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The Structuralist View of Theories

*A Possible Analogue
of the Bourbaki Programme
in Physical Science*

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Preface

The present text originated with the intention of writing a brief reply to Feyerabend's detailed discussion of my book *The Structure and Dynamics of Theories*. For reasons explained in the Introduction this turned out to be an impossible undertaking. What resulted was a self-contained new approach to the structuralist view, combined with an attempt to bring it up to date by including a report on the latest developments.

As matters stand it would have been unreasonable and unfair of me to ask the editors of *The British Journal for the Philosophy of Science* to publish this text which exceeds by far the size of an average essay. Thus, a separate publication seemed advisable. I am deeply indebted to Springer-Verlag for making this publication possible.

Since the publication of the above-mentioned book I have learned a lot from the works, partly unpublished, of Professor Joseph D. Sneed, Professor Carlos-Ulises Moulines, and Dr. Wolfgang Balzer. I should like to thank my co-workers Dr. Wolfgang Balzer and Dr. Matthias Varga von Kibed and my student Michael Heidelberger for many constructive, critical remarks on the first draft of the manuscript and, in addition, Dr. Balzer for collecting and organizing the material for the Formal Appendix. Last, but not least, I express my warm thanks to Mrs. Clara Seneca, Oldenburg, and Mr. Roberto Minio, Springer-Verlag, for amending my English formulations.

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Introduction

In *The British Journal for the Philosophy of Science*, Vol. 28, No. 4 (Dec. 1977), pp. 351–369, there is a discussion by Professor P. Feyerabend of the original German edition of my book, “The Structure and Dynamics of Theories,” [52]. It was my original intention to write a brief reply to this review, in which reply I wanted to answer various questions raised by Feyerabend and to meet some of his objections. This turned out to be impracticable. Since the publication of [52] there have been so many developments and, what is more important, I discovered so many misrepresentations in my book that it would have been a hopeless undertaking to deal with all these aspects in a brief comment on Feyerabend’s discussion.

I therefore decided to give the present systematic account which, occasional references to Feyerabend’s [13] notwithstanding, is self-contained in principle and gives a synopsis of the latest views while using as few technicalities as possible. This being the case, I feel obliged to say a few words in this introduction about Feyerabend’s review. On the whole it seems to me that it is an excellent discussion, not only because it represents concisely and clearly the main features of my thoughts in a fair, although very critical, manner. For me, its merit consists above all, in having brought to my attention many shortcomings, gaps, and problematic claims in my book. I cannot remember ever having been more stimulated by critical remarks than by Feyerabend’s presentation. It inspired me to write the following text. I can only hope that the quality of this text lives up to that of his review.

There are three main respects in which it seems to me that Feyerabend’s account ought to be corrected or modified.

(I) The structuralist approach should be looked at as *the striving for an extension of the Bourbaki programme to science* rather than as an attempt to reconstruct the ideas of T.S. Kuhn. The fact that, with this approach, some aspects of Kuhn’s philosophy of science can be substantiated, or at least made more plausible, should be considered as a side-effect only.

(II) The phrase “non-statement view” is ambiguous. By distinguishing between four different meanings of this term I shall strive to remove several confu-

sions. Furthermore, I shall try to show that with respect to the two most important meanings *the opposite*, i.e., *the 'statement view'*, *provides no viable alternative at all* (because it is either wrong or it exists only as the product of wishful thinking).

(III) Of no less importance is the correct evaluation of Sneed's conception of theoreticity and of the amended Ramsey view.

I shall try to clarify these points in §§ 1, 2, 3, and 7. In §§ 4, 5, 6, 9, and 10 I report on new developments. In § 8 I correct some of my objections against Kuhn which were mentioned by Feyerabend. In § 11 I endeavour to say a few new things on the topic 'incommensurability' (which I never dealt with systematically in previous publications).

Having spoken of the need to correct Feyerabend's account, I now hurry to emphasize that I myself feel completely responsible for the confusions I seem to have created, partly by excessive emphasis on the Kuhn-reconstruction (due to my own 'conversion'), partly by overlooking the ambiguity of the 'non-statement view' and partly by an insufficiently clear presentation of the import of Sneed's criterion of theoreticity.

From the point of view of the structuralist approach, the first four sections are the most important ones. Having digested them, the reader may turn to any of the later sections whose topic interests him.

§ 1. The Origin of the Structuralist Approach: The Attempted Integration of Physical Science into the Bourbaki Programme by P. Suppes.¹ Non-Statement View₁

Most critics of my book [52] seem to assume that it was my main objective, to give a corrected account of T.S. Kuhn's philosophy of science, a new 'Kuhn-reconstruction' so to speak. This assumption is not correct, neither in an historical nor in a systematic respect². A few words of clarification are necessary, not only from the viewpoint of structuralism as a purely systematic endeavour. It seems to me that it should be welcomed from the point of view of Kuhn's philosophy as well. For it is my conviction, and I hope that Professor Kuhn will share this conviction, that it is detrimental to the discussion of the philo-

1 Readers interested in more details than those mentioned in this and the next section are advised to read the article [33] of Moulines and Sneed.

2 It may contribute to an elucidation to add a few words of a partly autobiographical kind. As far as my [52] is concerned, it must be looked at in the context of the whole vol. II of which it is only the 2nd half-volume. In the last chapter of the first half-volume I tried to give an extensive account of the Ramsey-sentence of a theory and its usual interpretations. Immediately after publication I learnt by chance that 'a young man' in Stanford, named Sneed, had written a dissertation which contained modifications and improvements of the Ramsey method. *Only because of my interest in the Ramsey method* did I begin to study Sneed's manuscript which I received a short time before it was published as [47]. For quite a long time I looked at Sneed's work exclusively under the aspect of how he continued the work of Ramsey and gave it a new interpretation. Such a thing as a new interpretation of Kuhn did not come into my mind at all.

Sneed's own objective in [47] was *neither* to give a Kuhn-interpretation *nor*, as I erroneously believed originally, to improve the Ramsey method. His main concern was the problem of how a physical theory, the mathematical structure of which has been described axiomatically according to the procedure of Suppes, could be transformed into a '*real empirical theory*'. The amendment of the Ramsey method came in only as a means to solve *one particular problem* namely 'the problem of theoretical terms' that arose in the course of this work; vid. § 2 and the article mentioned in the previous footnote.

sophical views shared by Kuhn, if the impression is created that a new formalism, invented as it were for the sake of belated justification, is forced upon Kuhn's philosophy.

I now mention a distinction whose significance I noticed only lately, namely the distinction between *general philosophy of science* and *special philosophy of science*. A detailed discussion will be postponed until § 7 where I will try to explain its importance. Suffice it to say for the moment that special philosophy of science, in the sense in which I shall use this term, always has to do with *particular* scientific theories.

This distinction reflects the emergence of two different trends in the philosophy of science, both of which endeavour to obtain systematic rational reconstructions. For the sake of vividness I call the procedure advocated by the first school the *Carnap approach* and the method recommended by the second school the *Suppes approach*. In both cases the first step of rational reconstruction with respect to a particular physical theory consists in an axiomatization which is intended to lay bare the mathematical structure of the theory in question; but there is a fundamental difference in the way this task is performed.

According to the Carnap approach the theory is to be axiomatized within a formal language. It was Carnap's firm conviction that only formal languages can provide the suitable tools to achieve the desired precision. The preoccupation with real empirical theories is therefore postponed until this first problem has been solved: how to formulate the given theory in a precisely described artificial language. For this reason we could also call this the *formal language approach*.

The Suppes approach is altogether different. Like Bourbaki, and unlike Carnap, Suppes uses only *informal logic* and *informal set theory* for the purpose of axiomatization. Following this course, he and his co-workers were able to show that real physical theories can be axiomatized in a precise way *without recourse to formal languages*. In what follows, I shall presuppose that he and his co-workers succeeded to a large extent in their endeavour. Besides, I assume as known that their axiomatizations are much superior to earlier attempts as far as clarity and precision is concerned³.

It is now more than twenty years since Suppes advanced the claim that philosophers of science should use *set-theoretical* instead of *metamathematical* methods. With this claim he began to lay the foundations of what I shall call the *structuralist view*. The phrase "statement view", in contrast, does not denote a sharply defined thesis, but stands rather for a family of concepts. As this fact has created some confusion, I shall try to differentiate between some of its uses. It will turn out that the phrase has three or four important different meanings.

3 Vid. the discussion in Moulines and Sneed, [33], Sect. 1.

By *statement view*₁ (*st.v.*₁) I refer only to the Carnap or formal language approach and by *non-st.v.*₁ I refer to the structuralist approach. I now formulate a main thesis:

Thesis 1: *To carry out the programme suggested by statement view*₁ *is not humanly possible. Thus st.v.*₁ *is not a realistic alternative to the structuralist view. Whoever compares the advantages and disadvantages of non-st.v.*₁ *and st.v.*₁ *weighs something that does exist against something that does not, and will not, for a long time to come, exist at all.*

One need not be a convinced follower of Quine's scepticism with regard to possible worlds in order to differentiate between logical possibilities and *realistic* possibilities. The reconstruction of physical theories within the framework of *st.v.*₁ will, for a very, very long time, be only a logical possibility but not a realistic one. Therefore, adherents of *st.v.*₁ are forced to use simple, fictitious examples instead of instances from real science.

Let us illustrate this with an example from mathematics. While the Bourbaki method is here regarded as the counterpart to the Suppes approach, metamathematics would correspond to the Carnap approach. This is not to deny the extraordinary progress made by metamathematics in the last few decades. But suppose that Bourbaki had made up his mind⁴ to use, as a universal language of mathematics, *formalized set theory* instead of *informal set theory* for his intended reconstruction of modern mathematics in precise terms. The Bourbaki volumes would then look like, for example, the last chapter of Shoenfield's *Mathematical Logic*. I really fear that the Bourbaki volumes would not exist, at least not yet, since instead of having published more than 20 volumes, Bourbaki would still be working on his first volume.

By rejecting the Carnap approach I seem to be joining those who have always contended that Carnap's main mistake consisted in overestimating the power of modern logic. But this is, of course, nonsense. It was one of the great and incontestable philosophical merits of Carnap that he recognized very early the importance of modern logic as an irreplaceable tool for many analytic purposes. If he misjudged something, it was not modern logic but our *human abilities* to handle this powerful tool.

As far as I know, there exist extraordinarily few articles which deal with real physical theories within a formalized language. One of them is a paper by Richard Montague⁵. As is generally acknowledged, Montague was an extraordinary logician, whose intellectual abilities and technical skill very few present

4 Here and elsewhere I take Bourbaki to be *one* person, relying on the veracity of his statement in the first footnote of Vol. I, where he informs us that he is "a member of the academy of science of Nancago".

5 Vid. [28a]. Another author who ought to be mentioned here is Aldo Bressan; vid., e.g., his applications of modal (type) logic to physical systems in [6].

philosophers of science, if any at all, can cope with. Comparing this article of Montague's with an analogous one of the Suppes approach, you will quickly recognize an essential difference. You will recognize it at least under the presupposition that with the latter the investigation to be carried out is done by a person who is an expert in the field in question and who is, in addition, sufficiently familiar with informal set theory, but who is not a prodigy of Montague's kind. *It is the difference between a few years of work and a few weeks (or perhaps afternoons) of work.*

Thus, the difference is a purely *practical* or *psychological* one. If an advocate of *st.v.*₁ starts with an assumption like this: "Suppose, *L* is a first-order language in which the physical theory *T* is axiomatized ...", and closer inspection reveals that *T* is a complex theory whose mathematical part makes use of tensor analysis, partial differential equations or even just the theory of matrices, then we may inquire again: where, on earth do we find the formalized theories containing these branches of mathematics? Nobody knows. They simply do not exist, just as most of the material in the Bourbaki volumes does not exist *rephrased a la Shoenfield*.

The advantages of the Suppes approach are to be looked at first and foremost from this practical-psychological point of view. "One of the major merits of Suppes' work is to have demonstrated by example that informal set-theoretic axiomatization is – at least for some kinds of questions about physical theories – a fruitful alternative to axiomatization in a formal language. The obsession with constructing formal axiomatic systems appears to account for much of the remoteness of current philosophy of science. Suppes' work liberates us from this obsession by demonstrating that departure from the method of formal axiomatization does not entail a lapse into obscurantism."⁶

Some readers who may still be sceptical will ask themselves what empirical assumption they must make in order to defend the statement view in the first sense, thereby shattering Thesis 1. In order to formulate the answer let us first introduce two predicates. The predicate "Super-Montague" is to apply to all philosophers who, without considerable efforts, can formulate the whole of modern mathematics, as represented by Bourbaki, in formalized languages. By a "Super-Super-Montague" we understand a philosopher who, in addition, is able to 'translate' all modern physical theories, general theory of relativity and quantum physics included, into formal languages and make them the object of his or her studies. (Like a metamathematician, he or she investigates theoretical physics written down in Shoenfield style).

Now we can formulate the empirical assumption:

(1) Among the philosophers of science there are not only many with an intellectual standard comparable with Montague's. We even find among them a suf-

6 Moulines-Sneed, [33], p. 66.

ficient number of Super-Montagues and Super-Super-Montagues who are able to represent the whole of modern mathematical physics within the Carnap approach.

I have not yet found supporting evidence in favour of claim (1). If somebody should object that, in his opinion, for a Montague to become a Super-Montague is “a triviality in principle (although perhaps not in fact)” I would answer that the emphasis lies on my claim of the *nonexistence of a Super-Super-Montague* in present philosophy of science.

The possible scepticism of the reader might have a source quite different from that we just discussed, namely not to understand *why the preference given to the Suppes approach leads to the structuralist point of view*. This, admittedly, has not yet been shown. In the next section I shall try to show that the kind of supplementation added to the Suppes programme by Sneed of necessity leads to the structuralist conception of theories. Without this further extension we would not even be justified in speaking of an *analogue* to the Bourbaki programme. Rather, we could speak only of an *integration* into the Bourbaki programme. As a matter of fact, Suppes and his co-workers confined themselves basically to rendering precise the *purely mathematical aspect* of physical theories. The structures discovered in the course of these investigations are in principle not of a different kind than mathematical structures in the usual sense, e.g., structures in algebra or topology. Informal set theory can be used equally well to define the predicate “is a group” and to define the predicate “is a classical particle mechanics”.

We thereby touch on the fundamental question of what distinguishes a *theory of mathematical physics* from a mere *mathematical theory*. This will become the main topic of the next section.

§ 2. Empiricism Liberalized, Informal Semantics and the Extended Bourbaki Programme ('Sneedification')

Sometimes the appreciation of a new philosophical trend is facilitated by revealing the difficulties which obstruct proper understanding. So let us try to find out the reasons which impeded comprehension of Sneed's procedure.

As a starting point we choose what formed the center of § 1: Suppes' philosophy of science, but this time including his views on the role of *experience* as well. I shall first try to explain the extraordinarily liberal form of empiricism advocated by Suppes. It is, as we shall see, much *too liberal* even for many of those philosophers who would never consider themselves orthodox empiricists. In a second step I shall try to show that the, partly explicit but mostly implicit, criticism of Suppes' philosophy of science by Sneed¹ goes in exactly the opposite direction to the potential criticism by an empiricist. Briefly, considered from the viewpoint of Sneed, the empiricism of Suppes is still *too narrow*, much too narrow in a certain respect. By properly understanding this very new and unforeseen liberalization of empiricism we have, I think, localized the point from which we may get an adequate grasp of Sneed's work. According to Sneed, the very liberal empiricism and operationalism of Suppes has to be replaced by something different, i.e., by what I shall name *informal semantics* (or *informal model theory*) of physical theories.

In itself, Suppes' main concern with each particular physical theory has nothing at all to do with the question of empiricism. What he drives at is a clarification of the internal mathematical structure of a physical theory which meets the highest standards of mathematical precision. At the same time, this task is pursued by disregarding the question of how the theories are related to extratheoretical entities, i.e., to the 'outside world', to which these theories are intended to apply. These two items, taken together, justify the claim, expressed at the end of § 1, that the Suppes approach integrates theories of mathematical physics into the Bourbaki programme.

¹ Insofar as I refer to the common article [33] of Moulines and Sneed, I ought to say more exactly: "the criticism by Sneed and Moulines".

But to say nothing more than this would give an overly restricted picture of Suppes' philosophy of science. He *has* dealt with the aspect of application as well, but only in a special context, namely within the theory of measurement. One *could* call Suppes an adherent of operationalism. But this must not be done without qualifications, since the operationalism he advocates is much more liberal and essentially more sophisticated than the position of earlier operationalists and logical empiricists². I shall try to give a brief intuitive survey.

All physical theories of any interest involve quantitative concepts, i.e., functions. The values of some of these functions are obtained by calculations from functions appearing in *other, more elementary* physical theories. Ultimately, we must reach a point where certain quantitative concepts are linked directly to qualitative concepts. According to the empiricists, the link is described by 'coordinating-definitions', 'correspondence rules' or 'meaning postulates'. *For Suppes, this role is played by theories of fundamental measurement.* As these theories are usually ignored by physicists, he considers it a task for the philosopher of science to formulate the theories of measurement which are implicitly contained in the standard expositions of physical theories. Only here is the point reached where the theory 'touches' reality: experiments, operations, and observations refer to the extensions of *qualitative* relations underlying the quantitative structures. The essential difference between the traditional operationalism or empiricism on the one hand and the method of Suppes on the other hand is characterized by Moulines and Sneed in this way: while those earlier theories introduce quantitative concepts by *intra*-theoretical relationship, as, e.g., in the case of correspondence rules, Suppes explicates a quantitative concept by *inter*-theoretical relationship. As distinguished from all earlier theories, his is a two-level theory which always refers to two parts: a higher-level theory, containing the quantitative concept, and a lower-level theory, describing axiomatically certain operations and relations. The precise form of the link between the quantitative concept of the higher-level theory and its corresponding qualitative theory is stated in a *representation theorem*. In this theorem, the *existence* and *uniqueness* of the quantitative concept, satisfying some conditions formulated in the higher-level theory, is proved. The satisfiability of the axioms of the qualitative theory in a given domain thereby serves as a premise of the derivation.

If one would like to call this operationalism at all, then presumably for the following reason: According to this theory, all the information we can obtain from the quantitative concept can already be obtained from the assumptions on the non-metric concepts in the underlying qualitative theory. We need not enter into a discussion of the potential objections of an empiricist arguing that this form of empiricism is much too moderate and too liberal. What interests

2 For the epistemologically relevant details vid. Moulines and Sneed, [33], part III and IV.

us in the present context is the fact that, surprisingly enough, Sneed finds this form of ‘operationalism’, at least as a general account of quantitative concepts in science, not too liberal but *too conservative*. And this implies that, for him, a variant of empiricism based on these ideas must be rejected as *too narrow*. Why is this so?

The main reason cannot be explained in a very simple way; therefore, I postpone this task to § 3. Briefly, it follows from Sneed’s *conception of theoretical terms*. According to Sneed and unlike the common view, the difference between the theoretical and the non-theoretical does not coincide with the observational – non-observational dichotomy. In addition, the first dichotomy cannot be explicated by reference to sublanguages within the language of science, since theoreticity must always *be relativized, not to a language but to a physical theory*. The exact denotation is, therefore, not “theoretical” but “*T-theoretical*”.

The theory-relativized concept of theoreticity is the first attempt to give an adequate answer to what I elsewhere described as ‘Putnam’s challenge’³. In the present context we are interested in the relation of the new conception of theoreticity to Suppes’ views on fundamental measurement. It can briefly be described as follows: if Sneed is right, then the requirement of Suppes to prove a representation theorem for all quantitative concepts introduced by fundamental measurement has to be given up as far as *T-theoretical* concepts are concerned. And this again means that the empiricism, implicit in the requirement for such a theorem, must be abandoned and give way to a still more liberal attitude. This does not only have philosophical consequences, for it affects many of the modern investigations on measurement, among them the well-known book of Krantz et al. [19]: with respect to a given physical theory *T*, most of the results quoted there hold for *T-non-theoretical* concepts only. *T-theoretical* concepts stand in a much more indirect and loose connection to ‘empirical reality’.

One could perhaps call the difference between theoretical and non-theoretical concepts a *semantical* difference – not in the sense of a purely referential semantics à la Tarski: a closer analogy would be to Wittgenstein, as the dichotomy is based on a difference in *use*⁴. The analogy has, however, to be handled with great care; for, in an essential respect, Sneed’s notion of use is more involved than that of Wittgenstein. While Wittgenstein tries to reduce certain questions of the meaning of a term to *the use of this term*, what counts for Sneed in judging theoreticity is *the use of the general and special laws in which*

3 [52], chap. 1.2, p. 26ff.

4 Vid. the following quotation from Moulines-Sneed: “One need not be an orthodox Wittgensteinian to recognize that some questions about meaning may be illuminated by looking at the *use* of the concepts in question”, [33], p. 78.

the term occurs. Because of the complexity and difficulty of this subject, the new conception will be elucidated, as mentioned before, in the next section.

For a better understanding of how Sneed's considerations belong to semantics, we now return for a moment to the description of the Suppes approach given at the beginning of this section. There we observed that his aim in axiomatizing a physical theory is to clarify the *internal* structure of the theory, meeting modern standards of mathematical rigor. Unlike many other axiomatizations, those of the Suppes approach are distinguished by not being philosophically biased. However, if we disregard the achievement expected from a theory of fundamental measurement, the ascetic self-restriction to the mathematical aspect has one great philosophical shortcoming: *the relation of the physical theory to something 'outside' itself is left out of consideration.* On the other hand, *Sneed's main point has to do precisely with the relations of mathematically described structures of theories to 'outside entities' which are, for their part, not theories.*

Let us recall the fact that Suppes makes use of informal set theory. If he *would* carry out his axiomatizations within formal languages, his studies would have to be described as *syntactic*, this word being used in the sense of Carnap. And all investigations of the relations of the syntactically characterized structures to their models would count as *referential semantics*. Since in Suppes' investigations informal set-theoretic structures take the place of syntactic structures it seems natural to see Sneed's investigations of the relations between such structures and 'real world models' as belonging to *informal semantics* of set-theoretically axiomatized physical theories.

In order to understand this kind of 'physical semantics', we must not be guided too much by our picture of formal Tarski semantics. There are some fundamental differences. Moreover, it will turn out that the boundary between physical semantics and *pragmatics* is fluid. However, it seems to me that this is no disadvantage but rather a philosophical advantage because it opens the door to a systematic pragmatics as an urgently needed discipline (vid. § 4). In any case, one should keep in mind that, for Sneed, physical understanding involves much more than 'settling the reference of physical concepts'.

