doing so.
[14] Cf. Stegmüller [28], holistic thesis (III), p. 272 and its discussion, p. 276.
[15] I. Scheffler [23], p. 186ff.
[16] This view was advocated, e.g., by J. Watkins in [30], as well as originally by I. Lakatos in [15].
[17] Cf. Stegmüller, *Personelle und statistische Wahrscheinlichkeit*, Berlin-Heidelberg-New York, 1973; Part II, Section III.
[18] This relative concession to hermeneutics should not be overestimated. For the source of the 'depth' and, thus, the 'superiority' of the understanding here lies in the (non-trivial)

logical, not the (trivial) *hermeneutic* component. For an analogous situation in which logical understanding has to take place of an explanation cf. Stegmüller, *Personelle und statistische Wahrscheinlichkeit*, op. cit., Section IV.

[19] Cf. Sneed [25], p. 90ff., and Stegmüller [28], p. 271ff. Three further kinds of 'holism' may be distinguished from the two mentioned here ('holism of empirical claims' and 'holism of empirical theories'). Sometimes the thesis of the '*theory-ladenness of observations*', according to which 'a theory defines its own facts', is included as a part of this thesis (cf. Stegmüller [28], holistic thesis (III) p. 272ff.). Besides these, there is also what could be called the '*holism of refutation and confirmation*'. According to it only an entire scientific system can be confronted with 'experience' and supported or falsified by it. A detailed examination of this form of holism is a task for confirmation and test theory. But in any case, one can extract a certain concession to this conception from the 'holism of empirical claims' described in Section I. Finally, there also appears to be something like a '*methodological holism*'. According to it one must answer all questions in the philosophy of science simultaneously. Theories, for example, cannot be investigated in isolation, but only in the context of the entire theory hierarchy. Or the problem of scientific explanation must be dealt with together with the problems of concept and theory construction, and these in turn with those of confirmation and testing. This fifth form of holism is unacceptable for the simple reason that it makes superhuman demands of philosophers.

[20] Thus we dispose of the difficulty sometimes called 'the problem of Kuhn-loss' first by conceding, not denying, that *a decision on the basis of value judgments* must be made here, and second, by holding these value judgments to be *provisional relative to the intention of the researcher.*

[21] To these belong, among others, those cases where the proponents of the new theory had no opportunity to demonstrate the rationality of their efforts because their theory itself was dislodged *by a third theory* before it was successful.

[22] This holds especially for [28], p. 248ff.

[23] Were there one, the phenomenon of theory dislodgment as such would have to be regarded either as an irrational process or as an incomplete description of a rational process. The present location of the 'missing link' is such that it need neither contest the phenomenon of theory dislodgment nor regard it as an incomplete description.

[24] Stegmüller [28], p. 254ff.

[25] G. Ludwig [17], p. 71ff.

[26] This theory, which uses quite strong topological concepts, cannot be sketched here. The starting point is the concept of *uniform filters* as introduced by G. Ludwig [17], p. 76f.

[27] Labeling this notion 'falsification' was very misleading. For it was not conceived to be a relation between theories and empirical data, but a relation holding between a theory T_2 and another theory T_1 iff T_2 supercedes T_1. For a detailed critique of this theory of Lakatos, cf. Stegmüller [28], p. 264 and 265.

[28] Cf. Bas. C. van Fraassen [5].

[29] Internal factors would, for example, be presystematic intuitive considerations of analogy or simplicity. The external factors would include, among other things, available technology and the dominating Weltanschauung. Concerning the latter, cf. also K. Hübner, 'Zur Frage des Relativismus und des Fortschritts in den Wissenschaften. Imre Lakatos zum Gedächtnis', *Journal for the Philosophy of Science*, Vol. V/2 (1974), p. 285–303.

[30] An impressive account of this deplorable situation within social philosophy has been given by Kurt v. Fritz in his booklet: *The Relevance of Ancient Social and Political Philosophy for our Times. A short Introduction to the Problem*, Berlin–New York, 1974.

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