The first important difference to formal semantics is the *nonexistence* of an analogue to a '*universe of discourse*', the universe of discourse being like 'one big application' of the formal theory we are interested in. The best way to discover the application of a physical theory is not to look at the remarks interspersed in an exposition of the theory – remarks which often claim the theory has just an all-embracing 'cosmic' application. Look rather at the author's examples (often given in the form of exercises). Then it soon becomes obvious that a physical theory has *countless intended applications*. The class of these intended applications is called *I* in [47] and in [52]. This class is not

rigorously circumscribed but is largely an *open* set⁵. Moreover, in most cases I is ‘anchored’ in a paradigmatic subset I_0 which remains a part of the theory’s intended applications during its whole history⁶. Newton, for example, gave the following list of paradigmatic examples of classical particle mechanics: the solar system; various subsystems of the latter, like earth – moon or Jupiter and its moons; several comets; pendulum movements; free-falling bodies near the earth’s surface; the tides.

If the mathematical structure of a theory is represented within a Suppes-type axiomatization by a set-theoretic predicate S , we may, in a first approximation, identify a physical theory with an *ordered pair* $\langle M, I \rangle$, whereby M is the extension of S , i.e., the set of possible models of this structure, and I the open set of ‘really’ intended applications.

The *special laws* form a particular and difficult problem for informal semantics. The axiomatization of classical particle mechanics given in [28] leaves open the possibility of stating special laws. But it is not indicated how the various applications of classical particle mechanics, for which special laws are stipulated, are related to each other and how they ‘hang together’. *A fortiori*, it does not follow from the account given there how to reconstruct the *development* of classical particle mechanics over time as new special laws and new applications are discovered.

The original solution given to this problem in [47] and [52] was in terms of *expanded cores*. This was a clumsy procedure which, moreover, had several disadvantages. It has since then been replaced by a much simpler and intuitively more satisfactory method. I shall report on this improvement in § 4.

There is an additional semantic feature which Sneed tries to capture with the help of his new concept of *constraint*. Formally, a constraint is a *set* of models, i.e., a subset of M ; the class C of constraints is therefore a subset of $\mathbf{Po}(M)$, the power set of M . As a starting point for understanding this notion one sensibly uses the fact that the elements of I , i.e., the different applications, *partly overlap*. The constraints produce *cross-connections* between these overlapping applications. The kind of connection engendered is not easily described, because all of the important constraints work via the theoretical functions. For the moment, the following hint will suffice: suppose the physical theory *were* formulated in a formal language. Then the connection between different models, often called “possible worlds”, would have to be described by means of ‘modal’ concepts, or more generally, by means of non-extensional concepts,

5 In his book [47], Sneed treated I like a definite platonic entity. I have strived for relief from this ‘Sneedean Platonism’ in my book [52]. This was the source of some complications in certain formulations and definitions, in particular in connection with the concept of holding a theory, D 30, p. 194f.

6 For the sake of fairness it ought to be mentioned that Suppes never did assume the existence of ‘one big application’.

describing ‘cross-world relations’. Sneed’s concept of constraint is, in my view, an ingenious device to reproduce such modal relations within a non-intensional, purely extensional framework.

Objections have been raised against introducing constraints *as a separate concept*. Tuomela, e.g., points out in [61], p. 230, footnote 2, that Sneed’s $\langle =, = \rangle$ – constraint for the mass function within the context of classical particle mechanics can be formulated as a law. From a logical point of view, this is indeed correct, but, for doing so, there is a price to pay which would seem to be too high: in order to formulate this law, one has to introduce a ‘cosmic mass function’. In this way, the ‘one big application’ which we wanted to avoid by introducing the open set I of intended applications would come in again by the back door. The present problem is not a question of logical correctness but of physical adequacy⁷.

In this section I have tried to facilitate the proper understanding of Sneed’s systematic work by pointing out its roots in the Suppes approach and by showing how it can be looked on as an essentially *semantic supplementation* of the method of informal axiomatization. In contrast, most critics of my [52] seem to see Sneed’s achievements mainly in connection with the reconstruction of Kuhn. Feyerabend, in [13], § 4, even speaks of “Kuhn Sneedified.”

Now it certainly *is* a remarkable fact that the Sneed formalism enables us, via ‘pragmatization’ (vid. § 5), to render precise and to reconstruct several aspects of Kuhn’s philosophy of science. But doing this was not at all the primary aim of Sneed’s work. Besides, in order to get such additional results, one has to introduce certain correspondences, like that of ‘normal science’ and ‘holding a theory’ or ‘theory-evolution’. All of this goes far beyond the purely systematic analyses, the latter of which are the centre of the Sneed-type investigations. Therefore, in order to put these into correct perspective, *Feyerabend’s ‘Kuhn Sneedified’ ought to be replaced by ‘Sneedification of Suppes’*. Following the Suppes approach *without* ‘Sneedification’ would, as we have realized, entitle us to nothing more than the claim that the mathematical structures of physical theories are *integrated into* the Bourbaki programme. Once the addition of informal model-theoretic research as begun by Sneed justifies talk of an *analogue to* or an *extension of* the Bourbaki programme in which *real physical theories* and not only their mathematical skeletons are studied.

7 It is interesting to see how Moulines and Sneed bring in this concept in their critical discussion of Suppes, [33], p.74, using some elementary examples from classical particle mechanics: “Suppes does not recognize that there are additional important structural features of the theory lodged in relations among different models (intended applications) of the theory. In the case at hand, *unicity* of mass ratios and extensivity of mass with respect to particle concatenation as well as certain features of special force laws appear to be most naturally treated as relations among different models (intended applications) of classical particle mechanics. Roughly, the MSS (= [28]) axiomatization captures the structure of *single* models (applications) of classical particle mechanics. It reveals nothing about relations among those models (applications) that are equally significant features of this theory’s internal structure.”

Admittedly, it would facilitate understanding if we could describe, in one single sentence, the mediating role of structuralism between the theory of P. Suppes on the one hand and the theories of T.S. Kuhn and I. Lakatos on the other. Let us try to do this with the following brief statement: *the constructive criticism of the Suppes approach leads to an informal semantic theory which, in its turn, by adding certain pragmatic items, allows a constructive criticism and a partial vindication of the philosophies of Kuhn and Lakatos.* More on this in § 8 and § 9.

§3. The Force of *T*-Theoreticity and the Ramsey-View Emended.

Non-Statement View₂ and Non-Statement View_{2,5}

As I stated previously, there is a passage in Sneed's [48], p. 132, which most readers find incomprehensible. Having stated the thesis that theoretical concepts are used in science as described by the Ramsey method, Sneed continues: "This is an empirical claim about how science is actually practiced. It must, of course, be defended against alternatives ... But it need not be defended against objections that the Ramsey method is in some way epistemologically suspect. *That empirical scientists do not live up to someones favorite epistemological credo is for them to defend, not me.*" (my italics). In other words: whoever has objections against the Ramsey method, should address himself to the scientists but not to the philosopher of science. Here we have the usual reaction to this: "Surely Sneed does not *seriously* mean that! The Ramsey method is a philosopher's invention and, what is more, a very artificial one. If there exists an activity at all by means of which the philosophers meddle in things they are not really competent in, *and which therefore is in need of careful justification*, then it is the construction of Ramsey sentences."

In this section I shall try to show that Sneed's conception of theoreticity, together with the view that every known and not wholly trivial physical theory *T* does contain *T*-theoretical terms, of necessity leads to the thesis implicitly contained in the italicized part of the quotation from Sneed.

For this reason, I have to touch on *the problem of theoretical terms*. Feyerabend says in [13], p. 353, footnote 1: "This problem does not arise in the statement view". But this is a fundamental misunderstanding¹. It is true, Sneed formulates the problem of theoretical terms by taking what he calls the 'traditional view'. In his book [47], he presents this view as saying that all em-

1 Incidentally, if Feyerabend *were* right, then his remark would be the strongest conceivable objection against the non-statement view in the sense of *non-st.v.*₂ to be explained below. For, in this case, the problem of theoretical terms would be nothing more than a problem *created by the structuralist approach* itself which, of course, it is not.

pirical claims (hypotheses) of physical theories have the form:

(1)
$$“a \text{ is an } S”$$
,

where “is an S ” is a set-theoretic predicate. But this was, in fact, done just for the sake of convenience.

As it was possibly this way of presenting the ‘traditional view’ which is responsible for the confusion, I shall dwell on it for a moment. It has been pointed out in § 1 that, because of our restricted knowledge of formal languages and because of our limited abilities in handling such languages, most interesting physical theories can *not* be represented within such a language. However, they can always be formulated in the language of informal set theory, *provided that they have been axiomatized in a precise way*. It is exactly this presupposition which is made by Sneed in formulating (1). More exactly: the only presupposition made in the passage in question in his [48] is that *the mathematical structure* of the theory meets the Bourbaki standard (or, for that matter, the Suppes standard).

For the sake of illustration I take an example from mathematics. Suppose one has to explain why the class of all counterclockwise rotations of a regular hexagon by positive multiples n of 60° , $n = 1, 2, \dots$, forms a group. An *old-fashioned* mathematician, as we shall call him, will proceed in the following way: as a first step, he will tell a brief story, consisting of a description of the axioms of group theory; as a second step, he will turn to the example and verify the axioms step by step. By contrast, a *modern* mathematician, using the method of axiomatization by defining set theoretic predicates, proceeds as follows: first, he gives an explicit definition of the predicate “is a group”. Secondly, he introduces a name, say Φ , for the above-mentioned class of rotations. Finally he makes the claim

(2)
$$\Phi \text{ is a group,}$$

and, by using the meanings of “ Φ ” and of “group” only, he proves (2). *One cannot say that the second mathematician ‘has not used the statement view’*, since all he has done is to *paraphrase* the formulations of the first mathematician in a new way.

Sneed merely transfers this method to the physical case, thereby starting immediately with an analogue of the ‘modern mathematician’. Suppose we want to state the hypothesis that the laws of classical particle mechanics hold of the solar system plus certain subsystems of it, like earth-moon. The ‘old-fashioned’ physicist will first choose one of the usual textbook formulations of that physical theory and then formulate his claim that these principles hold of the solar system and the subsystems in question. On the other hand, the ‘modern’ physicist or the physicist familiar with the Suppes approach imme-

diately starts with the definition of the set-theoretical predicate “is a *CPM*”. Furthermore he will introduce a new individual constant denoting the solar system plus the subsystems in question, say “ p_s ”. Finally, he formulates his empirical hypotheses as follows:

(3) p_s is a *CPM* .

This is a sentence exactly of form (1) [or (2)].

Perhaps the misunderstanding was created by substituting “oldfashioned” for Sneed’s use of “traditional”. Sneed really intended to describe the *traditional* view, but *in modern paraphrase*, i.e., in form (3) or, in the general case, in form (1). He could, of course, have used the ‘oldfashioned formulation’. But this would have had the disadvantage that the elucidation of the problem of theoretical terms would have become very much more complicated and clumsy.

What is the problem of theoretical terms? Suppose that the theory axiomatized by means of the set-theoretic predicate S contains S -theoretic terms. (For the particular example (3) we may assume that “mass” and “force” are *CPM*-theoretic.) Then (1) [or, in the example, (3)] *is not an empirical statement*. Here we call a statement empirical only if it is empirically testable. The precise form of the test procedure is irrelevant. The argument in favour of the claim that (1) [or (3)] is not empirical can be stated briefly: in order to check (1) [or (3)], another sentence *of exactly the same form and with exactly the same predicate* is presupposed. (A more detailed explanation of this will follow shortly.)

“But,” the immediate objection runs, “physicists *do* make empirical claims!” *Exactly*. “But Sneed asserts that the claims of physicists which can always be paraphrased like (1) or (3) are *not* empirical claims!” *Exactly*. “But that is absurd!” *Exactly*.

The open question is: what is responsible for this absurdity? Most philosophers would probably say: “It must be Sneed’s account of theoreticity with its alleged consequences that (1) and (3) are not empirical statements.” But if Sneed is right, and I am convinced that he is right on this point, then such a reaction is totally wrong. The correct answer would be: “The ‘absurdity’ is implied by the assumption that the traditional view is correct, i.e., that empirical physical claims are of form (1) or (3).” It should be observed that, according to Sneed, the traditional view of physical claims is wrong, not only that the view of philosophers of science *about these claims* is wrong²!

What does it mean to say that a quantity (function) f of a physical theory T is T -theoretical? Roughly speaking, it amounts to a brief story contained in the following two statements. In order to perform an empirical test of an empirical claim containing the T -theoretical quantity f , we have to measure values

² For footnote see page 18.

of the function f . But all known measuring procedures (or, if you like, all known theories of measurement of f -values) presuppose the validity of this very theory T .

In [33], p. 29, the situation is described concisely with respect to the *force function* in classical particle mechanics: "It does not require a very subtle reading of the standard expositions of classical particle mechanics to note that all the methods described therein for measuring forces (determining the values of force functions) require us to assume at least that Newton's second law holds and commonly that some other special force law holds as well. Only a little further reflection is required to notice that the *extension* of the concept of force in this theory will be determined, in part at least, by which special force laws are claimed to hold in which intended applications of classical particle mechanics. That is, the values we actually assign to forces depend on how we *use* laws involving forces." Here we find expressed, in addition, that 'generalized Wittgensteinian conception' of meaning as use that was mentioned earlier.

We get the intermediate result that the class of the following three sentences is inconsistent:

- (i) the traditional view of physical claims is correct, i.e., these claims are of type (1) [type (3)];
- (ii) classical particle mechanics contains the *CPM*-theoretical term *force*;
- (iii) the claims of type (1) [type (3)] are empirical, i.e., they are empirically testable.

2 Recently, an amusing example of a grotesque misinterpretation of this argument has been produced by W. Habermehl on p. 18ff. in his introduction to: K. Eichner and W. Habermehl (eds.), *Probleme der Erklärung sozialen Verhaltens*. Habermehl, by totally misapprehending the logical structure of the argument, believes that in [52] I had intended to bring forward an argument against Popper's demand for falsifiability of empirical hypotheses, and he takes great trouble to show that I am wrong. But it is *the premise of his reasoning* which is wrong. For the argument only begins as follows: "If the traditional view of empirical claims of theories *were* right then these claims *would* not be testable (either in the Popperian or in another sense, but this does not matter here)." And then the argument *continues*: "As these empirical claims *are*, of course, testable, the traditional view must be wrong." How can Habermehl's misinterpretation have come about? Apparently in the following way: First, he cut off the whole second half of the argument; secondly he overlooked that the first half is formulated in subjunctive mood. By tampering with my presentation of the argument in this way he arrives at the strange metamorphosis: what, in reality, was an indirect argument *using the effective testability ('falsifiability') of empirical hypotheses as an additional premise* in favour of the Ramsey view, suddenly becomes an anti-Popperian argument in favour of nonfalsifiability.

I mentioned this point only because it may prevent readers from making similar mistakes. But, quite apart from this, I shall try below to make the argument more perspicuous than I did in [52].

If Sneed is right with respect to the role of the force function within classical particle mechanics then (ii) cannot be denied. To reject (iii) would amount to denying the empirical character of physics. Hence there only remains the conclusion that the traditional view of physical claims cannot be correct, *i.e.*, (i) must be wrong.

One of Sneed's main theses on the level of *general* philosophy of science (vid. § 7) is that every nontrivial physical theory *T* contains not only *T*-non-theoretical terms but, in addition, *T*-theoretical ones.

I shall now try to give an independent, additional explanation of the concept of theoreticity by taking as an example a well-known discussion on the role of conventions in space-time metrics. Reichenbach and, following him in this respect, Grünbaum advanced the claim that, in order to be able to introduce the concept of congruence, we need a 'coordinate definition' which is a convention (although it is not just a case of 'trivial semantic conventionalism', *i.e.*, it is not a case of giving the *noise* 'congruent' a specific meaning). In other words, *the choice of a metric itself is a matter of convention*'.

It is a pity that Quine, when attacking the analytic-synthetic dichotomy, never made explicit reference to Reichenbach. Had he done so, the whole discussion would presumably have become much more interesting. Not only did Reichenbach³ assert the conventional character of metrics, but *he also claimed that he could prove it*. His argument was very simple: Suppose, he said, we try to find out empirically whether two rods, located at different places, are congruent. It can be shown easily that we are lead into a circle. I shall call this the *surface circularity in the empiricist search for congruence*. For Reichenbach there existed only the following alternative: either empirical discovery or conventional stipulation. With the first ruled out by the circularity argument, he concluded that congruence is determined by fiat. Exactly how this is to be done is extremely difficult to say. This is one of the immediate results of the discussion between H. Putnam and A. Grünbaum.

As a first approximation, the conventional stipulation consists in the choice of a distance on a solid body. However, this holds only 'as a first approximation' because *appropriate corrections have to be made, eliminating the effects of differential (perturbational) forces*. Reichenbach believed that these corrections could be made without resorting to a metric, *i.e.*, with the help of a procedure, described in detail by Grünbaum and called "quasi-operational" by Putnam.

As Putnam has shown, this assumption is based on an error of Reichenbach's⁴.

3 For the sake of simplicity, I identify in the following discussion Reichenbach with Reichenbach₁ in the sense of H. Putnam; for details vid. [34] and [35].

4 The technical details are described in the appendix of [34] and, more simply, in the appendix of [35].

More precisely, we even encounter a superimposition of two mistakes, going in opposite directions. On the one hand, Reichenbach's procedure is *much too weak*. He believed that excluding 'universal forces' would determine the metric uniquely⁵. This is not true: even after setting all universal forces equal to zero, the metric still remains indefinitely underdetermined. On the other hand, Reichenbach's procedure is *much too strong*. When pressed with the question why forces are needed at all in order to account for, e.g., the change of the size of a body, it would have become clear immediately that Reichenbach presupposed Newton's second law *as an analytic truth* which, of course, is not true either. At the end of § 8 we shall try to explain the features of this law which are presumably responsible for the impression of analyticity.

Actually, it should be clear from the outset that Reichenbach's (and Grünbaum's) procedure cannot be successful. For we can speak about forces *only in the context of particular force laws*. And such laws belong to a theory employing the concept of distance as well⁶. Therefore, *the appeal to a correction method itself leads to a circularity*. For the following reason we may call it a *depth circularity* in search for a congruence. If we were confronted only with the first circularity we could follow Reichenbach's proposal, at least in case there were no other way out. But this second circularity is irrevocable. What are we to do now?

By disregarding the complications alluded to, we could describe the different attitudes of Reichenbach and Sneed in the following way: For Reichenbach, epistemological circularity, wherever it appears, forces us to replace empirical research by conventional stipulations. According to Sneed, such a circularity is *a symptom of there being a theoretical quantity involved*. And this symptom is even *conclusive* if the circularity, as in the case of our depth circularity, is irrevocable. Looking at a quantity *as theoretical with respect to a particular theory* is, thus, a third alternative, distinguishing it from empirical determination as well as from stipulation. It may be that Reichenbach had something of this kind in mind later when he tried to fix the reference of such quantities by means of the '*interposition of theories*'. At least, in Putnam's interpretation, this was Reichenbach's view; and it seems to be Putnam's view as well⁷.

I shall now try to describe the differences between Putnam and Sneed. This description will have a somewhat speculative air because Putnam's account is much more programmatic than Sneed's. If I misrepresent him, than I hope that Professor Putnam will forgive me, since my aim is only to contribute to a clarification of the theory-relative notion of theoreticity and the problems connected with it.

5 For a concise description of the meaning of "universal force" vid. Putnam [35], p. 173.

6 Within the general theory of relativity such force laws make use of the g_{ik} -tensor.

7 Vid., e.g., [35], p. 174.

There appear to be two main differences. First, Putnam seems to hold a view which could be called *methodological holism*, according to which a theory has to be chosen, embracing geometry and physics as a whole. Although Sneed, too, accepts even several variants of holism⁸ with respect to the present problem, he prefers a ‘*piecewise*’ *reconstruction* as described in “The Limits of Holism”, [48], p. 133f.⁹. This would mean, for example, that for classical physics we get a hierarchical structure, beginning with physical geometry and leading through mechanical theories to classical equilibrium thermodynamics. The distance function would then be *theoretical only with respect to physical geometry* and *non-theoretical with respect to the theories following in this hierarchy*. (In a similar way, *pressure* would be theoretical with respect to mechanical theories but non-theoretical with respect to thermodynamics.)

Closely connected with this is an important second difference. We can describe it with the help of a possible empiricist objection against the view advocated by Putnam. This objection arises in the context of answering the question, what counts as a reason for accepting the theory in question. In his ‘coherence-determines-reference’ account, Putnam requires internal and external coherence of the theory to be maximized¹⁰. As far as *internal coherence* is concerned, like ‘simplicity’ and ‘agreement with intuition’, everything is alright. However this is not so for *external coherence*, i.e., ‘agreement with experimental checks’. For, how should these checks be performed? Certainly, they require calculations of the functions occurring in the theory. But each way to determine them empirically leads to a circle, as we have seen. This shows that the requirement for maximizing external coherence cannot be satisfied.

One could say that this is the way in which Putnam’s coherence account of science is confronted with *the problem of theoretical terms*.

Now we can return to an earlier discussion. We have seen that, of the three sentences (i), (ii), and (iii), the first one must be wrong. In other words, the traditional view, according to which empirical claims of theories are of the forms (1) or (3) is to be rejected. Presently, there is only one known way out of the dilemma, namely, by accepting the *Ramsey solution* of the problem of theoretical terms¹¹. It consists in *replacing* (1) and (3) *by their corresponding Ramsey-sentences*, existentially quantifying over precisely the *T*-theoretical terms. This really is a solution of the problem. For in order to check a Ramsey-sentence of this kind empirically, calculations of values of theoretical functions are no longer needed; the sentence imposes conditions only upon the non-theoretical functions.

8 I have tried to differentiate between them in Ch. 17 of [52], p. 231ff.

9 Actually, Sneed, in loc. cit., gives a much more cautious formulation than I do here for the sake of perspicuity.

10 [35], p. 165.

11 For a detailed description of this method vid. [52], Sect. 4.2, p. 58–63.

The question raised in connection with the passage quoted from Sneed can now easily be answered: *We have no choice but to interpret the empirical claims made by physicists as Ramsey-sentences*. Sentences of this kind express what scientists ‘really mean’. Therefore, whoever finds this method ‘epistemologically suspect’, has to address him- or herself to the scientists and not to the philosophers of science.

The ‘formal’ representation of an empirical physical claim has the form of the sentence (VI) in [52], p. 90. It is a *modified* Ramsey-sentence because, in it, all the additional semantic aspects mentioned in § 2 come into play as well. For that reason I called it a *Ramsey-Sneed-sentence*, expressing the emended Ramsey view.

The theoretical – non-theoretical dichotomy is responsible for only one of the semantic complications, namely for splitting up the extension M of the set-theoretic predicate “is an S ” of (1) into three sets M_{pp} , M_p , and M . M_p is the set of all *possible models* including the apparatus of theoretical concepts; M_{pp} is the set of all *partial potential models* obtained by ‘throwing out’ the theoretical components; M is the set of all possible models satisfying the fundamental laws. The minimal requirement for the set of constraints C mentioned in § 2 can now easily be described as $C \subseteq \mathbf{Po}(M)$.

Let us understand by *statement view*₂ (*st.v.*₂) the conception according to which the empirical hypotheses of physical theories are (potentially infinite) classes of sentences. By contrast, the emended Ramsey view says that at any time, the empirical content of a physical theory is *one single big claim*, indivisible into smaller parts. This gives us one clear sense of the expression “holistic view”. If Sneed’s conception of theoreticity is correct, then the statement view₂ is wrong. We call the thesis according to which the empirical claims made by physicists must be reconstructed as Ramsey-Sneed-sentences the *non-statement view*₂ (*non-st.v.*₂).

Suppose the Ramsey solution of the problem of theoretical terms is chosen within Putnam’s coherence account of science as well. This would not make the two approaches indistinguishable, although the *non-st.v.*₂ would be accepted in both cases. Putnam’s holism seems to be a global one while Sneed’s is not. As long as we can reconstruct physical theories stepwise ‘in hierarchical order’, then it suffices to apply the Ramsey method stepwise too. For a function which is theoretical with respect to a ‘lower-level’ T_1 is non-theoretical with respect to the ‘higher-level’ T_2 for which T_1 is the ‘underlying theory’. But, as Sneed points out in [48], p. 133–134, the development of theories may well go in the direction of what I just called global holism.

At several places the proviso has been inserted: “if Sneed is right”. This was done in order to remind the reader that Sneed’s conception of theoreticity is not the result of a mere logical or epistemological analysis, but rather that the core of his conception contains an *empirical statement* about what empirical

claims of physicists amount to. *He might be wrong*. All I can say is that, up to now, nobody has been able to show that he is wrong.

Time and again the '*relativistic character*' of Sneed's criterion has been brought forward as an objection against the notion of *T*-theoreticity. Since the criterion makes explicit reference to existing expositions of the theory, it may happen, it is argued, that a new exposition is found 'overnight' transforming a function which was thought to be *T*-theoretical into a *T*-non-theoretical one¹². But the word "existing" in "existing exposition" is to be understood in a timeless sense and not in the sense "existing up to this moment". The change from *T*-theoreticity into *T*-non-theoreticity by means of a newly devised method would amount to something like a *scientific revolution*.

Compare the following two sentences:

(a) Theophrast, the disciple of Aristotle, could have invented the theory of general relativity;

(b) tonight a physicist may succeed in contriving a procedure to measure the Newtonian force function in a theory-independent manner.

Both (a) and (b) express *logical possibilities*. Still, everybody will agree that, from a realistic point of view, (a) is just nonsense. I claim that there is a *difference only in degree* between (a) and (b). For, (b) too, is unrealistic. Perhaps you have to become a 'Kuhnian', at least to a certain extent, in order to understand that a scientific revolution, like that alluded to in (b), cannot just happen overnight, brought about by one single person but that such a change would have to depend on the cooperation, perhaps persisting over generations, of numerous scientists.

Irrespective of whether one is willing to accept this as an answer, there is the question of whether it would be possible to formulate necessary and sufficient conditions of *T*-theoreticity without making explicit reference to the existing expositions of *T*. One could try to do this, using the considerations in § 2 about the role of representation theorems. A tentative answer would be: *a physical theory T employs exactly those functions as T-theoretic quantities for which no representation theorem can be proved*. This 'criterion' does not, of course, give us a decision procedure. Besides, it makes reference not only to the theory in question but to the underlying qualitative theory, as described in § 2, as well.

The expression 'non-statement view' has, up to now, been endowed with two different meanings. While *non-st.v.₁* names the acceptance of the Suppes approach (or the Suppes-Sneed approach) and the rejection of the formal language approach, *non-st.v.₂* is a designation of the holistic way of representing empirical claims, i.e., of the emended Ramsey view, according to which the

12 This kind of objection is made for example by R. Tuomela in [61], p. 3. I am not sure whether Feyerabend in [13], p. 352, footnote 3, had a similar objection in mind.

empirical claims of theories are not to be formulated by infinite sets of sentences, but rather each by a Ramsey-Sneed-sentence. The first rests on the truth of Thesis 1 in § 1; the second is based on the correctness of Sneed's conception of theoreticity. There is a third sense of this phrase. I shall only explain it intuitively. It refers to *the way* particular laws *enter the 'big empirical claim'* in the sense of *non-st.v.₂*. Although the laws thereby 'lose their independence' it may very well be the case that a Ramsey-Sneed-sentence founders on experience because a particular law is falsified in the subdomain of *I* in which it was intended to hold. The latter will happen, e.g., in all those cases where the 'naïve view of special laws' applies.

There is a more interesting type of case, consisting of laws of relatively high generality, for which originally neither particular specializations nor particular constraints have been contrived. In such a case, if the Ramsey-Sneed-sentence of a theory turns out to be false at any particular time, one can not just pick out one of these laws and make *it* responsible for the failure. At the end of § 8 we shall analyse this in some detail with respect to Newton's second law. There we shall see that such a law which, if looked at pre-analytically, 'seems' to be falsifiable, is, by entering into the big empirical claim, sufficiently deprived of content so as to become 'immune against falsification'. It is this fact which gives additional import to the statement that empirical claims of physical theories are to be interpreted *holistically* as single comprehensive claims.

Since this aspect of the 'non-statement view' is closely connected with that one numbered 2, we give it the name "*non-statement view_{2,5}*" (by imitating a well-known convention in general topology) while reserving the designation "*non-statement view₃*" for something different in § 7.

§4. Theory-Nets Instead of Expanded Cores

It is only those basic laws of a theory which hold in every intended application and, similarly, only the general constraints which are included in the *core* of a theory. Formally, a core K may be represented either as a quadruple $K = \langle M_p, M_{pp}, M, C \rangle$ or as a quintuple $K = \langle M_p, M_{pp}, r, M, C \rangle$. Here, M_p , M_{pp} , and M are the sets mentioned at the end of § 3; C is the set of constraints, i.e., a subset of the power set of M ; and the *restriction function* $r: M_p \rightarrow M_{pp}$ transforms an element of M_p , i.e., a potential model, into an element of M_{pp} , i.e., into a partial potential model, by ‘lopping off’ all theoretical functions. It depends on the particular aim and on the kind of analysis whether r is to be included into K or is, like the ‘application function’ A , to be introduced separately¹.

It is a further peculiarity of Sneed’s informal semantics that it includes, besides the fundamental ingredients of physical theories just mentioned, the special laws and special constraints which hold in some (but not in all) applications. As already mentioned in § 3, using the concept of force in classical particle mechanics for illustration, the extension of a theoretical concept is partly determined according to which laws about this concept hold in which applications. Again, only by giving full consideration to the special laws and special constraints, can we learn about how the various applications ‘hang together’. The general mathematical structure of the theory, as exhibited in the axiomatization according to the Suppes approach, does not reveal anything of this kind. Finally, these special laws and constraints are essential to a proper understanding of the dynamic aspects of a theory. Just as the development of classical particle mechanics was determined by those *new special laws* which were discovered and applied in old as well as in new applications, so in any other case the *evolution* of a theory can only be understood by taking into account the

¹ This is a merely technical point. Whenever it is not necessary to include r in K , it is advisable to use the simpler construction of K as a quadruple, as was done in [48]. In other cases, this is not possible, so that r *must be* included into K . The latter construction was used in [47], [52], and [4]. The interested reader will find more about these technical details in the appendix.

processes of discovery, of improvement and, of course, of occasional abandonment of alleged special laws and the constraints connected with them.

The systematic treatment of special laws within Sneed's framework underwent radical change since the publication of [47] and [52]. The original procedure consisted in the construction of so-called *expanded cores*, as described by Feyerabend in his review. This method had several disadvantages, theoretical as well as practical ones. The main theoretical shortcoming was that laws and constraints could not be analyzed separately, because they were lumped together into the two big classes L and C_L respectively. Therefore, for example, no systematic distinction could be made between laws of different degrees of generality. A great practical disadvantage was the clumsiness of the 'application function' which was needed in order to formulate empirical claims².

It was the idea of Wolfgang Balzer, Munich, to reconstruct a special law as a miniature theory or a *sub-theory* obtainable from the original theory by means of *core specializations*. This, by the way, is in accordance with scientific terminology, for physicists speak alternately of the *law of gravitation* or of the *theory of gravitation*. If the main theory is represented by the ordered pair $\langle K, I \rangle$, consisting of the *core* K and the set I of intended applications, then a special law can be represented by $\langle K', I' \rangle$ with the 3 necessary conditions holding: $M' \subseteq M$, $C' \subseteq C$, $I' \subseteq I^3$. In such a case we say that the law $\langle K', I' \rangle$ was obtained from $\langle K, I \rangle$ by *theory specialization* and K' from K by *core specialization*. If, in addition, the *naïve view of laws* holds, we have a further condition, namely, that $M'_{pp} \subseteq M_{pp}$. According to this view, we first choose a set of non-theoretically described situations, i.e., we define a subset of M_{pp} , and then state the law we assume to hold in them. Almost all philosophers have looked at laws this way. A more sophisticated view of laws does not presuppose the possible applications to be given at the outset. Rather, such a view would let the laws themselves determine their range of application. This idea amounts to what I have called in [52], p. 176 and 196, the '*method of autodetermination of the applications of a theory*', now generalized from the main theory to those of its sub-theories for which this sophisticated view obtains.

Thus, the new method makes no formal distinction between theories and laws. Let us therefore coin the new term "*theory-element*" for both of

2 In his review Feyerabend for the sake of simplicity mentions just the simple function A , defined in [52] on p. 116. But this function can be used only with a *core* K as an argument. In the case of an expanded core, the much more complicated function A_e from p. 117, D 23b, has to be used. The latter definition corresponds to D 35 in Sneed's [47], p. 181, which, however, is not correct.

3 A technical detailed account of the new method is given in the article of Balzer and Sneed, [4]. Here, the general theorems which Feyerabend missed in my book are proved. Proofs of similar theorems within the old framework would have become extremely difficult, in view of the complicated structure of the function A_e .

them. What was originally called “*the theory*” now becomes the distinguished theory-element from which all specializations start. This is called the *basic element* whose first member is the *basic core*.

What results from this method of introducing new laws by specialization is a whole *net of theory-elements* or briefly: a *theory-net* N . By cancelling the second member of pairs, respectively, we get the *net of theory-element cores* or briefly: the *core-net* N^* . Whenever it becomes necessary to distinguish them from the pragmatically enriched nets of § 5 we shall call them *abstract nets*. If, in a theory specialization or core specialization, the proper inclusion relation holds, we speak of a *proper* specialization. If a net N' is obtained from a net N by one or more specializations, we call N' a refinement of N , and a *proper refinement*, if at least one of the specializations leading to N' is a proper one. The hierarchical structure, consisting of the fundamental law, general laws, and more specific laws, is made much more perspicuous by theory-nets and core-nets than by the method of expanded cores.

Moreover, the description of empirical claims becomes much simpler. To each theory-element $\langle K, I \rangle$ we assign the *empirical claim* of this element: $I \in A(K)$ with $A(K) := \tilde{r}(P(M) \cap C)$. This is just a ‘stenographic’ or ‘macrological’ way of formulating the emended Ramsey-sentence corresponding to the given theory-element. It says that to the elements of I theoretical functions can be added in such a way that they become models, i.e., elements of M , and that, in addition, the array of theoretical functions satisfies the constraints C^4 . The empirical claim corresponding to the whole net N has the form $I \in A(N^*)$ and can be considered as the conjunction of the claims mentioned before, corresponding to *all* theory-elements of the net.

We have now described all of the novelties falling under the common heading “*informal semantics*”. Every single semantical step creates an enrichment as well as a complication in the erection of a physical theory. The totality of these steps amounts to the transition from a *mathematical skeleton* of a *possible* theory to a ‘*real empirical theory*’.

Let us summarize these steps: (1) The prejudice, implicitly contained in the formal language approach, according to which a theory must always have one big application, has been replaced by the idea of an open set I of *numerous, partly overlapping, intended applications*, all of which are ‘*anchored*’ in a *paradigmatic* subset I_0 of I ; (2) the *theoretical – non-theoretical dichotomy* forces us to split up the extension M of the one set-theoretical predicate, representing the mathematical structure of a physical theory, into the three sets M_{pp} , M_p , and M ; (3) taking into account ‘cross-connections’ between possible models

4 In order to meet the objections of heterogeneousness of I , in [56] I chose I to be not a subset of M_{pp} but of the power set of M_{pp} . $I \subseteq \text{Po}(M_{pp})$. In that case the empirical claim is: $I \subseteq A(K)$.

leads to the introduction of the concept of *constraints*, as distinguished from fundamental as well as from special laws; (4) the way special laws are obtained from the fundamental laws by chains of specialization and how they are assigned to particular elements of I , i.e., to particular intended applications, is revealed by the construction of the *theory-net*.

Suppose we add to this list the most important *intertheoretical* semantic relations, like *strict* and *approximative reduction* as well as *presupposition*, which are still the subject of intense study. We then obtain that kind of 'Sneedification of the Suppes approach' which justifies our speaking of an *extension* of the Bourbaki programme to physical sciences instead of an *integration into* this programme only. The extension consists in including 'pieces of the real world', like the I 's and M_{pp} 's, and so non-mathematical entities, into the systematic account. In the near future no more than a very fragmentary realization of this programme will be possible, restricted to theories whose basic mathematical structure is sufficiently clear to allow axiomatizations which meet the demands for rigor in the sense of Suppes.

The concept of a theory-net induces generalizations in which several *kinds of* theories in *hierarchical order*, vaguely alluded to in the presupposition relation, are explored. Roughly speaking, within such an order the M_{pp} 's of a given theory are the M_p 's or M 's of an *underlying* theory. In the course of the systematic study of such hierarchical structures important new intertheoretical relations may be discovered. Among other things, it is to be hoped that in this way many of the still controversial problems of the space-time-philosophy will be solved (§ 11 see also).

§ 5. Pragmatization. Theory-Evolution in Scientific Communities

As already pointed out, informal semantics cannot be sharply distinguished from pragmatics. The theoretical – non-theoretical dichotomy, for example, exhibits pragmatic traits because of its reference to the *use* made by certain laws. Besides, the present framework can, in a natural way, be enriched by concepts of an unquestionably pragmatic kind. Thereby it may perhaps turn out to be a starting point for a systematic pragmatics which has been overdue for a long time. We shall speak of ‘concepts of an unquestionably pragmatic kind’ if they refer to *persons* and *human institutions*, to *knowledge situations* and to *beliefs*, to *confirmation* and *test procedures*, to *historical time intervals*, etc.

The most important pragmatic notion introduced in [47], [52], and [53] was that of *having or holding a theory*¹. It was distinguished from the non-pragmatic concepts of a theory-net and of the empirical claims of theory-elements and theory-nets. The main purpose for introducing this notion was to build a bridge between the systematically and the historically oriented philosophy of science. In particular, with the help of this concept it was possible to make precise some important aspects of the concept of *normal science* in the sense of Kuhn and of *research programme* in the sense of Lakatos. The fact that several persons belong to the same tradition of normal science can be expressed, for example, by saying that they hold one and the same theory.

Since the introduction of that notion, Moulines has shown in [32] that we can as well use theory-nets as starting points for introducing and analyzing pragmatic concepts. Let us call theory-nets in the sense of § 4 *abstract nets* (*a. nets*). They consist of abstract theory-elements, i.e., of pairs $\langle K, I \rangle$, where K is a quadruple $\langle M_p, M_{pp}, M, C \rangle$ (neglecting the ‘lopping-off function’ r) and I is a set of intended applications. The hierarchical arrangement of the *a. theory-elements*, i.e., their partial ordering, is engendered by the relation of specialization.

We now decide to create *pragmatically enriched theory-nets* (*p.e. theory-nets*) out of pragmatically enriched theory-elements (*p.e. theory-elements*). A

1 Apart from minor details, the main difference between my characterization of this idea and Sneed’s consisted in my attempt to get rid of treating the set I of intended applications as a Platonic entity, contrary to Sneed.

p.e. theory-element is a quadruple $\langle K, I, SC, h \rangle$. Here, K and I have the same meaning as before. SC is a *scientific community* and h is an *historical time interval* such that SC intends to apply K to I during h . Core specializations and specializations of theory-elements are introduced in perfect analogy to the abstract case. We thereby obtain a *p.e. theory-net* N in which the scientific community and historical time remain unchanged throughout, i.e., for every pair $T_i, T_j \in N$, $SC_i = SC_j$ and $h_i = h_j$. To each p.e. theory-element and to each p.e. theory-net there uniquely corresponds an abstract theory-element and an abstract theory-net respectively.

Such p.e. theory-elements and p.e. nets can be used *to study various kinds of developments of theories, called theory-evolutions*². Such a study is of interest in itself. But it should be noted that the following passages may also be taken as a preparatory step towards an adequate understanding of Kuhn's notion of 'normal science'. For, as will become clear shortly, that a scientist works within an historically given frame in no way implies an 'irrationalism' on his part.

Viewed temporally, a particular p.e. net exhibits a *static structure*. For with each p.e. theory-net N there is a uniquely associated historical time interval h_N . We obtain *temporal sequences of p.e. theory-nets* by means of the relation $h_N \leq h_{N'}$, saying that the time interval h_N associated with the p.e. net N is *previous to* the time interval $h_{N'}$ associated with the p.e. net N' . "The p.e. net N' immediately follows the net N " is short for: " N is previous to N' and there is no p.e. net N'' , different from N and from N' , such that $h_N \leq h_{N''} \leq h_{N'}$ ".

A *theory-evolution* E is a finite sequence of p.e. nets $N_1, N_2 \dots$ such that, for every i , $i \geq 1$, N_{i+1} immediately follows N_i , and N_{i+1} is a proper refinement of N_i .

We may analyze *epistemic features* of a theory-evolution by building in additional factors leading to finer distinctions. In particular, certain *confirmatory aspects* can be included without committing oneself to a preconceived theory of confirmation. Suppose, for example, that there are certain *epistemic relations* between SC on the one hand and the intended applications of its abstract theory-nets on the other hand. For the present purpose it would be sufficient to assume that there is *general unanimity* among the members of SC as to whether a subset I' of I is a *well-confirmed* application of K or not. In the positive case we call the set of applications I' *secured for all* members of SC during the historical period h . There may be other applications in I which are only *believed by some* members of SC , i.e., by a subset SC_0 of SC . (SC_0 could even consist of only one member). Here we are analyzing the situation *completely*

2 I should like to emphasize that Moulines' choice of the word "evolution", and equally my own choice of the expression "evolutionary tree" in § 6, are made only for the sake of *vividness*. No parallel to biological evolution is claimed. Therefore, e.g., the criticisms of J. Losee, [25], do not apply to the present cases.

independently of whether all or some of the epistemic attitudes of these members seem acceptable to us or not. In other words: *we remain epistemologically neutral*.

Epistemic neutrality in this sense must sharply be distinguished from our being not more specific with respect to the epistemic relations mentioned. While the latter means that we leave it completely open whether *the scientific community under consideration* consists of ‘inductivists’ or ‘deductivists’, of ‘Bayesians’ or ‘Anti-Bayesians’ etc., epistemic neutrality means that *we* do not commit *ourselves* in any of these aspects. Of course, we may decide to give up our indifference in either of these two dimensions or in both of them. In the one case, this would result in a study of theory-evolutions whose members are ‘Popperians’ or ‘Bayesians’ etc. In the other case, i.e., withdrawing the epistemic neutrality, this would mean not only *describing* a (more or less specific) theory-evolution but *evaluating it critically*. Combining both, we would get things like, e.g., ‘inductivist analyses of Popperian theory-evolutions’, ‘Popperian analyses of Bayesian theory-evolutions’ etc. Strictly speaking, the neutrality is withdrawn only in the moment we *decide to accept* one type of analysis as *epistemically superior*. This observation will be relevant at the end of this section when we comment on the *normative-methodological aspect* of Lakatos’ theory of research programmes.

Viewed from this confirmatory aspect, theory-evolutions can be ‘*optimal*’ in *two different respects*. First, it may happen that for two arbitrary nets N_i and N_j of a theory-evolution E ³, such that N_i is previous to N_j , the set of secured applications of N_j properly includes the set of secured applications of N_i . This is the case of a *progressive theory-evolution*. Intuitively speaking, a progressive evolution takes place not only if new laws of different levels of generality are successively discovered (as happens in *each* case of evolution), but if, in addition, there is an increasing improvement of the confirmation situation consisting in a successive growth of the range of secured applications. Secondly, a progressive theory-evolution E may be such that for every net N_i in E , different from the last member in E , there is a ‘later’ net N_j , i.e., with $h_j > h_i$, in E for which the following holds: all the applications assumed by some members of the scientific community at the time of N_i are secured for all members of this community at the time of N_j . This is a *perfect theory-evolution*. Intuitively speaking, a perfect theory-evolution, besides being progressive, is ‘hope-fulfilling’ in the sense that hypothetical assumptions of certain members of the community at a given time later turn out to be well-confirmed according to the ‘confirmation codex’ of the community, so that these assumptions can be accepted as secured.

While the concept of a progressive theory-evolution still captures a ‘realistic idea’, being reminiscent of a ‘progressive research programme’ in the sense of Lakatos, “perfect theory-evolution” denotes an ‘utopian ideal’ for which, in all probability, there are no historical examples.

3 Let us not forget that, by definition, a theory-evolution consists of a successive, step-by-step *refinement* of the original net.

The possible supplementations and modifications mentioned at the end of § 4 could be used to introduce suitable richer concepts of evolutions. Then one could analyse the developments not only of *single isolated* theories but of whole hierarchical structures of theories. Such a study has to wait until the systematic results concerning the appropriate intertheoretical relations are available.

The Kuhnian notion of a *paradigm* can also be incorporated into the concept of theory-evolution in such a way that we free ourselves from the special and too narrow assumption of [52], according to which the paradigm was first introduced by *particular persons* (or by *one person*). For convenience, further discussion of this point will be postponed until § 8.

The pragmatically enriched conceptual apparatus sketched above derives part of its importance from its usefulness for the study of particular historical processes. As Moulines was able to show, the *evolution of Newtonian particle mechanics* was a progressive evolution for a certain period of history but it was never a perfect one. From such results it does not follow that the utopian character of perfect evolutions makes this concept worthless. It can be used, for example, to get a firm grasp on the ‘gap between ideal and reality’, the ideal being posited not on the meta-level by the analyzing philosopher but on the object-level by the members of the scientific communities.

This does not mean, of course, that within the present approach ‘ideals’ can only be *described*. They can be grasped *normatively* as well. As has been hinted before, the philosopher who has acquired a confirmation- and test-theory for himself can *critically comment* on the individual steps of a theory-evolution. For such a comment to be epistemologically and historically useful, the *empirical claims* at the various stages of the evolution must be carefully analysed and reconstructed. Besides, special care is needed for the description of the confirmation procedures holding in *SC*. This is worth mentioning because there is often a great difference between the methods *really used* by scientists and the methods *they believe they have used*. The man, philosopher and scientist, I. Newton is an impressive example for this, but far from being the only one. In any case, the comment will be more *interesting* the greater the difference between the conceptions of test and confirmation of the members of *SC* on the one hand and of the philosopher tracing the theory-evolution on the other hand. Perhaps the ‘*normative-methodological*’ aspect of ‘critical rationalism’ can at least partially be interpreted in this way. I would only warn against absolutism in either direction. Interesting as it may be to criticize, from a Popperian point of view, a theory-evolution of a community whose members adhere to ‘inductive prescriptions’, evaluating a theory-evolution of headstrong non-inductivists from a Bayesian point of view could perhaps be even more interesting.

§ 6. Progress, Progress-Branching, Kuhn-Loss and Rationality in Science

The concept of progress as applied to normal science in the sense of Kuhn, i.e., of progress with the basic core retained, has already been discussed in § 5. In addition, the last paragraph of § 5 pointed out an ambiguity of “progress” in the sense of “progress with respect to confirmation”. It can either refer to such progress *according to the convictions of the scientific community* which belongs to the historical sequence of pragmatically enriched nets, or it can mean confirmatory progress *from the viewpoint of the philosopher (and historian) analysing these phenomena*. The first could be called confirmatory progress on the object-level and the second confirmatory progress on the meta-level. We have realized that these two meanings of “progress” can, and often will, diverge from each other. Nevertheless, for the sake of simplicity, we shall use in this section only one undifferentiated notion of progress. This will do no harm, because the reader can, at any point, use either interpretation mentioned.

Even disregarding the distinction between object-level and meta-level we can, from a logical point of view, distinguish between three kinds of ‘normal scientific’ progress: (1) *theoretical progress*, consisting of a refinement of the net of theory-element cores, (2) *empirical progress*, consisting of a successive increase in the set of intended applications: $I_t \subset I_{t+1}$, and (3) *progress in confirmation* by which assumed elements are transformed into firm elements of I .

In each of these three kinds of progress there is the possibility of *progress branchings*¹. We can speak of the possibility of a *purely theoretical progress branching* at an historical time t if, in the situation at t , at least two different refinements of the theory-net can be produced successfully but if, in addition, they can only be performed simultaneously either on pain of inconsistency or on pain of incompatibility with accepted empirical data.

For the second type of progress branching let us confine ourselves for the sake of simplicity, to the case of a single theory-element core K and the set of its intended applications I . We may speak of a *purely empirical progress branching* if these ‘data’ to which K is applied² are growing over time in such a way

1 I mentioned this phenomenon before in [53], pp. 158–161, and in [54], pp. 30–32.

2 This is just a picturesque way of saying that $I \in A(K)$.

that the growing paths are *mutually incompatible*. Sneed has studied these growing processes in some detail in [49], p. 10ff. His starting point is the concept of a *maximal K-compatible set*. This is a *possible set* I which can not be enlarged without thereby pushing it outside $A(K)$. Intuitively speaking, such a maximal K -compatible set is a *possible direction* in which successful applications of K might develop over time. *There is no reason at all to assume that for a given K there is only one maximal K -compatible set.* On the contrary, normally the number of these ‘ideal empirical possibilities of development’ of the theory-element core K will be very large.

There even exists the following third possibility of branching: It may happen that a scientific community at a time t can *either* successfully refine the net while leaving the set I_t untouched *or* choose one I_{t+1} (or even several I_{t+1} ’s) such that $I_t \subset I_{t+1}$ but only at the cost of not refining the net.

I am not quite sure how such situations would be described by Kuhn. According to the text of his Postscript 1969, branchings are not possible in the ‘normal scientific’ case. On the other hand, I would prefer to reserve the word “revolution” for the case of ‘theory-dislodgement’ where a fresh start is made at the bottom, starting with a new K_0 . Perhaps, the best way would be to distinguish the present cases as a *third* category, leaving the detailed description to the historian depending on the particular case. For some historical situations he may prefer to say that the scientific community subdivided into two parts, each one following a different path. In other cases it will be more appropriate to speak of a small subgroup splitting off from the main community etc.

These results describe, of course, only *possibilities*. To establish them for particular physical theories at certain stages of their development, thereby illustrating them with concrete examples of ‘real life’, would be a matter of historical research. Despite their being potentialities only, they are of twofold importance, if looked at from a merely systematic point of view. First, they reveal the admissibility of the ‘evolutionary tree’ picture for scientific evolutions. Secondly, they localize certain *bounds of human theoretical reason*.

These bounds are of a very different sort than those which Kant claimed to have discovered. Let us try to formulate the difference. The question of *whether there is progress* in one of the respects mentioned can in principle be answered by theoretical reasoning only, this reasoning including, of course, the evaluation of ‘agreement with the data’, ‘explanatory force’, etc. However, the question of what *ought to be done at the points of bifurcation* can no longer be answered by theoretical reasoning alone. This does not mean that theoretical reason bounces against limits which it cannot transcend. It only means that theoretical reasoning gives way to practical deliberation. For it is a question of what *to do*, i.e., a problem of *decision*.

No precipitate conclusions should be drawn from this. In particular, one must not interpret this as saying that at those points ‘rationality comes to an end’ and we therefore have to surrender to ‘subjective arbitrariness and irrationality’. For “rationality” does not mean the same as “theoretical reasoning”.

Borrowing Kant's terminology, we could say that at the points of possible progress branching we have to change over from the domain of *theoretical reason* to the domain of *practical reason*.

Carnap's later work on so-called 'inductive logic' might in this connection gain in unexpected interest, at least if reinterpreted as laying the foundations of decision theory, as I interpreted it in [51a].

T.S. Kuhn has emphasized several times the role of *value judgements* and *decisions* in the course of scientific development. Most critics have counted this against him as symptomatic of his alleged 'epistemological subjectivism'. However, we cannot forego 'value judgements and decisions' at points of bifurcation, since there are no purely theoretical reasons telling us whether we should follow all possible paths or only some and if so which. Hence, at least with respect to those periods belonging to what Kuhn calls "normal science", the criticism is unfounded.

In fact, the criticism is unwarranted in the 'revolutionary case' as well. Here, the situation becomes philosophically much more dramatic and exciting, and much more complicated as well. First, not only does there *exist* the possibility of progress branching. Where one theory T is dislodged by a more progressive theory T' ³, we can hardly say anything substantial about what other, relative to T equally progressive theories T'' , ..., $T^{(n)}$ *could have been contrived*, if none of these theories entered a human being's mind. Secondly, we could encounter a fundamental problem this time. What does it *mean* to say that a scientific revolution, in the course of which a new theory supplants an old one, 'is *progressive*'? With respect to this question, many philosophers seem to be skeptical and consider it a hopeless task to explicate such a concept of progress. Since the same philosophers believe in science as a rational and progressive enterprise, they feel themselves forced to contest the notion of a 'radical paradigm change' in the sense of Kuhn.

Part of this skepticism presumably comes from the statement view, i.e., from thinking of theories as classes of sentences. For sentences, formulated in different languages with different vocabularies, seem to be incomparable. Within the structuralist approach on the other hand, the internal structure of different theories, as expressed in their core-nets and theory-nets, can be analyzed in the same informal language. Therefore, there is some hope that the concept of 'revolutionary' scientific progress, as a relation between theories with different cores, can be explicated as well. If it can be done at all, it must not be done with the help of a teleological metaphysics but in terms of *suitable intertheoretic relations*.

3 By this I mean, roughly speaking, that a whole net, *together with its basic core*, is abandoned and tentatively replaced by a different net with a *different* basic core.

In [52], I expressed the daring hypothesis that the relation of *reduction* will do the job. The basic idea goes back to Adams. Sneed improved it in many respects in [47], making it applicable to his much more complicated and sophisticated framework. It was greatly simplified by me in [52], without loss of precision. Finally, it was transferred into the ‘language of nets’, together with various improvements, by Balzer and Sneed in [4].

I called the claim that the concept of revolutionary progress can be reconstructed in terms of an intertheoretic relation of reduction “daring” because the characterization of the reduction relation on the level of general philosophy of science (see § 8) does not tell us how the *special* problems are to be solved, problems which arise as soon as this relation is to be applied to two particular theories one of which supplanted the other.

The basic idea of the reduction relation can be intuitively characterized as follows. We have to start from the fact that the reducing theory T' is normally richer in means of expression than the reduced theory T . What is, from the standpoint of T , one and the same state of affairs, may split up into many different states of affairs, if viewed from T' . Therefore, in the general case, there will be no injective mapping between M_{pp} and M'_{pp} but a one-many relation R ; in other words, R *reductively corresponds* M_{pp} with M'_{pp} , $rd(R, M_{pp}, M'_{pp})$, in the sense that the converse of R is a function from M'_{pp} into M_{pp} . The concept of *reduction* of a given theory element T to a theory element T' is to capture the idea that there is an R such that, first, $rd(R, M_{pp}, M'_{pp})$, and secondly, everything the reduced theory-element says about a given application is entailed by what the reducing theory-element says about any corresponding application. With \tilde{R} for the relation ‘working one level higher’ than R^4 , the second requirement can be expressed by:

“for all $\langle X, X' \rangle \in \mathbf{Po}(M_{pp}) \times \mathbf{Po}(M'_{pp})$, if $\langle X, X' \rangle \in \tilde{R}$ and $X' \in A(K')$, then $X \in A(K)$ ”.

(Remember that ‘what a theory says about a given application’ is expressed by a Ramsey-Sneed-sentence of the form $I \in A(K)$.)

The *strong reduction relation* is the analogue of this reduction relation at the theoretical level⁵. The sentence which thereby corresponds to the phrase quoted above says that, whenever a set of ‘reducing structures’ Y' satisfies the theoretical laws and constraints of the reducing theory, the corresponding set of reduced structures Y satisfies the theoretical laws and constraints of the reduced theory.

The concept of strong reduction can be transferred to ‘theories’. This transfer is not trivial, for one must not use *arbitrarily contrived* special laws of T'

4 More exactly, if $rd(R, M_{pp}, M'_{pp})$, then \tilde{R} is the class $\{\langle X, Y \rangle \in \mathbf{Po}(M_{pp}) \times \mathbf{Po}(M'_{pp}) \mid \text{there is a bijective function } \varphi: X \leftrightarrow Y \text{ such that for all } x \in X: \langle x, \varphi(x) \rangle \in R\}$, $\mathbf{Po}(N)$ being the power set of N .

5 The precise formal definition is given in Balzer and Sneed, [4], D 10.

in order to 'reproduce' the effect of special laws in T . Rather, one has to use laws of T' that already have some 'standing' within T' . This intuitive requirement is satisfied if the strong reduction relation R is construed as a relation between specific *theory-nets* N and N' such that R strongly reduces the basic theory-element T of N to the basic theory-element T' of N' and every theory-element of N to some theory-element of N' .

With the help of the conceptual apparatus sketched above we may try to say what it *means* that one theory in Kuhn's sense, having dislodged a foregoing one, is *progressive* with respect to the latter. *In this attempt "revolutionary progress" is defined in terms of "reduction"*. Regardless of whether such a definition is adequate or not (it is *not* adequate in this version for reasons to be given later), we have received an interesting intermediate result: *Within the structuralist framework it is possible to introduce intertheoretical relations that apply to 'theories' which are 'incommensurable', in the sense of having very different 'theoretical superstructures'.* Of course, this is not to claim to have 'solved the problem of incommensurability'. But there are good reasons for hoping that we have thereby made *an important step toward* solving at least *one* of the intricate problems concealed behind the term 'incommensurability' (vid. § 11).

Meanwhile, the matter has turned out to be more complicated. But before entering a discussion of this problem, I should like to draw attention to the fact that, with respect to the theme "progress branching", the situation in the revolutionary case is in principle analogous to the case of evolution of *one* theory. This would hold even if we were using an intertheoretic relation which is quite different from reduction in order to define "progress". What counts, in order to use this predicate correctly, is only, intuitively speaking, a surplus in achievements of the new theory with respect to the old one. As in 'normal science', the surplus may lie either more in the 'theoretical' or more in the 'empirical' domain or in both. In any case, the number of possible ventures is indeterminable.

There is, presumably, a gradual difference between the present case and the case of a theory-evolution in two respects.

(I) The weight of the decision component is much heavier in the present case. Many philosophers writing on progress seem to feign that we live in a kind of fool's paradise in which unlimited resources and unrestricted time are at our disposal. But, alas, we live in a world where most means are scanty, human energies and abilities are severely restricted and time is limited. Therefore, a decision about how many and which ways to choose will become the more risky and weighted with responsibility the more radical the change turns out to be. Once we have realized this, it becomes obvious that, although we are dealing with a challenge to human rationality, we do not have to struggle with

problems of reasoning, but rather with problems of decision making, irreducible to 'theoretical rationality'.

(II) There is at this time a great 'phase lag' between first suspicion and real knowledge. Since the newcomers not only have to tinker away at a given theory-net but also have to thoroughly rebuild a new one, beginning with a new basic core, the scientific work may go on for more than a generation before the progressive character of the novelty becomes *known*. However much work is done on the empirical and theoretical level during this period, the researchers will derive their working energy not from these fragmentary insights at all, but *from their belief and hope in final success*. This being the case, one can justifiably say that the fresh start has *neither* a theoretical *nor* an empirical fundament and is still by no means irrational. Since the completion of the work depends on growth of the scientific community, it is small wonder that attempts are continually made to win interested persons as co-workers by means of methods which look more like propaganda than like rational arguments.

All reduction concepts mentioned above belong to the family of *strict* reduction. In many cases it will be necessary to replace the concepts belonging to this family by more liberal ones of the *approximative* type. While the structuralist approach originally contained no reference to approximation, some promising attempts have recently been made in explicating concepts like approximate applications of theories and approximative reduction of theories to others. Unfortunately, most of the studies in this field are highly technical since they require the use of strong topological methods (vid. § 10).

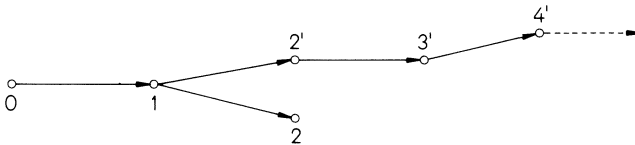
The problem of approximation is not the only one that has emerged. It has in the meantime turned out that, in addition to the *theoretical incommensurability* existing in the different natures of the theoretical superstructures of reduced and reducing theories, we encounter a new difficulty which could be called *empirical incommensurability*. Technically speaking, a general presupposition made in all the reduction concepts mentioned is not fulfilled, namely, there is a one-many relation between M_{pp} and M'_{pp} . Often no 'natural correspondence' between the partial potential models of the two theory-nets may be produced. I shall illustrate this very subtle problem in § 11 with an example on which Sneed is presently working. It is to be hoped that the result of these investigations will contribute to the solution of at least *one* further aspect of 'the problem of incommensurability'. Whether I am right or not in this assumption, it in any case contains a strong concession to Feyerabend's claim that the problem (or problems) of incommensurability are much more serious than most philosophers seem to recognize.

During the last years several authors have tried to establish the one-directedness of scientific progress by recourse to Tarski's concept of truth. It seems to me that all these attempts amount to a misuse of an important model-theoretic concept for epistemological purposes. One can try to show that it *is* a misuse

in basically two different ways. One possibility would consist in showing, by analyzing the semantic concept of truth and by evaluating its efficiency, that it is not suited for such a purpose. This kind of criticism has already been anticipated by Quine in the second to last paragraph of his [39], Chap. I, which begins with the following sentence: “It is actually when we turn back into the midst of an actually present theory, at least hypothetically accepted, that we can and do speak sensibly of this and that sentence as true.” Recently, H. Field, in his excellent article [14], has laid bare the roots of some epistemological misinterpretations of Tarski’s work and has, it seems to me, destroyed the hopes of all those who believe that scientific progress could be defined as convergence towards truth.

We have taken the second course. Our results on the possibility of progress branchings in the two main types of theory change contain a direct refutation of the conception of one-directedness of progress and thereby an indirect refutation of identifying progress with convergence to truth. From the point of view of the result, the indirect way we chose is to be preferred because it exempts us from discussing the notion of truth. All we need is the insight that the main forms of progress in physics consist either in an expansion of the set of intended applications or in a refinement of the theory-net or in an inter-theoretical relation obtaining between two theories. In all these cases branchings are possible.

The different forms of progress branching can be used to explain one aspect of a curious phenomenon, first observed by T.S. Kuhn and since that time called *Kuhn-loss*⁶. The situation can be graphically represented in the following way:



Each arrow represents one progressive step. The exact nature of these steps is irrelevant for the present purpose. Point 1 is a node at which a twofold bifurcation started. But in one of the two branches, at point 2, the progress came to an end. Remember that this does not mean something like “definitive falsification at point 2”. It just amounts to something like this: the members of the scientific community, possibly after a very long historical period of unsuccessful attempts, gave up their hopes for making any progress at this point.

6 I deliberately claim to refer only to *one aspect* of this phenomenon. A very different account would have to be given for the fact that, as Kuhn emphasizes, after a revolution some of the old problems become pointless or are even declared ‘unscientific’.

If it was originally decided to take the way from 1 to 2 only, then the revision had to start at the earlier stage 1. Starting from there, newer and newer progressive steps were made. *But the particular achievements from 1 to 2 can never be produced again.* The picture is in a certain way misleading, since it is, of course, *possible* that at some stage of the other branch: 1, 2', 3', ... all these achievements *will* be reproduced. But it is *not necessary* that such a thing will ever happen. And this is all we need. For then there will be a *partial incomparability* between 2 and *all* points on the other branch.

We may draw two conclusions from this: First, if by “the solution of the problem of incommensurability” is meant to prove that in every case of *apparent* incomparability there must be an underlying ‘true’ comparability, then there is no solution to this problem. Secondly, the fact that progress was stopped at 2 does not imply that the scientific community must forego the achievements reached at this point. They may *decide* to do so or they may *decide* otherwise. How they decide will depend on how important the irreproducible achievement was for them. Even in cases of blocked progress, practical reasoning may supersede theoretical reasoning at branching points.

§ 7. On the Importance of the Distinction between General and Special Philosophy of Science. Non-Statement View₃

Feyerabend complains on p. 363 about the irritating repetitions in my [52]. I shall, first, classify these repetitions and then try to show of what nature they are. In itself, it would be very silly to take up such an unimportant point. The only justification I can give is that I intend to bring up for discussion an important distinction which can best be explained by reference to the repetitions mentioned.

(1) In Part I, Chaps. 1–5, I gave a comprehensive account of the Ramsey method as modified and emended by Sneed. The method used is an *intuitive-linguistic* one in which the symbols of modern logic serve only the purpose of abbreviation. There were two independent reasons for the choice of this starting point. First, the most difficult aspect to understand in Sneed's approach seems to be *the problem of theoretical terms*. Since this is the origin for recourse to the Ramsey view and for the various modifications of the Ramsey-sentence, it seemed natural to choose this point of departure. Secondly, the German edition of [52] was only the second half-volume of [49a]. The last Chapter of the first half-volume contained a discussion of the Ramsey-sentence from the traditional point of view. From there it seemed wise to begin the second half-volume with a continuation of this discussion which, because of the first reason, was necessary anyway.

(2) In Part I, Chap. 7, the intuitive-linguistic representation was replaced by the *informal set-theoretic* one. The modified Ramsey-sentence (Ramsey-Sneed-sentence), which in the 'linguistic' version was, despite the use of logical symbols, a rather unhandy formula, namely formula (VI) on p. 90, is now transformed into a statement which looks like an atomic proposition of set theory, namely $I \in A(K)$ or $I \in A_e(E)$ [or, in the present formulation: $I \in A(N^*)$].

(3) The investigations in (1) and (2) do not refer to any particular theory. In order to illustrate the conceptual apparatus with a particular example, a *fictitious mini-theory* m is used. (It is 'fictitious' in no stronger sense than that in which *all* discussions within the *st.v.*₁ are fictitious, as explained in § 1.)

(4) In Part I, Chap. 6, the general apparatus is applied to a non-fictitious, 'real' *empirical theory*, namely classical particle mechanics. This section is much shorter, however, than the corresponding discussion in Sneed's [47].

From a systematic point of view, (1) and (3) are unnecessary. They can be justified on didactic and psychological grounds only (and these, of course, apply to the average reader but not to an experienced reader like Feyerabend). The difference between representations of kind (2) and (4), on the other hand, are irremovable. Expositions of kind (2) belong to *general philosophy of science*, those of kind (4) to *special philosophy of science*. Despite admitting graduations, this difference is fundamental. Because I became conscious of its importance only after publication of [52], I shall make a few remarks on it.

The attitudes of philosophers of science toward this distinction differ greatly. On the one hand, there are those who believe that most of the important 'metascientific' concepts – like 'theory', 'law', 'explanation', 'disposition', 'corroboration' etc. – can be explicated within general philosophy of science. Practically all positivists and earlier empiricists, but many other philosophers as well, belong to this first group. On the other hand, there are some who are skeptical in this respect and who believe in the possibility of explication only in the context of particular theories. P. Suppes, e.g., must presumably be assigned to this second group. (This, by the way, is perhaps the reason why his name is usually not mentioned when people speak of 'the great philosophers of science of our time.' In the writings of Suppes we hardly encounter sweeping remarks on all of science. But, apparently, in this best of all possible worlds one must drop remarks of such a kind in order to be considered a great philosopher.) Some of the more historically oriented philosophers must also be considered as belonging to the second group since, in their opinion, not only the content of scientific terms but that of metascientific terms as well is altered in case of radical paradigm change¹.

When my [52] was published I would have considered myself as belonging to the first group. While I am still convinced of the importance of general philosophy of science, my position has become closer to that of Suppes in the following essential respect: Whenever the matter at issue is the explication of an important metascientific concept then we can, on the level of *general philosophy of science*, give no necessary *and* sufficient conditions for this concept. Rather, we must be content to give some necessary conditions.

With respect to my [52], this has the following effect. Most 'definitions' given there, belonging to level (2), lose this character and have to be interpreted as giving necessary conditions only. Technically, this is reflected in the introduction of concepts in [52] like "n-matrix for a theory of mathematical physics" (p. 108), "is a constraint for" (p. 113), "is a law" (p. 114), by the fact that

1 For illustration, vid. Shapere, [45], p. 122.

all the “iff’s” *have to be replaced by “only if’s”*. (If, for practical reasons, it seems advisable to have *definitions* available at the general level, we may keep the iff’s and put the expression “potential” in front of the defined term).

What I just described is not just a technical manoeuvre in order to meet justified objections of Tuomela². Rather, it mirrors a drastic change of my philosophical attitude. As I emphasized elsewhere³, it is possibly *the* fundamental error of most present philosophers of science to look for definitions, giving necessary and sufficient conditions, within general philosophy of science. I do not want to enumerate the reasons for the legitimacy of a general philosophy of science. In the present context the purely practical indication may suffice that a systematic exercise in the new apparatus of nets, subnets, and their relations on the abstract level will be imperative in order to acquire sufficient familiarity with this apparatus for application to particular cases.

Let us illustrate the importance of reflections belonging to *general* philosophy of science with an example from a very different field, namely *scientific explanation*.

(I) Up to now no one has succeeded in giving even an adequate explication of “deductive-nomological explanation of facts”. This may be taken as symptomatic of the fact that necessary *and sufficient conditions* for this concept cannot be given in general terms. This claim gets additional support from the fact that “explanation” is a *pragmatic* term and that the sufficient pragmatic conditions will vary with the topic.

(II) This negative statement notwithstanding, one can find out, on the basis of general considerations only, that three types of cases must be systematically distinguished:

(1) *The explanation of facts by means of deterministic laws*. Explanations of this kind presuppose the truth of the explanandum. For it would be absolutely counterintuitive to claim that something had been explained that *could* have happened but actually *did not* happen.

(2) *Statistical explanation*. Two types are to be distinguished, neither of which, properly speaking, ought to be called ‘explanation’.

(a) To the one class there belong those cases which were first investigated by Hempel. His procedure has recently been improved upon by P. Gardenfors⁴. Here the attempt is made to parallel the explanations of kind (1) as much as possible with the probabilistic case. As I have shown elsewhere⁵, the explica-

2 Vid. [61]. In these objections, counterexamples are given which satisfy the definiens, although they are not intended as instances of the definiendum.

3 [56].

4 [15].

5 [51 a], second half-volume.

tion founders on the “*paradox of the realizability of the improbable*”. For, an event correctly predicted even with high probability need not happen. Whether it *will* happen depends on chance. In the negative case we would have to give that kind of answer to the ‘explanation-demanding question’ which, in (1), we just recognized as absurd. That we, nonetheless, are inclined to use the expression “explanation” has, presumably, a psychological reason: if the event happens – and it will happen more often than not in case of high probability – then *it is thereby explained why it was rational to expect the occurrence of the event*. In other words, not the event but the rationality of its expectation is explained. This rationality remains intact even if the expectation should be unfulfilled.

(b) The second class consists of those cases which W. Salmon tries to account for in his ‘relevance theory of statistical explanation’. This time, again, the ‘paradox of the improbable’ reveals the inadequacy of the term “explanation”. For, in case of negative relevance, Salmon would have to say that the event happened *because* something else took place which *lowered* the probability of its occurrence. But this, of course, is absurd too.

I therefore suggested in [51a], IV, to assign the first-type cases to the domain of *statistical inference* (‘single case probabilities’) and to speak in the second-type cases of *statistical analyses*. Those cases where before the occurrence of the event a type-(a)-argument was given (‘the event was to be rationally expected’) and after its occurrence a type-(b)-analysis provided positive relevance (‘the analysis provides positive relevance’), may be called cases of “explanation”. But this is hardly more than a linguistic decision in favour of which as well as against which several arguments can be brought forward.

(3) *Explanation of theories*. This type of case is different from (2) and from (1). This time it makes sense to speak of explanations, in contrast to (2). In order to see the difference between (3) and (1) we first have to notice that the most important type of explanation of theories is not that of *strict* explanation but that of *approximative* explanation. The “explanation” of Kepler’s laws by Newton’s theory of gravitation is of this kind. As will be *explained* in some more detail in § 11, the approximatively explained theory is wrong (and only ‘approximatively true’), if viewed from the standpoint of the explaining theory. In the particular example this means that, if Newton’s theory is correct, then Kepler’s laws are wrong. Therefore, in contrast to (1), the *falsity* and not the truth of the explanandum is part of the explicated concept.

These considerations under (II) are intended to show that it would be premature, after acceptance of (I), to jump to the conclusion that ‘general philosophy of science is of no use at all’!

Let us now return to the topic “statement view”. In § 1 and § 3 we distinguished between two meanings of this expression, both belonging to *special philosophy of science* which deals with particular theories. With respect to

both conceptions of ‘statement view’ we came to a negative result. *Statement view*₁ is identical with the formal language approach and the latter is, with respect to ‘interesting’ physical theories, although logically possible, *not possible for humans* (presently and for centuries to come); vid. § 1, thesis 1 and its discussion⁶. *Statement view*₂, according to which empirical claims of physical theories are to be represented by classes of sentences, was given up too; vid. § 3. The reason was totally different from the first one. We remember that it consisted in the fact that, at least up to now, no solution to the problem of theoretical terms other than the Ramsey solution is known. This result, too, was derived from an *empirical* premise, namely, that Sneed’s conception of theoreticity is in principle correct. Finally, at the end of § 3, we introduced, in addition to *non-st.v.*₁ and *non-st.v.*₂, the notion of *non-statement view*_{2,5} in order to account for the way in which a law of the theory-net enters the holistic claim of *non-st.v.*₂, i.e., the Ramsey-Sneed-sentence. The reason for doing this is, as I shall explain at the end of the next section, that laws which *prima facie* and considered in isolation seem to be falsifiable become irrefutable parts of the holistic claim.

All of these results leave the question in principle unanswered what position we are to accept in *general* philosophy of science. Here, to weigh the pros and cons of the ‘statement view’, as done by Feyerabend in [13] on p. 359–363, is justified. And we *might* come to the result that for various reasons, some of which are given by Feyerabend, we should, within general philosophy of science, favour the statement view. But even here we must be cautious and explain exactly what we mean. Otherwise we would get into trouble, as we shall immediately see.

By *statement view*₃ I shall understand the position according to which philosophical talk about theories, their achievements and their drawbacks *within general philosophy of science* conceives these theories as classes of sentences or classes of propositions. It would be too easy to argue in favour of this attitude as follows: “After all, this is the view accepted by almost all philosophers of science and by most scientists.” It would be too easy a life even in general philosophy of science to argue like that. For, *if* the arguments against *st.v.*₁ and *st.v.*₂ are correct, then *st.v.*₃ can be tenable only in combination with some kind of philosophy of the as if. Thus, the argument may not run as follows: “Let us, for such and such reasons, within general talk about theories, their interrelations, their applications, their corroborations and their failures, understand by ‘theory’ the same as ‘class of sentences of a certain kind’.” The

6 Besides, even if *statement view*₁ were humanly possible, it would still be highly questionable whether there are *any* reasons to follow this line of thought. For, *what additional philosophical and metascientific insights would we have to expect by replacing informal set theory by a formalized one?* Remember the analogous situation in mathematics between *Bourbaki* and *metamathematics*!

argument can only run something like this: “For such and such reasons, within general talk about theories, we should act *as if* these theories were classes of statements; in other words: we should act *as if* *st.v.*₁, related to particular theories, were feasible (although it is not), and furthermore we should act *as if* the arguments against *st.v.*₂ were false and could therefore be neglected.”

Instead of discussing the virtues of the structural view on this general level, I shall restrict myself to mentioning some disadvantages and dangers connected with *st.v.*₃:

(1) As pointed out in § 1, we cannot give elaborated pictures of important physical theories within *st.v.*₁. In general talk about theories as classes of statements we pretend that we could. Being forced to give up this illusion in every particular case almost inevitably, i.e., out of psychological necessity, leads one to commit another fundamental mistake, namely, to obtain *a much too primitive picture of a physical theory*. The philosophical tendency to analogize physical theories and universal conditionals, like “swans are white”, has brought little advantage and has done much damage.

(2) The formal language approach suggests the idea of a single intended domain of the theory. Here again, it makes no difference whether we speak of a particular theory or talk about theories in general. As we can learn from experience, it seems to be rather difficult to untangle oneself from the notion of ‘one big application’ and to carry out *the transition to an open set I of intended applications*. It is easier to make a fresh start such that the open set *I* is part of our conceptual apparatus from the outset. Otherwise it might well happen that, ultimately, we could not resist the ‘cosmic-application’ temptation.

(3) An adequate reaction to what in [52], Part I, 1, I called Putnam’s challenge and what has been discussed again in the present text in § 3 is obstructed by thinking in terms of ‘the language of science’. The linguistic approach has led to the subdivision of scientific language into two levels: the observation language and the theoretical language. The logician’s method is thereby imitated. In much the same way as the logician subdivides, in a first step so to speak, the signs into the logical and the descriptive, the philosopher of science differentiates, in a second step, between those descriptive signs which are ‘observational terms’ and those which are ‘theoretical terms’. The elementary insight into the fact that the ‘theoreticity’ of a term has to depend *on the role this term is going to play within the theory* is thereby dismissed.

(4) In many cases, intuitions based on the statement view turn out to be *extremely misleading*. Although this may be one of the strongest arguments, I shall not pursue it here but postpone it to the next section. There the discussion of the epistemological status of Newton’s second law will serve two different purposes: first, it will give an illustration for what I called non-state-

ment view_{2,5}, and secondly, it will show why, in [52], I went too far in my criticism of Kuhn.

(5) ‘Growth of knowledge’ in philosophy often depends on making the right distinctions at the right place. Let us take for granted, for the sake of argument, that the distinction between *laws* and *constraints* is an important one. I think it impossible that one could discover this dichotomy within the statement view, since there is no difficulty in formulating constraints ‘as if they were laws’. Only thinking of mathematical structures, of possible models satisfying these structures and possible cross-connections between partially overlapping models leads one, in an unconstrained way, to distinguish between these two items.

(6) Similar remarks apply to the differentiation between *theories* or *theory-nets*, *empirical claims of theories* or *theory nets*, *acts of holding a theory* and *evolutions of pragmatically enriched nets* in the sense of § 5. The last two notions, in particular, seem to be naturally called for in the structural approach, while they could be introduced into the statement view₃ only with considerable artificialities and complications. As I emphasized in [53], it is these notions, or something very similar to them, which are intended by *scientists themselves* when they speak of theories. For they understand by theories in normal discourse *neither* mathematical structures *nor* abstract classes of sentences, but something filled with the ‘blood of reality’ and dealing with persons, their skills and beliefs, their expectations and hopes. All of this is built into the concept of holding a theory.

Suppose we select, by a random procedure, about a hundred physicists working in the Newtonian tradition and distributed over two centuries. If no particular whim of chance is involved, presumably no two of them will accept *exactly the same hypotheses* for their explanations and predictions. Some of them will work with forces and special force laws of which the other ones had never heard etc. Quite independently of the question whether “holding a theory” or “theory-evolution” are useful tools to redescribe Kuhn’s notion of a normal science in somewhat more precise terms, it seems to me the most natural thing in the world to say *that these hundred physicists held the same theory although no two of them entertained the same empirical hypotheses*. This single example illustrates another aspect of the non-statement view. In order to make it clearer let us, for the sake of simplicity, forget the *non-st.v.*₂. In other words, let us disregard the problem of theoretical terms such that we can neglect the Ramsey view. What, under this presupposition, is the difference between the present view and the traditional one, as far as empirical claims are concerned? *There is no difference at all.*

But, of course, there *is* a difference. It consists in the fact that we have, in addition to the empirical hypotheses, another entity available, namely the

theory or the basic theory element. *Every empirical claim must be relativized to a particular historical moment, while a theory element need not be.* If you identify a theory with the set of these claims, then all changes at the periphery of a science are automatically mirrored by a change of the theory. If you want to be entitled to speak of the theory remaining the same, despite the occurrence of such slight changes, you are forced to change the traditional way of talking. Otherwise, what you pronounce is a contradiction. If you keep remembering this then you will realize that there is no conflict between the structuralist view and the statement view at all. There is only the difference that the former is richer with respect to the wealth of expression and, therefore, can say things which the latter cannot.

I should mention in passing that we could, by analogy, introduce concepts like '*holding a (strict or approximative) reduction of one theory to another*'. In this way we could try to render precise the idea of working with a theory which is *progressive* with respect to the supplanted theory. Not only would such a concept be imperative for our historical understanding of certain phenomena; it would, besides, be needed for a reconstruction of certain ideas of Lakatos.

We could use a more 'objective' or more 'subjective' variant of such a concept. In the first case the fact that the reduction relation '*really obtains*' would have to be used, while in the second case only *the belief of the scientists in such a relation would matter*

(7) The new approach provides better tools than the statement view to analyze *scientific changes*. This claim holds, for reasons mentioned in § 3, even more for the apparatus of nets than for that of 'expanded cores' used in [47] and [52].

Besides, theory-nets can serve to give a precise illustration of Quine's distinction between central, peripheral, and intermediate parts of a theory. The general 'rule of correspondence' is this: the more central, the higher in the net. Accordingly, three kinds of '*theory changes*' can be distinguished. A '*big scientific revolution*' happens if the basic core on top of the net is changed. '*Mini-revolutions*' of various degrees, which Kuhn has taken into account in the post-script of his [20], take place if the basic core remains untouched but a theory-element core at a relatively high level of the net is replaced by another one. (If necessary, degrees of generality can be introduced.) Normally, laws of relatively high speciality remain unaffected by a scientific upheaval. In the 'language of nets' this may be expressed in the following way. Upper or middle parts of a net may change while the lower part remains unchanged. The latter is, of course, only a picturesque description. What is meant is, more precisely, that the lower parts of the new net are obtained by *equal* specializations of *other* more general theory-element-cores.

If one tries to account for the partly very drastic and partly very complicated changes which Feyerabend mentions in [13] on p. 361, then it will certain-

ly not be sufficient to concentrate on *one particular net*. Rather, we have to take the *hierarchical structure* of theories into consideration. But before doing that we must first get a better understanding of the second important inter-theoretical relation besides the reduction relation, i.e., the '*presupposition relation*' between theories.

(8) What has been said in (7) about scientific change holds analogously for all *pragmatic aspects*. In this respect I hardly have to add anything of importance to the remarks in § 5.

(9) For the sake of completeness only I recall the fact, dealt with at some length in § 6, that within the structuralist approach the notion of '*revolutionary progress*' can be clarified, the difficulties arising can be rendered precise⁷, and, in addition, there is some hope that the logical and epistemological problems connected with it will be solved. In contrast, it would be a hopeless undertaking to try to do the same within the statement view – either on the general level in statement view₃ or on the special level for particular theories in statement view₁.

Since the non-statement view₃ only mirrors, on the level of general philosophy of science, the position of the non-statement view₁ on the special level, I conclude this section with a repetition of an earlier remark on *non-st.v.₁*. As far as *non-st.v.₁* and *non-st.v.₃* are concerned, there is no such thing as an 'intrinsic superiority' of the structuralist view as compared with the statement view. Rather, the matter is a purely practical one, and in particular, a psychological one. All I have done was to give some reasons why, even on the level of 'intuitive' and 'semi-formal' reasoning about physical theories, the structural approach is much less endangered by errors and confusions than the statement view and, in addition, is much easier to handle than the latter.

Of course, this modest result holds only for *non-st.v.₃* as the counterpart of *non-st.v.₁*. By contrast, *non-st.v.₂* does not only hold for practical but for *logical* reasons and must therefore be accepted on the level of general philosophy of science as well as on the level of special philosophy of science.

7 This claim will get additional substantiation in § 11.

§8. Kuhn-Interpretation and Withdrawal of Objections Against Kuhn

On p. 359f. of [13], Feyerabend gives one of my arguments in favour of the structuralist view: “the statement view makes Kuhn appear irrational while Stegmüller’s procedure does not.” This could create the impression as if, in my opinion, there consists a ‘chain of justification’, beginning with Kuhn’s philosophy of science and ending with the non-statement view. Some of the criticisms which have meanwhile come to my attention seem to go one step further, identifying the non-statement view with an formalistic *ad-hockery*, devised only for the purpose of defending Kuhn’s philosophy of science. Undoubtedly, Feyerabend would never say something like this. But for the critics mentioned, his remark quoted is certainly grist to their mill. Actually, in my view, there *is* something like a chain of justification. *Alas, it seems to run in exactly the opposite direction!*

The real situation is somewhat more complicated and is best described by the following subdivision. I shall presuppose acceptance of the arguments in § 1–3 in favour of the structuralist approach which are of a purely systematic kind and have nothing at all to do with historical interpretation:

(1) The investigations of Suppes and Sneed on the one hand and of T.S. Kuhn on the other hand are totally independent of each other. Neither in the point of departure nor in the way of argumentation nor in the collection of supporting data is there any similarity between these two trends. Our discovery that they, all these differences notwithstanding, ‘*converge*’ in important respects gives mutual support to these two approaches. The connecting links making these convergences conspicuous are certain *pragmatic notions*, i.e., either the concept of *having (holding) a theory*, like in [47] and [52], or the concept of a *theory-evolution* as introduced in § 5. As ascertained, these concepts enable us to explicate the notion of ‘normal science’ in such a way that the impression of irrationality disappears. As we shall shortly see, it can be shown that, similarly, the revolutionary processes issuing in a *theory dislodgement* are normally not ‘irrational’ either.

At the beginning of the previous paragraph I emphasized the mutual independence of the Suppes-Sneed approach from the work of Kuhn. This claim

must now be qualified in two important respects. First, the independence holds only insofar as the approach in question makes use of informal axiomatizations and, as I called it, of 'informal model theory'. It no longer holds true as soon as the *pragmatic* and *dynamic* aspects, some of which are mentioned in the second half of the last paragraph, are taken into account. All such concepts, like 'scientific community', 'theory evolution', 'holding a theory', 'paradigmatic set I_0 of intended applications', 'paradigmatic theory-element core K_0 ', 'theory dislodgement' etc. reflect the work of Kuhn. Thus, e.g., all investigations of the kind to be found in § 6 would have been impossible without Kuhn's analyses. Similar remarks apply to the *formulations of problems* connected with these pragmatic and dynamic notions. A particularly drastic example is the problem of scientific progress. Earlier philosophers restricted themselves to giving what they believed to be true accounts or correct explanations of scientific progress. They devoted little or no attention to the question of how to define what they were talking about, presumably because they considered it as a more or less trivial matter. Only after Kuhn's illuminating descriptions of scientific revolutions has it come to light how extremely difficult it is to explicate "*progress*" in such cases where the basic theory-element is supplanted by a different one creating a new net whose elements are specializations of the new basis.

Another point follows from historical considerations. Even if an historian would decide to identify a theory with a set of sentences, he would have to interpret the term "sentence" differently than a logician. While for the logician a sentence is an abstract entity, the historian must take it as a concrete statement. Statements, being related to one particular moment, are ephemeral objects. But here a difficulty arises. It would be unreasonable to describe the permanent daily changes at the periphery of the body of science as continual transitions to new theories. In order to avoid this, one must use notions characterizing a theory as a temporally persisting entity. This urgent need of the historian to have permanent objects, in addition to things which are not more than candid photographs, provides a challenge for the systematic philosopher to analyze the basic structures of these persisting entities in precise terms. Thus, although the structuralist concepts developed independently as described, one might think of them as partly having emerged from the demand for systematic counterparts of historical notions.

In order to avoid misinterpretations of this first point, it should be emphasized that it leaves Kuhn's analyses and arguments untouched. They are neither 'improved' in some way nor are they 'supplemented'. Whatever they amount to, they are in need of independent testing.

(2) Insofar as Kuhn's results are accepted, the convergence observed under (1) may be used as additional supporting evidence for the structuralist approach. Accordingly, this could have been mentioned in § 7 as a supplementary point

(10). There is a simple psychological reason why we did not do so. The non-statement view addresses itself primarily to philosophers with a systematic interest. And among these there seem to be many more opponents to Kuhn than theoreticians agreeing with his analyses and descriptions. But no Kuhn-opponent would accept *such* an argument against *st.v.*₃ (or *st.v.*₁).

(3) The most important point has not been mentioned yet. First, I remind the reader of the logical structure of the anti-Kuhnian epistemological arguments. They are *a priori*-arguments in the following sense. No counter-examples based on careful empirical studies are given against Kuhn's main theses. Rather, the epistemological critics try to prove that his basic claims lead to positions, which, for general logical reasons, are untenable, like historical relativism, epistemological subjectivism, and even irrationalism¹. It is *this way of arguing* that is obstructed as soon as one accepts the explications of "holding a theory", "(progressive) theory-evolution", "paradigm", "theory dislodgement", "reduction", "progress branching" etc.². The talk of *a priori*-arguments is justified because what all the mentioned critics do is to point out *alleged* consequences of Kuhn's philosophy of science and to apply *modus tollens* to this and the *alleged* epistemological untenability of these consequences.

In [53] I tried, in addition to the corresponding passages in [52], to show in detail *that and why this type of 'epistemological Kuhn-falsification' is absolutely unfounded*. But that's all! Here again, we must add that Kuhn's own arguments remain untouched and that they can therefore be attacked, as Feyerabend stresses correctly, by independent arguments based on detailed empirical studies. The only effect my reflections on Kuhn have is to narrow down considerably the range of possible epistemological arguments against Kuhn.

On p. 357 of [13], Feyerabend reports my view on the 'absolute limit' of the immunity of a theory as formulated in [52]: If the fundamental law occurring in the basic core should fail in the paradigmatic subset I_0 of intended applications, then the theory *must be given up*, no matter whether an alternative is available or not. This was indeed my view. I thereby tacitly presupposed that we can always find out whether the fundamental law holds in I_0 or

1 While it is often claimed that Kuhn expounds a relativistic and subjectivistic position, he can be called an irrationalist only in the 'indirect' sense that he, in his analyses, allegedly *imputes* an irrational attitude to the scientists but, of course, not in the 'direct' sense that he *himself* holds a philosophical irrationalism. In the latter case he would renounce analyses and arguments and, therefore, could not have written the book [20].

2 My article [53] is almost exclusively concerned with criticism of this kind of epistemological *a priori*-argumentation against Kuhn. It goes without saying that this way of screening-off Kuhn's philosophy of science from certain kinds of epistemological attacks works only insofar as these and other concepts are accepted as 'natural' and 'adequate' equivalents of corresponding notions in Kuhn's writings.

not. But can we? Meanwhile, I have become skeptical about this point and it now seems to me that not even such a ‘boundary of immunity’ exists. My skepticism with respect to the existence of the required test procedure may perhaps be of a general interest because it makes implicit reference to the omitted argument of (4) in § 7.

For the sake of illustration, let us consider the fundamental law in the basic core of classical particle mechanics in the Newtonian formulation, i.e., Newton’s second law. The law is representable by a simple formula containing three functions. The formula expresses a universal sentence. This representation immediately leads to the question: “Is it an empirical law or an *a priori* truth?” Numerous answers have been given, from “It is an elementary analytic truth, namely a mere definition of force” at one end of the spectrum to “It is an empirically falsifiable hypothesis” at the other. I maintain all of these answers are wrong. We can say even more: this whole discussion concerning the epistemological status of the second law is just nonsense. The sterile dispute has its roots in the misleading way of stating this law *as an isolated and ‘self-contained’ universal sentence*.

We get a very different picture if we look at the matter from the structuralist point of view. For we must then ask another question, namely: “What particular claim, for which this law is responsible, must be contained in *all* empirical claims which can be formulated with the help of classical particle mechanics CPM?” Roughly speaking, the answer amounts to this:

For all intended applications of CPM, forming at any historical time t no clearly defined class but a largely open(!) set I_t , we can find two CPM-theoretical functions f (force) and m (mass) standing in a particular relation to the second derivative of a third, CPM-non-theoretical function s (position) such that the function f will in most(!) applications satisfy certain (!) special laws and both functions f and m will create cross-connections between certain(!) applications through certain(!) constraints.

This statement is undoubtedly not ‘analytic’ but it is, as I expressed it in [56], sufficiently empty to withstand any possible refutation. (And for this reason we shall come back to it in § 9 in order to illustrate Quine’s observation on the prevailing confusion between centrality and analyticity.) I have dovetailed exclamation marks in those places where constituent parts of the claim appear which resist possible refutation: we may fail to discover the correct boundary of I , even if such a boundary *exists*; we may fail to discover the two theoretical functions, although *there are* such functions; we may miss the *appropriate* applications in which the special laws hold, or which are cross-connected by constraints; or we may discover these applications but *misrepresent* the constraints or give *wrong* formulations of the special laws etc. Therefore, it can never happen that an empirical claim of classical particle mechanics ‘is empirically refuted because Newton’s second law foundered on experience’.

Suppose the second law is a paradigmatic case³. Then my above-mentioned conception of an ‘absolute boundary of theory-immunity’, as reported by Feyerabend, breaks down. *There is no such a limit*. On the whole, there are then three independent reasons why a physical theory is ‘partially immune against possible empirical refutation’. *First*, no finite number of unsuccessful attempts to properly refine a theory net proves that the net could not have been refined (it possibly could if suitable intuitions and theoretical skills on the part of the users of the net plus *luck* were available). *Secondly*, even the fundamental law is not falsifiable, not even in the paradigmatic subset I_0 because it is ‘a cluster law of sufficient degree of emptiness’. *Thirdly*, because of the openness of the set $I_t - I_0$ at any time t , the scientific community can, in view of recalcitrant failures of attempts to apply the theory to this set, come to the conclusion: “This is no application of the theory” instead of: “In this domain the theory failed.”

In § 4 I have already referred to the ‘rule of autodetermination’ of a theory and to its possible generalization of special laws, i.e., of those laws for which the ‘sophisticated view’ holds instead of the ‘naive view’. That the naive view is not the normal case is suggested by two observations: *first*, that physicists often refer to subtheories not in terms of their applications but in terms of their characteristic laws, like in the case of the ‘theory of Hook’s Law forces’; *second*, that great and perhaps unsurmountable difficulties arise in “precisely specifying the characteristic applications by conditions expressed in the non-theoretical vocabulary”⁴. I guess that the sophisticated view holds for almost all physical laws. If this assumption is correct, then in the present context this would mean that the rule of autodetermination holds here as well and that, therefore, empirical falsification is not even possible for those special items by means of which a theory-net can potentially be refined. The immunity of theories expands to an immunity of empirical laws. This could have been mentioned in the last paragraph as a *fourth* kind of immunity.

For the sake of simplicity I have distinguished up to now between two opposite cases: developments in the sense of normal science or theory-evolutions and radical revolutions consisting at least of replacements of the basic cores. It should be observed that the new formalism of nets makes it possible to account for ‘mini-revolutions’ as well. The force of such a change will depend on the relative ‘height of the level’ within the net at which a core is replaced. Terminologically, these small-scale revolutions have already been introduced in the theory-evolutions. In order to keep these two cases apart from each other it seems recommendable to build in the notion of paradigm, as is done below, only in the case of a theory-evolution proper.

3 In [56] I referred to a law connecting all T -theoretical and T -non-theoretical functions occurring as a *cluster law*, thereby dualizing H. Putnam’s well-known term “cluster concept.”

4 Sneed, [48], p. 126.

A logical interpretation of one aspect of the Kuhn-loss property and of possible progress branchings has already been given in § 6. Each of these two results exhibits an additional approximation to Kuhn's position which I had not yet foreseen in [52]. The first one contains, in addition, a logical clarification of *one* form of incommensurability leading to a *partial incomparability* of theories. This incommensurability derives its importance from the fact *that it is irremovable*. Whatever 'solution' one may contrive to the problems it creates, it can only belong to the domain of *practical decisions*.

If everything in § 6 and in the present section is taken together, then, it seems to me, all my criticisms of Kuhn in [52] reduce to one single important but not polemical remark: the notion of *scientific progress* may be *used* in history, sociology, and psychology of research but it cannot be *explicated* solely on the basis of such studies. Whoever wants to learn what "scientific progress" *means* wants to find out why science is called a *rational* enterprise; and in his attempt to *explicate* the meaning of "progress" he is referred back to systematic logical analyses. A partial explication of the simpler concept of progress in 'normal science' has been given in § 5. As far as the revolutionary case is concerned, I still entertain the *philosophical hypothesis* that an inter-theoretical relation of the kind of the reduction relation will either do the job or will at least constitute the core of the analysis.

While, with respect to this last point, my conviction has remained in principle unchanged, it has been liberalized in one respect and become more critical in another.

(1) In [52] I envisaged only the case of *strict* reduction. Today it seems to me that the notion of *approximative* reduction is more adequate or, at least, more important because it is applicable in many more cases (vid. § 10).

(2) That kind of 'incommensurability' which ought to be overcome by means of the concept of reduction has turned out much more obstinate and the problems connected with it much more difficult than I believed at the time of writing [52]. I will come back to this point in § 12.

There is still an open question which we postponed in § 5, namely, how the notion of a *paradigm* can be built into the concept of theory-evolution. In [52], I suggested considering the paradigm in the sense of Kuhn as consisting of two parts, a 'pragmatic-empirical' component and a 'formal-theoretical' one, so to speak. The former consists of the subset I_0 of *paradigmatic examples* of the whole set of intended applications I ; the latter consists of the *basic core* K_0 (using the new terminology). In the definition of "holding a theory", [52], D30, p. 194, it was assumed that both I_0 and K_0 had been introduced by the creators of the theory, as components which are retained by all later members of the scientific community. Moulines has shown in [32] how we may free ourselves from this too narrow assumption.

Suppose I_i is the set of intended applications of the theory-element T_i . We may take I_i as consisting of a class of partial potential models, each one containing ‘homogenous’ applications only, i.e., applications which are, from an intuitive point of view, ‘of the same kind’⁵. Now we proceed in the following way. The expression ‘paradigm’ is neither explicated in isolation nor is it introduced in connection with the concepts of ‘theory’ or ‘holding a theory’, as in [52]. Rather, it is introduced into the concept of theory-evolution. Roughly speaking, $\langle K_0, I_0 \rangle$ is called a *paradigm for the theory-evolution E* if K_0 is a theory-element-core, $I_0 \subseteq M_{pp}$, and for every theory-net N in E and for every theory-element T_i in N , $T_i = \langle K_i, I_i, SC_i, h_i \rangle$:

(1) K_i is a core-specialization of K_0 , and (2) every domain of intended applications occurring in the theory-evolution E can be subdivided in homogeneous subdomains, each one having its own paradigmatic subsets of examples, i.e., subsets of I_0 ; this fact is acknowledged by the scientific community SC_i ⁶.

Again, the specific ‘Kuhnian’ aspect is introduced neither in the concept of a theory nor in the concept of holding a theory, as was done in [52]. Rather, it is now made an ingredient of evolution. This can be done in a very simple way. Just call a theory-evolution E in the sense of § 5 a *Kuhnian theory-evolution* if there are K_0 and I_0 such that $\langle K_0, I_0 \rangle$ is a paradigm for E .

This procedure has various advantages over the earlier methods of explicating “paradigm”. First, the notion of a paradigm is introduced directly into the *dynamics* of normal science. Secondly, it is *not* postulated that $\langle K_0, I_0 \rangle$ is the historically first theory-element of the theory-evolution. (Actually, this ordered pair need not be a theory-element at all!) Thirdly, as far as I_0 is concerned, it is not any more required that *this whole set* is a paradigmatic subset for every domain of intended applications appearing in the course of the theory-evolution. It suffices that small subsets of I_0 are empirical paradigms for small subdomains of the kind described.

The concept of a paradigm, introduced in this way still does not capture the whole of Kuhn’s notion. As T.R. Girill has pointed out in his interesting review [16], Kuhn treats science not entirely in terms of ‘knowing-that’ but “is equally preoccupied with science as knowledge-how”. In [54], § 10, I have tried to make allowance for this fact. While K_0 and I_0 may be called paradigmatic *objects* because they are the products and objects of scientific activity, the treatment of science in terms of ‘knowing how’ asks for the underlying *paradigmatic dispositions* which manifest themselves in scientific acts and objects. These personal skills and social habits are implicitly referred to by mentioning the scientific community SC in the concept of theory-evolution. De-

5 Within the Newtonian theory of gravitation, free falling bodies would form one such subdomain, planets another one, etc.

6 For a precise definition vid. D 9 of [32]. I have sketched the details in [56].

tailed analyses of the abilities of the single members of *SC* as well as of *SC* as a whole had to be given in special disciplines, like psychology and sociology of research.

There is one respect in which historically oriented analyses and descriptions like those of Kuhn's will leave behind all such systematic investigations, even if the latter are supplemented in the way just hinted at. They will leave them behind, at least for the present and presumably still for quite some time, because the systematically oriented philosopher cannot master science in its full richness, as the historian can. The reason for this was alluded to at the end of § 4 and then again before the end of § 5. Our phrase "Kuhnian theory-evolution" therefore savours a little bit of a 'philosophy of the as-if'. We feign as if we already had a systematic equivalent of the corresponding historical portrayals. This is not the case. Although the structuralist approach, unlike *st.v.*₁, need not start from the bottom, but can rather begin to analyze physical theories at a relatively high level of generality and abstraction, it has to start with a relatively simple case, like every systematic procedure. Only at some future time can we hope to obtain a clearer systematic picture of scientific evolutions in which several layers of theories 'one lying over another' and being connected by the relation of theoretization and its converse, undergo simultaneous change.

In order to prevent misunderstandings, I should like to stress one point which I have emphasized already in the Helsinki-paper [56], namely *the absolute independence of the Suppes-Sneed-approach*, conceived as an analogue to the Bourbaki-programme, on the one hand, and *the reconstruction of 'Kuhnian notions'* on the other hand. You can *welcome* the structuralist approach for *systematic* reasons and nevertheless *dismiss* the Kuhn-interpretation, e.g., by rejecting some correspondences, like "normal science" and "theory evolution" as 'unnatural' or 'inadequate'. And, vice versa, you can *accept* the reconstruction of Kuhnian notions as clarifying and still *reject* the structuralist approach for systematic reasons. No 'structuralist' is morally obliged to become a 'Kuhnian' and no 'Kuhnian' is forced to do, or even to become *interested* in, that kind of work, which was begun by P. Suppes and continued by J.D. Sneed.

§9. Holism, Underdetermination of Theories and Research Programmes: Remarks on W. V. Quine and I. Lakatos

Almost every student who wants to become familiar with Quine's philosophical positions will soon run into trouble. Those theses of Quine's which belong to philosophy of science seem to be very general and very abstract. It is often not quite clear how they are to be established and sometimes not even what their precise content amounts to. Some readers of Quine's works resign themselves to the position that Quine's philosophy is basically *philosophy of logic and of language* and that his philosophy of science is of secondary importance.

Against this I would say, first, that such a view is incorrect since it is a peculiarity of Quine's outlook that his philosophy of language and his philosophy of science are inseparably linked together. Secondly, such a separation, even if possible in principle, would be of no use for those interested in Quine's philosophy of language. For one thing seems to be undeniable: certain views of Quine belonging to philosophy of language presuppose the correctness of assumptions which must be classed as philosophy of science.

A prominent example is Quine's *holism*. One only has to read Quine's brief contribution to the Schlipp-volume on Popper and Popper's reply in order to see that one must have acquired a particular position in the theory of confirmation or corroboration before one can feel competent to judge Quine's thesis. If, in addition, one studies Grünbaum's extensive discussion of the Duhem-thesis which is accepted by Quine, then one gets the definite impression that one would have to go into subtle questions of confirmation theory before one could take sides in the dispute about holism. On the other hand, holism has gained entry into various philosophical conceptions of Quine's, e.g., in the thesis of indeterminacy of translation and in the rejection of the analytic-synthetic dichotomy. Thus, the philosopher who wants to study and evaluate only Quine's philosophy of language runs into difficulties. His situation will be totally desperate if he, like me, comes to the conclusion that the theory of confirmation is presently in a very confused state. Must we perhaps suspend all

our judgements on Quine's philosophy until all the confusions in that particular area are removed?

Fortunately, there is another way open to us. As far as I know, all the pre-Sneedeian arguments in favour of holism are of a 'confirmatory' kind. Sneed's argument is not. Holism in the sense of Sneed is, as I see it, a trivial consequence of what I called *non-statement view*₂ in § 3, i.e., of the thesis that the empirical claim of a physical theory at a particular historical time is not to be represented by an infinite class of sentences but is just *one big single claim*, represented by an emended Ramsey-sentence $I \in A(N^*)$ (or $I \in A_e(E)$ in the original formalism). If this is correct then we cannot normally isolate single empirical partial claims from this *one* claim. True, the correctness depends on a philosophical question. But this problem is much more sharply defined than the problem of the status and of the correct results of confirmation theory. It amounts to the question of whether all important physical theories contain theoretical terms in the sense of Sneed.

Actually, the situation is slightly more complicated. Since within structuralism a distinction is made between theory nets and the empirical hypotheses associated with them, the thesis of holism gets, so to speak, a two-fold tinge. Only holism in the second sense came up for discussion in the last paragraphs. The holism in the first sense simply consists in the fact that the decision for a theory is an 'all or nothing' decision in the sense of a decision to work either with *this* core or with a different one. In all those cases where no real alternative is known, the talk about a 'decision' may sound misleading or at least bombastic. In such a case, the core could be called *a priori*, although not analytic (in a similar way in which H. Putnam ascribes an *a priori* status to Euclidian Geometry for the time before noneuclidean Geometry was discovered). This first variant of holism is based neither on an assumption about confirmation theory nor on a claim about theoreticity. Rather, it exemplifies an insight of Quine's which tells us that the so-called analyticity is often nothing more than misunderstood centrality. The decision for a particular core amounts to the decision for a central structural item, as illustrated in § 8 by the example of Newton's second law.

In a similar way, a new account can be given of Quine's thesis of the *underdetermination of theories*. According to it, theories may be empirically equivalent and still incompatible with each other. The concept of empirical equivalence is thereby defined in terms of the totality of all possible observational consequences.

I recall two peculiarities of Quine's conception. First, the concept of an *observation sentence* is not introduced, as it is in the Carnap approach, by recourse to an observation language, but rather with the help of stimulus-meaning. I consider Quine's procedure, which makes no use of an empiricist conception of language, an ingenious device whose importance seems not yet to have been grasped by philosophers of science. Secondly, the concept of a *possible* observation does not involve an excursion into possible worlds. It is

only assumed that to all space-time points of the *real* space-time continuum there are assigned true observation sentences.

As I have realized, readers of Quine often do not know what to say to this thesis. There is a whole spectrum of possible reactions, which range from calling it trivial (“no law is verifiable; every piece of empirical evidence is compatible with an indefinite totality of laws”) to finding it incomprehensible. Some, again, object that it is vague because the concept of empirical equivalence has not been rendered sufficiently precise. Usually an important aspect is hereby overlooked, e.g., the fact that reference is made to *all possible* observations. Be that as it may, we seem, this time again, to lose our way in any case in the jungle of confirmation theory when trying to evaluate Quine’s second thesis.

What the notion of underdeterminacy intends to express can be illustrated, it seems to me, by means of the concept of progress branching. If I am right, then it is an advantage of this illustration as well that it makes no reference to confirmation problems. It is even sufficient to take into consideration only one of the possible bifurcations, namely empirical branching, which manifests itself in the fact that, relative to a given core K , the set I of intended applications forks (such that the maximal compatible set of K is no singleton). In § 6 I have emphasized the role of value judgements and of rational decisions at the points of bifurcation. The same state of affairs could have been formulated in this way: the fact that the rationality of theoretical reasoning does not exhaust the concept of scientific rationality has its roots in the underdetermination of scientific theories, since this makes rational decisions necessary.

What distinguishes the present illustration from others is that it does not look at the matter from a static point of view but from a dynamic one. What we considered was not the relation between a momentary photograph of a theory and the totality of true observation sentences, but *the way theories can evolve through time*.

The present framework may, besides, be used to clarify several aspects of Lakatos’ ‘*methodology of research programmes*’. Actually, I have shown this in detail in [52], pp. 220–229. But, to my dismay, my account of Lakatos has been totally misinterpreted by virtually all of those critics who discussed this point. I therefore decided to repeat the essentials of my reconstruction in the Helsinki paper [56]. In the present context I shall confine myself to mentioning those respects in which the reconstruction of Lakatos deviates from that of Kuhn. The first step consists in removing a fundamental ambiguity in the writings of Lakatos¹. Expressing the same in more benevolent terms, one could say that Lakatos anticipated the distinction between theories and em-

1 For details, vid. [52], p. 221 ff., and [56], where I tried to defend my account against Tuomela’s criticism.

pirical claims of theories *in our sense*, but that he was not consistent in his terminology. Whenever he speaks of *theories as members of a sequence* with the sequence representing the research programme², his “theories” correspond to our “empirical-hypothetical claims”. Sometimes he speaks of Einstein’s theory or of Newton’s theory. In such contexts the word “theory” is apparently meant in the sense of “research programme”³. Keeping this in mind, we get the result that, apart from minor details, his concept of a *progressive research-programme* coincides with the concept of a *progressive theory-evolution* in the sense of § 5.

For some time I harboured the thought of using the phrase “to hold a research programme” instead of “to hold a theory”. Several misinterpretations would then not have arisen. But such a decision would have been historically unfair to T.S. Kuhn. After all, Lakatos himself had pointed out that his concept of a research programme was ‘reminiscent’ of Kuhn’s notion of normal science. I therefore decided to keep the neutral version “holding a theory” and to use it as a conceptual tool in the reconstruction of ‘normal science’ as well as of ‘research programme’.

A new concept of Lakatos’, having no analogue in Kuhn’s philosophy, is the notion of *sophisticated falsification*. As I have tried to show in detailed analyses in [52], pp. 224ff., this expression refers to an *intertheoretic relation* corresponding to the relation of reduction. There is the following distinction regarding what is usually called “falsification”. First, in the ‘sophisticated case’ no hypotheses are compared with empirical data. Secondly, not even empirical hypotheses are related to each other, but rather theories or theory-nets in our sense. (Wherever Lakatos uses the word “theory” in *this* connection, he ought to have spoken of research programmes in order to remain terminologically consistent.) In any case, he seems to have held the opinion that in the revolutionary case the concept of progress has to be based on an *intertheoretic relation*.

As far as the normative-methodological aspect in the representations of Lakatos is concerned, I expressed myself skeptically in [52], [53], and [54]. In [53], on p. 160 I suggested replacing the pretentious word “rule” within the phrase “methodological rule” by the more modest term “*recommendation*”. Today it seems to me that I could give a more *constructive* account even of this *normative* aspect of Lakatos’ view, namely in the sense of the last paragraph of § 5. Here, it is very important to realize that it was Lakatos’ intention to include the confirmation aspect while, up to now, this aspect has not yet been included into the Suppes-Sneed approach!

2 Vid., e.g. [23], p. 132.

3 This becomes obvious as soon as one tries to conceive of the whole theory of Einstein as a member of a human research programme, which is obviously impossible.

Suppose the analyzing philosopher has accepted as one of the *objects* of his studies a theory of support which differs from the theory implicitly contained in the confirmation and test behaviour of the scientific communities SC_i . In this case he must, of course, give up his epistemic neutrality and *critically evaluate* the behaviour of the SC_i 's. If, e.g., the members of SC_i should prove to be stubborn enumerative inductivists, while he himself has become an adherent of Popper's theory of corroboration, then it may very well happen that a progressive theory-evolution in the sense of § 5, *from the standpoint of this evaluating philosopher*, will turn out to be an *illusion* of progress consisting, in truth, of growing dogmatization and fossilization.

A final clarification will have to wait until the fog presently permeating the theories of confirmation and of corroboration clears up. I take this opportunity for a brief comment on Feyerabend's remark in [13], middle of p. 361, where he points out that the 'protective belt' of Lakatos can suitably explain certain kinds of relative immunities of 'paradigm'. From a logical point of view, the situation is like this: Lakatos calls our attention to something very important, namely to what Putnam once called the 'underestimation of the role of auxiliary hypotheses by philosophers'. Considerations of this kind belong to the contexts of 'theories of support' and 'theories of test'. In those contexts one has to deal exclusively with empirical hypotheses, but not with theories and theory-nets in our sense. There is no reason for me to deny that new 'aspects of immunity' will arise within a satisfactory theory of support. But if this should be the case, I would not take it, as Feyerabend seems to, as a symptom of the superiority of the statement view over the structuralist approach, but just as reflecting the simple fact that *test* and *support* have to do with empirical claims and not with theory-elements or with nets of such elements.

§ 10. Some Additional New Results

Quite a number of new results since the publications of [47] and [52] have already been reported on in the present paper. Let us briefly recapitulate. Perhaps the most important improvement consisted in replacing expanded cores by the concept of *theory nets* (§ 4). A systematic account of theory nets and the associated core nets, of the empirical claims belonging to them and of the various *types of reductions* is given in the article of Balzer-Sneed, [4], where the reader will find proofs of numerous theorems which Feyerabend missed in my book.

The way to pragmatization is paved by the study of various kinds of *theory-evolutions* (§ 5) and by the generalization of the concept of a *paradigm* (§ 8). The non-teleological character of scientific progress in normal as well as in revolutionary science is brought out by the notion of *progress branching* and by distinguishing between *different types of rationality in science* (§ 6). An important recent contribution by Sneed to the concept of *invariance* and thereby, implicitly, to '*empirical incommensurability*' will be mentioned in § 11.

On the level of special philosophy of science the dissertations of W. Balzer [3] and C.-U. Moulines [29] are to be mentioned. An abbreviated version of the second one is to be found in [30]. A characteristic feature of these works is, among others, the *generalization* of the concept of *constraint*.

In [3] Balzer reconstructed the *classical theories of space and time* as Sneedian-type theories, underlying classical mechanics. He described in precise terms a *presupposition relation* holding between classical mechanics and the theories of space and time; or, what amounts to the same, he described in what way kinematical systems may be understood as '*theoretizations*' of space-time structures¹. The distance function is introduced as a *theoretical* quantity satisfying Sneed's criterion of theoreticity. A boundary line is drawn between

1 Roughly speaking, a theoretization is brought about by adducing new theoretical components. Thus, potential models are theoretizations of partial potential models. These may be called '*inner*' theoretizations, as distinguished from '*outer*' theoretizations, holding between theory-elements. Only the latter represent *intertheoretic* relations.

finite and infinite models. All entities requiring infinite models are built into the concept of *constraint*. Two achievements of this work are of particular importance. First, it is explained how the meaning of kinematical terms, like position, rests on the underlying concepts of space and time, like point, straight line, congruence. This allows a deeper investigation of cases of *meaning change* involving kinematical concepts. Secondly, on the nontheoretical level we come across, for the first time, particular ‘things of our real world’ and here it is made clear *exactly what is involved in the claim* that geometry and theories of space and time, regarded as empirical physical theories, *are true of these things*.

The two works of Moulines contain an elegant reconstruction of *classical equilibrium thermodynamics* within the Suppes-Sneed-framework. I shall stress only two peculiarities of these works. First it is shown that the fundamental principles of thermodynamics are most naturally reconstructed not as laws but as constraints. Secondly, in order to be at all able to give an account of classical equilibrium thermodynamics in precise terms, one must first build up a *proto-physics* of thermodynamics describing the most general properties of concepts, like system, state of a system, transition of states, etc.². This result suggests that other physical theories may as well be based on protophysical principles.

The expression “protophysics” has, in addition, a more general use according to which the topics it denotes belong to the domain of general philosophy of science. In order to do justice to radical changes in physics one must presumably take into account, besides scientific revolutions *as describable within the framework of Sneed*, i.e., replacements of basic cores, also such changes which involve upheavals of general and special protophysics. Thus, the rise of quantum physics is revealed as a part of a protophysical revolution in the course of which principles tacitly presupposed for such notions as *system*, *state*, *law*, and even *being identical with* have been replaced by new ones.

Some of the recent work done on the level of general philosophy of science, but applicable in special domains as well, deals with *problems of approximation*. This work has been carried out in two different directions. Many stimulations for all of them come from the analyses done by G. Ludwig in [26].

The article of Moulines [31] concerns *approximative applications* of theories. The main tool used is the concept of ‘model-theoretic fuzzy sets’.

In the two papers of D. Mayr, [27], I and II, strict reduction is liberalized to *approximative reduction*. Although the technical apparatus needed is rather complicated, I shall try to outline the intuitive background with an example. According to the basic idea, it is the *reducing* theory ‘which does the approximation’. Let there be given two theories T and T' such that T is to be approximatively reduced to T' . T may be Kepler’s theory and T' Newton’s theory of gravitation. The reduction relation is rendered precise in terms of models and

2 I remind the reader that the expression “protophysics” is used in the sense of M. Bunge.

sequences of models. A given model of T is approximated by an infinite sequence of ‘corresponding’ models of T' . The correspondence relation is not only one-many but one-infinite.

One technical difficulty arises from the fact that the sequence mentioned must *converge*. The elementary concept of convergence is, of course, inapplicable because it refers to numbers only. In modern topology a more general concept of convergence is known, the *convergence of filters*. If we represent the set of potential models by a Cartesian product of sets X_i , we can reconstruct inaccuracy-sets, corresponding to the inaccuracy of measurement, as filters on the X_i 's. In order to get unique limits, separability is required too. A second difficulty consists in the fact that convergence requires *completion* of the given topological space. Therefore, *uniform spaces* are needed, since a metric would be too special. In this way, the ‘limit elements’ of the approximating sequences can be added. (The elements of the completed uniform space are equivalence classes of Cauchy filters).

Suppose that the technical manoeuvres hinted at are successful. Then the limits added to the sequences of models of the reducing theory T' are isomorphic to those models of the reduced theory T which were approximated by the sequences. The approximative reduction of T to T' is then identified with the strict reduction of T to the completion of T' .

The result is of general epistemological interest, as may be illustrated by the Kepler-Newton-example. The sequence of the approximating models of Newton's theory is such that the mass of the ‘sun-particle’ tends to Kepler's constant while the masses of the planets tend to zero.

It becomes immediately clear that the limit of the sequence can no longer be a possible model of Newton's theory. (In all these possible models the mass function is strictly positive.) We can formulate the epistemological upshot as follows: *The approximatively reduced theory turns out to be wrong if viewed from the standpoint of the reducing theory*. Bearing in mind that the approximative reduction is presumably the most important type of ‘explanation of one theory by another’, we have hereby substantiated a claim expressed in § 7, namely that there is a fundamental difference between explanations of facts and explanations of theories. For, explained facts are, of course, described by true sentences only.

§11. Incommensurabilities

H. Putnam emphasizes in [38], p. 281, that in his view radically different theories can be compared. And he continues:

(1) “We do not have to agree with Sir Karl Popper or with Paul Feyerabend that there is an incompatibility between accepting the existence of radical paradigm change in science and accepting the idea of a growth of objective knowledge (of course, Popper and Feyerabend, differ on whether to reject radical paradigm change or growth of objective knowledge). We can have our paradigm shifts *and* our objective knowledge too.”

It is not my intention to investigate the historical question whether the alternative-presupposition imputed to both Popper and Feyerabend by Putnam is correct. Suffice it to say that, in my opinion, Putnam has expressed, in the first of the two sentences quoted, in a concise and excellent way the feeling most philosophers seem to have with respect to the phenomenon of scientific revolutions. They seem to think that either these events are not radical paradigm shifts or, if they are, our notion of increase in objective knowledge down. And I fully agree with Putnam’s claim as pronounced in the second sentence of (1).

However I do not think that the solutions to the problem are as simple as Putnam seems to believe. When Putnam tries to reduce the whole question to ‘preserving reference across theory change’, he apparently has in mind the traditional (e.g., Carnapian) view of theoretical terms, according to which these terms belong to the non-observational or theoretical part of the ‘*language* of science’. This traditional view rests on the assumption that we are confronted with *only one* dichotomy: observational – theoretical.

It is my conviction that we have to distinguish between *two totally different dichotomies*. One is the *epistemological* dichotomy *observational – non-observational* (with “observational” to be characterized positively); the other one is the ‘*quasi-semantic*’ dichotomy *theoretical – non-theoretical*, as described in §§ 3 and 4 (with “theoretical” (1) to be characterized positively (!) and (2) to be relativized to a particular theory *T*).

The main difficulty in defining “progress” for the ‘revolutionary case’ consists in the fact that the theoretical superstructure of the supplanting theory T_2 is different from the theoretical superstructure of the dislodged theory T_1 . As T_1 -theoreticity means something different from T_2 -theoreticity, no manoeuvre of ‘ontological transtheoreticity’ will do!

Wherever I used the word “incommensurability” in [52], and I used it in very few places, I referred to the following difficulty and to nothing more:

How can one say that theory T_2 is progressive relative to T_1 if the two theories are incommensurable?

Feyerabend is highly critical of my comments on this topic. He says:

(2) "There is only one place where Stegmüller's virtues seem to be almost entirely absent and this is in his discussion of incommensurability" [13] (p. 363).

I frankly admit: Feyerabend is right. At least he is right if the relevant passages in [52] are read with the implicit demand on the text that it ought to give a satisfactory account of *all* aspects of this complex topic. Nowadays, if I just hear the word "incommensurability", there rises in my mind's eye the temple of the ten thousand Buddhas (or, for that matter, Professor Sidney Hook's temple of the ten thousand Marxes). And I have no ambition at all to bring 'law and order' into that entity full of nooks and crannies¹.

Feyerabend proceeds historically in his remarks. He distinguishes between Kuhn's account of incommensurability (inc_K) and incommensurability as looked at by him (inc_F) and in both cases he makes additional differentiations. All readers of my book who expect that I would give an account of all these theses and would answer the ensuing questions will undoubtedly become totally disappointed because they will find much too little about it. In what follows I shall try to do three things. First, I shall explain the reason for my one-sidedness. Secondly, I shall make a few remarks on some, but not most, of the themes raised by Feyerabend. Thirdly, and perhaps most important, I will describe why even my view on the very restricted special problem, as expressed in [52], is in need of revision.

As to the first item, the quotation (1) from Putnam can be taken as a starting point. Since publication of the relevant works of Kuhn and Feyerabend the question has been raised again and again whether, *as opposed to the views of Kuhn and Feyerabend*, there is 'real progress' in science.

All considerations and discussions following this line of thought seem to me *entirely worthless* for at least three reasons. First, they impute to Feyerabend and Kuhn a view which presumably neither of them ever shared but which results only by interpreting their positions from the angle of a particular cliché of rationality. Secondly, all such debates tacitly presuppose that all of us know, or at least that it is very easy learning to know, what "scientific progress"

1 There is a somewhat similar situation, although on a much smaller scale, with respect to the notion of a paradigm. Feyerabend seems to assume that I wanted to give an exact account of Wittgenstein's uses of "paradigm" (vid., e.g., footnote 4 on p. 356). But a Wittgenstein-exegesis was not my intention. Heaven forbid! I only tried to push the analysis forward to the point where one can see the connection with Kuhn's use of this word. As it happens, for the careful but unprepared reader there is, *prima facie*, no connection at all. For Wittgenstein would *never* have used phrases like the following. "Persons X and Y share the same paradigm".

means, in particular what it means in cases of ‘radical paradigm change’ or, as I call it, in cases of theory dislodgement. Thirdly, even if we take for granted that we have at our disposal a satisfactory explication of “progress in case of theory dislodgement”, we would have to check every case of *alleged* progress in history to see whether ‘real’ progress was obtained. And this may, in many interesting cases, turn out to be an extremely difficult question to answer. It will certainly *never* be a purely *philosophical* question. Rather, it is a peculiarity of this kind of difficulty that various fields of research will have to be combined in order to overcome it. The main weight will almost never lie on the philosophical aspects but on historical subtleties and/or mathematico-physical technicalities. And as this holds for every particular ‘progressive event’, it holds *a fortiori* for the history of natural science, insofar as this history embraces numerous cases of alleged progress.

Overwhelmed by the load of these and similar questions, I decided to concentrate on what still seems to me the decisive philosophical problem, namely, contributing to the clarification of what I just called, under point two, the tacit presupposition of all such discourses: the notion of scientific progress in those cases where one theory supplants another. For if we cannot succeed in explicating this notion we would have to do no less than *capitulate to those who deny the rationality of the enterprise called science*. It seems to me that with this question the problem of the rationality of science is localized, rather than with surface-problems of the kind whether at a particular time (or several times, or often, or always) ‘there was real progress or not’. And it was *this* question which I used in the last section of [52] as a challenge (actually, the only challenge) to Feyerabend.

Thus, I still believe that it is one of the most important and presumably one of the most difficult tasks, perhaps *the* most difficult task, for the philosopher of science *to explicate the concept of revolutionary progress*.

It should be remembered that all remarks on progress made in the last few paragraphs are to be read with the *qualifications and provisos* of § 6 and § 7. To the necessary *qualifications* there belong, in particular, all our observations of *possible* progress branchings, of partial incomparabilities, of the *importance of mere approximations*, of the *primacy of practical versus theoretical reason* at certain decision nodes, of the ‘*evolutionary tree of progress*’ etc. The most important *proviso* comes from § 7. Do not expect too much from *general* philosophy of science! At the general level we shall, I guess, never succeed in giving more than a ‘formal framework’ of concepts of strict or approximative reduction which, only after being *specialized to particular* pairs of theories in *particular* historical situations, will get enough ‘throbbing blood in its veins’ to do the real job it was intended for.

Furthermore, I remember that I referred to my claim that ‘revolutionary progress’ has to be explicated in terms of strict and approximative reduction

as a daring philosophical hypothesis. *I may be wrong*. Future investigations may prove that there are *other intertheoretical relations*, still more involved ones or perhaps whole ‘nets’ of such relations, that would do the job. Such possible alternatives would have the following three features in common with my present hypothesis: (1) explicating scientific progress is *no trivial task*; (2) it is primarily a *logical task* and not something to be accomplished by psychological, sociological, and historical tools; (3) the explication would have to be a *precise* one. The predicate “precise” is, again, to be understood in the Bourbaki sense, not in the Carnap sense; but this goes without saying.

But let me be absolutely clear about this one point. I would only accept a successful explication *in terms of different intertheoretical relations* as a refutation of my philosophical hypothesis! For, otherwise, I would admit that I had been wrong with respect to a *fundamental presupposition* to this hypothesis, namely in the belief that ‘there exists something like objective progress’. True, from a logical point of view there would still be two alternatives open to us: *either* to deny the reasonableness of progress *or* to explain this concept in terms of nearness to truth. At this point I can only confess. I confess that I would rather throw in the sponge and deny the very meaning of scientific progress than look for salvation in a system of teleological metaphysics.

Herewith I conclude my comments on the first points. In evaluating them, the reader should not forget how unpretentious they are. Neither have I tried to defend my position against objections nor have I tried to answer further questions. I have just *explained* my one-sidedness.

I have not too much to say to the second point because many of the incommensurabilities that interest Feyerabend by far exceed the competence of a logician. It seems to me that a new dimension of problems could be opened up if one would systematically include, in addition to the knowing-that aspect, the aspect of knowing-how. I have already dwelled on this question in connection with the notion of a paradigm in § 8. To the present context of incommensurability questions I would reckon the problem of incompatible physical intuitions; ‘incompatible’ not only in the weaker sense that one and the same person cannot *simultaneously* have two of them, but in the stronger sense that a person is normally unable to give up one in favour of the other. There may be a connection with age: people may be able to change their physical intuitions in their youth but not in later periods of their life². This would perhaps give a partial explanation of the fact that *theories die out*. On the whole, a

2 This has perhaps parallels even in logic and mathematics. I remember that once a famous mathematician told me he would have to change his profession if one day constructivism or intuitionism would become generally accepted. And he continued somehow like this: “in my youth, I could perhaps have adjusted my mind to the constraints on admissible proofs introduced by these schools. But today these restrictions would totally suppress my inspirations and destroy my intuitions.”

wide field of possible psychological and sociological research opens up before us.

There is one remark in § 6, p. 366, on incommensurability which indicates that Feyerabend may have overlooked a passage in my [52]. He mentions that the thesis of the theory dependence of observations (or theory dependence of observation sentences) “has no implications for the issue between the statement view and the structural account.” This is certainly true. But the reader is likely to get the impression that I had not dealt with this problem. Actually, I have been dealing with it, although very briefly and, perhaps somewhat unhappily, within the context of holism. Under the heading “amplification of the holistic thesis by Kuhn and Feyerabend” in (III), p. 238 and 238f., I dealt with the thesis of *theory ladenness of observations*, and in (IV), p. 239f. with the problem of *meaning change of theoretical terms*. As far as the latter is concerned, it is shown that under precisely describable circumstances there may occur a change of the truth conditions of the empirical hypotheses on the values of theoretical functions. This is a substantiation of the claim that theory change can bring about ‘meaning changes’ even *in a purely extensional sense*.

In the present context, (III) is of greater interest. After all, it is this aspect with respect to which Quine takes a point of view strongly opposing that of Polányi, Kuhn, and R. Hanson, calling it “epistemological nihilism”³. (Quine does not mention Feyerabend; but I assume that he would include him in the list of authors to whom his reproach applies.) In order to clarify this point, I distinguished two types of cases. According to the *weaker* thesis of the theory ladenness of observations, what counts as a fact for one theory is always to be determined by other theories. This creates no special epistemological problem if one agrees that theories are to be introduced in a particular order mirroring the presupposition relation between them.

With slogans like “theories determine their own facts”, a much *stronger* thesis of the theory ladenness of observations is to be expressed. It is this thesis which is felt as paradoxical and which presumably induced Quine to reproach nihilism. The situation referred to by it, if it exists at all, creates a difficult problem which seems to lead either into a maze or to the radical variant of subjectivism, telling us that ‘theoreticians mould the facts to fit the theory.’ My suggestion, animated by a reflection of Sneed on p. 93 of [47], consisted in reformulating the paradox in approximately the following way⁴: “How is it possible that facts, serving to support a theory, have to be described in terms of concepts which one can understand only if the theory is presupposed as valid?” It is not difficult to see that this amounts to nothing but a veiled way of formulating the problem of theoretical terms. We know the solution to this

3 [40], p. 87.

4 The present formulation is an improvement of that given in [52].

problem. An additional confusion is sometimes produced by erroneously identifying the Observational with the Nontheoretical and the Nonobservational with the Theoretical. I shall make a few remarks on this at the end of this section.

I now come to the third point. It has to do with a difficult problem which became recognizable only after publication of [52] and which shows one aspect of the theme “incommensurability” in a new light. As mentioned earlier, in [52] I concentrated on the problem of how to explicate the concept of progress for the case of theory dislodgement. Somewhat more precisely, the problem was the following. Let us say that two theories claiming to explain the same empirical phenomena are theoretically incommensurable, briefly: *inc_t*, if the theoretical superstructures of the two theories are different. If it was T_2 which dislodged T_1 , then we speak of *progress* only if T_1 is reducible to T_2 , but not *vice versa*. A concept of reduction serving this purpose must be applicable even in those cases where the theory dislodgement represents ‘radical paradigm change’ in the sense that $T_1 \text{ inc}_t T_2$. I set up the *philosophical hypothesis* that a simplified variant of the Adams-Sneed-reduction served this purpose. There-with the concept of reduction became the cornerstone to explain scientific rationality in such a way as to become compatible with a denial of what Putnam, in the quotation given at the beginning of this section, called the common assumption of Popper and Feyerabend.

Unfortunately, all the considerations following this line of thought were based on *too naive a view of physical systems*. At first, this may seem surprising. For the ‘physical systems’ or elements of M_{pp} were not introduced just as individuals of a certain sort, but in a more sophisticated way as *individuals cum non-theoretical functions*. At the meeting in London/Ontario, Professor Kuhn pointed to the fact that in cases of radical paradigm change *even the M_{pp} ’s of the two theories will turn out to be different*. At that time I conjectured that in such a case we would have ‘to step down deeper into the hierarchy of theories’ in order to re-establish comparability. As a matter of fact, it is one of the great advantages of the Suppes-Sneed approach, in distinction to the formal language approach, that it allows us to render precise the structure of theories at a relatively high level of abstraction, without doing the same for all ‘underlying’ theories. But, of course, these theories *can* be included in the theoretical research programme as soon as this becomes necessary.

In the meantime Sneed has carried out interesting investigations, including an attempt to show that the reduction relation obtains between a subpart of classical particle mechanics with the core K_{CPM} and a corresponding subpart of relativistic particle mechanics with the core K_{RPM} . These results have contributed to our better understanding of the problem and they admit assumptions about analogous cases.

First, I must explain what I meant with my remark on the ‘naive view’ of physical systems and the necessity of giving it up. In the formulations of clas-

sical particle mechanics in MSS, as used before by Sneed, the elements of M_{pp} are systems of moving particles as described by the position function. If one systematically tries to take the underlying geometry into consideration, one gets an indeterminacy with an imbedding into the vector space. This indeterminacy is mirrored in the set of admissible transformations. Now we encounter a new fundamental difficulty.

In *CPM* the laws to be formulated are Galilei-invariant. In *RPM* they are Lorentz-invariant. This has rigorous consequences for the formal reconstruction. Let us call the equivalence classes resulting in the first case E_G , and those resulting in second case E_L . The two theories to be compared no longer concern *the same* 'empirical systems'. In the case of classical particle mechanics the set M_{pp} has to be replaced by the *quotient set* M_{pp}/E_G . And in the case of relativistic mechanics it has to be replaced by the *quotient set* M_{pp}/E_L . *These two quotient sets are different.*

Prima facie, this seems to be an intermediate result which is devastating for the idea of reduction. For, the two theories to be compared are no longer theories 'about the same'. True, the physical systems forming the starting point *are* the same. But the two theories do not deal with these systems. Rather, they deal with more abstract objects. In the first case these abstract entities the theory is about, or, in other words, the new partial potential models of the theory are *the equivalence classes generated by the group of Galilei-transformations*. In the second case the new partial potential models consist of the *equivalence classes generated by the group of Lorentz-transformations*. Any arbitrarily selected 'empirical system' s belongs to an equivalence class of the first kind as well as to a class of the second kind. But this is of no help at all because s is *the only* element common to both of these classes. There is no 'natural correspondence' between the elements of the two systems of equivalence classes.

Here we have come across a new, plausible and precise, interpretation of "incommensurability". In order to distinguish it from inc_t , we call it inc_e (for "empirical incommensurability"). It applies, as in the case described, if the M_{pp} 's of the first theory are different from the M_{pp}^* 's of the second theory such that the presupposition for applying the reduction relation is not satisfied. At the same time we have obtained an exact version of the above mentioned objection of Kuhn's.

The intermediate result describes an unalterable fact. Considered as *dynamic theories*, Newtonian mechanics *NM* is not reducible to relativistic mechanics *RM* because of the relation $NM\ inc_e\ RM$ ⁵.

A solution of the problem can be found by going back to the *underlying physical geometries*. It turns out that *they* are not incommensurable but com-

5 The sketch of the argument referred to small subtheories of *NM* and *RM*. The *negative* result transfers by a simple '*a fortiori*-argument' to these two more comprehensive theories.

parable. More exactly, they are two competing, *incompatible* geometries, the one of which may be taken as empirically refuted.

I shall give some brief hints about Sneed's reflections on physical geometry. A geometric theory is modelled according to the pattern of a physical theory. For the first time, the M_{pp} 's are 'ordinary things', namely solid bodies. They are described only with respect to their *topological* structures by means of a part-whole-theory or *mereology*. (The term "mereology" was first introduced by S. Lesniewski. Systems of similar kinds later became known under the name "calculi of individuals". In the present context the reader can disregard the question of whether the ontology of such systems must be in accordance with the principles of nominalism or not). For measuring objects, a subset of these 'topological objects' is chosen. The choice starts from a 'paradigmatic primal set' of such objects and is made consonant with pragmatic aspects. By adding a function d the M_p 's are constructed from the M_{pp} 's. The following two additional requirements give us the elements of M : first, d must be a *distance function*, satisfying the well-known distance axioms; secondly, the resulting *metric* has to *represent the topological structure*. The reader will already have guessed, correctly, that within this framework d is introduced as a *theoretical quantity*. Of the distances on the measuring objects chosen it is required that they are *the same* in every model in which they appear. This requirement is expressed by the *constraint C*. (More exactly: the stipulation of rigidity is identified with the $\langle \approx, = \rangle$ - constraint; for a precise definition of this vid. [52], p. 75 and 83). The measuring objects can therefore be considered as the carriers of the metric function, briefly: as *metric carriers*.

The well-known difficulties and circularities in the discussion about the nature of the rigid body have their source in the fact that within physical geometry distance is a theoretical quantity, "theoretical", of course, to be understood in the sense of Sneed. This had already been shown in the work of Balzer, [3]. The intuitive proof of the theoreticity of the distance function runs like this⁶: Suppose the distance between two points is measured by a rigid rod and found out to be 1. Now what happens if the rod is not 'really straight' any longer because it has in the meantime been bent? In such a case the rod would have become 'too short' for measurement. The measurement of the distance as the 'shortest' or 'most straight' interval between two points *presupposes* that the privileged rod is straight; in other words: it is *presupposed* that it is part of a Euclidean straight line. This amounts to nothing less than to claim that the measuring rod together with its endpoint already *is* a model of the present theory. *In order to determine the distance, another application of the theory must have been successful, namely that application in which the straightness of the rod is ascertained.*

6 For a more detailed discussion, vid. Balzer, [3], pp. 100ff.

The remarks in the last two paragraphs are not only vague. In one essential respect they contain, moreover, an oversimplification. It is true that somewhere we must ‘get down to earth’ and that it is the business of mereology to start with ‘real’ objects studying their part-whole relations. But from there we cannot reach physical geometry by ‘one big jump’. Rather, this theory is the top of a three-level hierarchy consisting of the following three layers:

- (1) mereology
- (2) topology
- (3) physical geometry.

The physical objects appear in (1) as well as in (2). But while in (1) they are treated as ordinary objects, within (2) they are reconstructed as abstract entities, i.e., as point sets. Thus, if we imagine our reconstruction of the hierarchical structure, ending with *CPM*, to be made in an opposite direction, working our way stepwise down to earth, then we do not encounter real mundane objects until we reach the level of mereology.

One could perhaps say that the interesting *philosophical* questions arise in the first transition step from (1) to (2), while the problems generated by the second step from (2) to (3) are mainly of a more *technical* and *mathematical* nature. The most interesting, and presumably the most difficult, philosophical question is how to introduce the relation of congruence on the meagre basis provided by mereology.

In principle the technical problems can be considered solved. I shall try to explain briefly what things look like within Sneed’s framework⁷. The *partial potential models*, i.e., the M_{pp} ’s, are entities $\langle N, t \rangle$ where each $\langle N, t \rangle$ is a topological Hausdorff space. The *potential models*, i.e., the M_p ’s, are entities $\langle N, t, \mathfrak{U}, G \rangle$, where $\langle N, t \rangle$ is a before, $\langle N, \mathfrak{U} \rangle$ is a differentiable 4-manifold and G is a subset of the Cartesian product $N \times TN \times TN \times \mathbb{R}$, TN being the tangent space of N . The *models*, i.e., the M ’s, are quadruples $\langle N, t, \mathfrak{U}, G \rangle$ as before but with two additional properties. For each $q \in N$, G_q is a certain symmetric and positive (0,2)-tensor; and for all subsets A of N and all C^∞ – vector fields X and Y on A the function f is C^∞ , whereby f assigns to each $q \in A$ the value $G_q(X_q, Y_q)$. In other words: models are Riemannian manifolds. In each model, G defines a metric on N and this metric induces a topology which can be proved to be identical with the topology of the subsets of N open with respect to \mathfrak{U} .

It may be of some use to recall Reichenbach’s considerations once again. From difficulties of this kind Reichenbach drew the conclusion that statements on metrics are not assertions but mere conventions. This result was for him somehow unavoidable because (1) the notion of theoretical concepts was foreign to him, and (2) empirical statements must be testable on an evidential basis without getting involved in circularity. It is perhaps fair to say that Reichenbach was hot on the scent of theoreticity but finally came, from true observations, to a wrong conclusion in this very novel field by using a tacit premise

⁷ For the following remarks in this paragraph some familiarity with the theory of differentiable manifolds is presupposed.

of the following kind: 'what cannot be tested by observations must be a convention'. One literally 'sees' the efficiency of the 'empiricist dogma', appropriately criticized by Quine, according to which there exists a sharp boundary line between conventions and facts. In contexts like these, one could add, *conventionality is misunderstood theoreticity*.

For a subtle analysis, distinguishing between surface circularity and depth circularity, vid. the second part of § 3.

The attitude of Reichenbach which has been criticized must not be confused with 'Poincaré's thesis of conventionality of metrics'. In a certain sense, Sneed's result can be interpreted as making that thesis precise. Roughly speaking, it says: *By a suitable choice of metric carriers we can, up to a certain limit, obtain arbitrary metrics*.

Almost all philosophical discussions of space and time work with the (explicit or tacit) presupposition that only 'solid bodies' can be taken as the instruments by which length is measured. But why? Not even the restriction to inorganic objects need to be accepted!

Imagine a so-called 'primitive' culture satisfying the following three conditions: (1) the wheel has not yet been invented, (2) by misfortune, no slaves are available, (3) relatively large areas have to be measured. People still *can* use rods of iron or of copper for that purpose. But this is rather inconvenient for them. Suppose a scientific genius has the idea *to use cows as metric carriers* (Let us assume that these animals have in front and in back prominent bones suitable for markings). The most convenient property of those measuring rods is that *they move themselves* and, in the overwhelming majority of cases, even *in the direction one wants them to be moving*.

Certainly, this cattle measurement creates some problems for its user. In any case, despite being well-sheltered, the animals die at some time, wherefore it is reasonable to have a sufficient number of copies, as perfect as possible, of the 'standard cow'. More irksome properties are the growing and sometimes shrinking. That people of this culture are plagued by such questions only demonstrates how early the obstinate problems of *rigidity* and *theoreticity* announce themselves.

Some skeptics may suspect that cattle has additional drawbacks. But if these skeptics have been willing to make a journey into the possible past they may, by straining their Fantasy a little more, invent animals without such shortcomings, like tame crocodiles or quick turtles.

But arbitrariness is not the normal case. Normally, on the one hand, further measuring objects will be included and, on the other hand, certain of these objects will be thrown away. Only the *paradigmatic objects* remain the same as long as one can work with the same geometry. Only if one encounters empirical difficulties (Michelson-Morley-experiment!) one reconsiders whether the choice of the measuring objects was correct. Deciding to use other measuring objects amounts to *constructing a new conceptual core*, i.e., *to changing over to a new physical geometry*.

For the sake of illustration we show how one can use Sneed's ideas to clarify a point which has caused some confusion in space-time-philosophy. It fits well into the present context because a supporter of the thesis of incommensurability may take this point as a confirmation of his view⁸. For convenience I

8 This aspect has been brought to my attention by Dr. W. Balzer.

refer to the book of Sklar, [46], pp. 97–99, in particular top of p. 97 and bottom of p. 98f. Sklar outlines the conventionalism of Poincaré and the criticism of Eddington and Reichenbach. According to Poincaré, two incompatible theories T_1 and T_2 can agree with the same observational data. Both theories have a ‘geometrical’ part and an additional ‘physical’ part. The difference between the geometrical parts of T_2 and T_1 is compensated ‘by making suitable modifications in the physical part’. As opposed to this, Eddington and Reichenbach emphasize that this is a misleading way of representing the situation: “Properly speaking there is only one theory, although it is written in two different ways” (Sklar, p. 97). What was overlooked by Poincaré is the fact that in the two theories ‘words are being used with different meanings’. This becomes obvious as soon as we realize that the correspondence rules (called ‘coordinative definitions’ by Reichenbach) fixing the reference of geometrical and physical concepts are different in the two cases: “Since the coordinative definitions for terms in the ‘two’ theories are different, these terms simply have different meanings in the two contexts” (Sklar, p. 99).

The idea seems to be the following: we are given two different theories T_1 and T_2 , formulated in the basic terms of these theories and describing the same phenomena P . The difference between T_2 and T_1 is compensated for by using for T_2 correspondence rules C_2 which are different from the correspondence rules C_1 used in the other case. Therefore, the ‘two versions’ of this ‘one’ theory can be formally represented by $\langle P, Z_1, T_1 \rangle$ and $\langle P, Z_2, T_2 \rangle$.

Feyerabend and Kuhn could object, *correctly in my opinion*, that there is an unclear point concerning the *phenomena P*. An object belonging to P , an ‘observed phenomenon’ or an ‘experimental result’, must be described with the help of a sentence containing the basic terms. If these basic terms get *different meanings* by C_1 and C_2 , then it is not only misleading, but *incorrect* to speak in the two cases of *the same phenomena*. “ P ” in the first case must be replaced by “ P_1 ” and in the second case by “ P_2 ”. Thus, upon closer inspection, what in Sklar’s report on Eddington and Reichenbach is called “*one theory written in two misleadingly different ways*” turns out to be *a case of incommensurability*.

Sneed’s account of physical geometry can help us clear up this point. In his presentation the phenomena P are *not* described in a language containing the basic geometrical terms. Rather, these phenomena are characterized within mereology (theory of wholes and their parts) whose language is ‘independent’ of geometrical notions. Every ‘phenomenon’ is an element of M_{pp} , and these partial potential models are mereological systems. It is true that the concept of *point* occurs in mereology as well as in geometry. But this does not do any harm. For the *relational* concepts are different in mereology and physical geometry.

It should be mentioned in passing, first, that with Sneed the ‘correspondence rules’ are replaced by certain constraints; secondly, that the precise nature of T_1 and T_2 does not matter at all. The salient point is that Sneed achieves what seems to be doubted by many philosophers: two *prima facie* *incommensurable* theories can be traced to a *common* ‘observational’ basis.

Encouraged by these results we may draw the following two conclusions:

(1) Within the structuralist approach some important concepts of incommensurability can be explicated;

(2) A particularly alarming kind of incommensurability is the empirical one, *inc_e*. In cases of radical theory change we shall encounter such cases of incommensurability again and again. *But this should not disturb us*. In all such cases our further research may be guided by the conjecture *that the theories are incommensurable because they are based on incompatible underlying theories*.

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

By the way, it must not be forgotten that we have discussed ‘the problem of incommensurability’ only by one paradigmatic example. Undoubtedly, there are further forms of incommensurability and, correspondingly, additional difficulties. Some of these have been treated by Balzer in [5] with explicit reference to examples and texts by Feyerabend.

Because of the zigzag of my exposition in this section it is perhaps of some use to summarize some of my main points. In order not to bore the reader too much I shall, at the cost of some repetitions, use some formulations from the well-known article [45] by D. Shapere, who gave an excellent and very clear survey on some of our problems:

(I) On p. 122, Shapere attributes to Feyerabend and Kuhn the view that science “is always relative to a background framework which itself is purely arbitrary and immune to rational criticism.”

We have severely separated questions of ‘immunity’ from problems of ‘rational criticism’. Theories *are* immune in several respects. But, of course, this does not show that empirical hypotheses are irrefutable. It only exhibits the inadequacy of explicating the notion of a theory in terms of hypothetical claims, as done by the various schools of empiricists and critical rationalists. Just because the notion of theory is, within today’s philosophy of science, in a bad state (vid. [45], p. 124), we must be very careful in differentiating between several notions. We have distinguished between three classes of concepts:

(a) *theory-concepts*, like theory-element, theory-net, core-element, core-net etc.; (b) the *concepts of empirical claims*; (c) *pragmatic concepts*, like holding a theory or theory-evolution.

If somebody objects that these differentiations looked artificial, I would only call to his or her mind our observation that it was the most natural thing in the world to say that all Newtonians held the *same* theory or that they all took part in the evolution of *this* theory. How do you want to express this within the statement view without torturous twists?

(II) In the same context, we find, on p. 122, the often heard claim ascribed to Feyerabend and Kuhn “that science is not ‘objective’, that it does not make real progress”. Taken at its face-value, this statement amounts to the claim that all of us, *including the empiricists, the critical rationalists, Feyerabend and Kuhn*, know exactly what “scientific progress” means in all possible kinds of situations, furthermore that we all have the same notion of progress, and finally that, while all the others believe in progress, Feyerabend and Kuhn do not. This little reflection shows, I think, that something must be fundamentally wrong with this imputation. A more plausible reconstruction of the views of Feyerabend and Kuhn would amount to no more than *a challenge rather than a denial*. This challenge concerns the question what, in case of radical theory change, “progress” could *mean*. And if the challenge is combined with a denial at all, then it is the denial that this question has been answered in the past. To all of this I would subscribe. No satisfactory answer has yet been given to this explicative problem.

If I am right then: (1) there *exists* an answer to this problem; (2) it has to be given in terms of particular *intertheoretical relation*; (3) on the level of *general* philosophy of science, the ‘solution’ consists in the formulation of important necessary conditions of strict and of approximative reduction; (4) only on the level of *special* philosophy of science can ‘real solutions’, in terms of intertheoretical relations, applicable to pairs of particular theories, be given; (5) after successful solutions of the conceptual problems there remain all the difficult problems to find out whether here and there theory change *was* progressive. These are empirical problems. I should perhaps add: “*if they are decidable*”. It might well turn out that they are not. Remember that the reduction relation compares theory-nets. If “progress” is defined in such a manner that, with respect to the dislodged theory, only nets over certain basic cores which have *effectively been constructed* are compared with the net over the basic core of the supplanting theory, then we may come to a definite conclusion: to all nets of the former kind there exist ‘superior’ nets of the latter kind. But what if the relation is defined in terms of *every possible net*? It was in this sense that I claimed in [53] that in a possible world there may exist a Super-Newton who supercedes the counterpart of our Einstein and that thinking this to be possible does *not* amount to accepting epistemological relativism.

(III) Besides the problem of comparability the Feyerabend-Kuhn approach has challenged, according to Shapere, the sharp distinction between “scientific terms” and “metascientific expressions” of the positivistic tradition (vid. [45], second paragraph of p. 122). Shapere mentions as an example the notion of explanation, whose meaning is claimed to undergo substantive change if scientific revolutions take place.

Here, again, it seems to me that the matter has to be looked at from a different angle. The basic mistake made by the empiricist-positivist tradition was not the sharp distinction between science and metascience but (1) the non-distinction between *general* and *special philosophy of science* and (2) the wrong assumption that all important philosophical explications could be given in terms of necessary and sufficient conditions at the level of *general* philosophy of science. Most, or even almost all, explications sought for can be given at the level of special philosophy of science only, i.e., at the level where we deal with *particular* theories, their *special* applications and their *particular* interrelations. For details the reader is referred to § 7. An additional important point to make is, that from this insight it must *not* be concluded that all further work to be done is no longer systematic but historical, or historical plus psychological plus sociological. For, while a great deal of that work certainly *will* turn out to be of this kind, most of the ‘preparatory’ work of systematic studies in particular and, of course, *not only* preparatory work but systematic analyses of the Suppes-Sneed-type as well, will become of at least equal importance. Only by a *combined* approach, including historical-psychological as well as systematic studies, can we hope to get an optimal understanding of the structure and change of science.

(IV) A somewhat similar answer has to be given to Shapere’s claim on p. 125 that “the positivistic analysis of scientific theories as interpreted axiomatic systems is of no help whatever”, for this presupposes “that we already know *which* propositions *are* part of the theory.” As a preliminary step, though, we must recall the ambiguity of “axiomatization” and remember that, in § 1 and § 2, we decided for the reasons given there not to accept the Carnap or formal language approach but the Suppes or informal approach.

But, of course, the force of this challenge remains the same after this decision. The problem is similar to that raised by Kuhn at the London symposium in [22], namely, by what criteria we *identify* a historically given theory. It seems to me that logical, epistemological, *and* historical aspects must be taken into account in order to answer this question for a particular case. In the long footnote 17 in [54], I made a first attempt to systematize these aspects and their relative weight.

(V) The perhaps most important, or at least most difficult, problem, besides problem (II), has to do with the question, dealt with by Shapere on p. 119,

p. 121, p. 124, p. 127 *et passim*, in what way the meaning of observation terms is dependent on theory. All considerations of Shapere's on this topic rest on the presupposition that this problem concerns the observational – theoretical dichotomy. But, if I am right, then we must first look for what this dichotomy is to be replaced by. For, by itself it is the confused amalgam of two different dichotomies, i.e., the *epistemological* dichotomy “observational – non-observational” and what in § 3 I named the ‘*quasi-semantic*’ dichotomy “*T*-theoretical – non-*T*-theoretical”.

There is, first, a *formal* difference between the two dichotomies, and, secondly, a fundamental difference with respect to the *role* they play in philosophy of science in general and in rational reconstructions in particular.

The formal difference is, by itself, fourfold. (1) Within the first dichotomy what is to be counted as “*observational*” is to be positively characterized. By contrast, in every particular case of a theory *T* where the second dichotomy applies, the *T*-theoretical terms are positively singled out by a criterion. (2) The ‘*continuum*’ view (Shapere, p. 127) applies to the first dichotomy only, because only there do we have to make, as Carnap put it, ‘a slice into a continuum’. The domain of application of the second dichotomy is a *finite* number, actually a *very small number* of terms, namely the basic terms of a physical theory *T* presented in informal axiomatization, some of which will turn out to be *T*-theoretical while the remaining ones will turn out to be *T*-non-theoretical. Here, the ‘continuum view’ does not have any sense at all. (3) The observational – non-observational dichotomy is basically *conventional*, while the theoretical – non-theoretical dichotomy is *non-conventional*. (4) Despite its conventional character, the first dichotomy is *non-relative*, while the second one is always *relative to a particular theory*. (One and the same term, e.g., “pressure”, may be theoretical with respect to one theory, e.g., classical particle mechanics, and non-theoretical with respect to another one, e.g., classical equilibrium thermodynamics).

The first dichotomy is of relevance, in particular, within a theory of confirmation and of test, or, more generally, within a theory in whose context it becomes important to clarify the notion of *evidential support*. While for some time some empiricist philosophers believed that observation sentences had to be absolutely certain, there seems now to be general agreement that, independent of where the ‘slice’ in the continuum is made, there will always be ‘hypothetical components’ in observation sentences. Thus, they *may* always be questioned in *particular contexts* and be given up in *certain circumstances*. Does this lead to epistemological difficulties *per se*? As far as I can see, such difficulties would arise only if what I called a ‘methodological holism’ were true, according to which all important theories have to be introduced simultaneously and not in a special order (like, e.g., classical physical geometry → classical kinematics → classical particle mechanics → classical equilibrium ther-

modynamics). For only in *this* case it would become an urgent and difficult problem how to test certain hypothetical assumptions on the basis of observational evidence presupposing the validity of empirical claims of other ‘elements of the whole family of theories’. As far as I can make out, the present available evidence is *not* in favour of this version of holism. Therefore, we can leave the question open whether this will become a serious problem; all the more so, since I neglected as far as possible the confirmation aspect in [52] as well as in the present paper. In [52], I have apparently caused some confusion by the use of the predicate “empirical” which may be associated with either the first or the second dichotomy. But the possible ambiguities can easily be swept away by the following observation: Unless explicitly stated otherwise, “*empirical*” has always been used as a synonym for “*non-theoretical*” and it is therefore to be taken in the *relativized* sense.

As far as the second dichotomy is concerned, the main problem connected with it is the problem of theoretical terms. This has been dealt with in § 3. It may turn out that some other difficulties automatically disappear, namely insofar as they are the result of the confusion between the two dichotomies. The theoretical – non-theoretical distinction can be used to give additional clarification to the notorious phrases “theory dependent” or “theory laden”.

I shall try to give a brief account of what is to be counted ‘*a fact for a theory*’, thereby assuming familiarity of the reader with the technical vocabulary which distinguishes between M_{pp} , M_p , and M . Actually, “fact for a theory” is ambiguous. For a theory T , containing T -theoretical terms, it makes a difference whether we focus upon the facts *described* or the facts *explained*. Suppose, e.g., we consider *CPM* (classical particle mechanics). What is this theory about? The simplest way to answer this question is the following. The theory does not *describe* ‘empirical systems’, i.e., systems of moving particles as describable in the non-theoretical vocabulary of particle kinematics. Rather, it *describes* the corresponding *theoretically enriched entities*, i.e., *particles as endowed with masses and including forces acting between them*. And by doing this, it *explains* the behaviour of the kinematical systems. In general terms: the empirical claims of a theory net are not to describe M_{pp} ’s; this is done by the underlying theory. They describe the theoretical enrichments, i.e., the M_p ’s, as becoming members of M and satisfying the constraints. Thereby, they *explain* the behaviour of the M_{pp} ’s, or, if you prefer, they *explain* the state of affairs describable in the non-theoretical ‘language of the M_{pp} ’s’⁹.

9 I should like to mention in passing that all of this is in accordance with Putnam’s way of characterizing the theory of relativity in [35]. This theory is not about rods, clocks etc., as the old empiricists believed. Rather, it is about a theoretical entity: the *metrical field* whose features it describes, thereby explaining among other things the behaviour of empirical entities, like rods, light rays and clocks.

We must not forget that this is no ‘two-level picture’ of description and explanation, but a multi-level picture because of the relativity of the theoretical – non-theoretical distinction, mirroring the hierarchical order of theories.

§ 12. Concluding Remarks

In view of the great number of logical, epistemological, pragmatic, and historical aspects discussed in this booklet it may have happened that important points have been supplanted by matters of secondary importance. Therefore, I shall try to summarize those points I wanted to accentuate.

The expressions “statement view” and “non-statement view” are ambiguous. Four different meanings are to be distinguished. The *st.v.₁* is the formal language approach or Carnap approach, while “*structuralism*” in the sense of “*non-st.v.₁*” designates that approach within systematic philosophy of science which, instead of working with formalized languages and formalized theories of logic, makes use of *informal* set theory and logic only. For brevity, I called it the *Suppes approach*.

My main claim was that, at least in the field of special philosophy of science (dealing with particular physical theories), *st.v.₁* is *not a realistic and viable alternative to non-st.v.₁*, but is rather a product of wishful thinking. This claim, expressed in Thesis 1 of § 1, is based on the empirical fact that (1) of § 1 is not true, i.e., that philosophy of science is presently not populated by Super-Super-Montagues. Therefore, the preference given to structuralism has its source in a merely *practical* demand. The situation was illustrated with an analogy taken from mathematics. To the Carnap approach there corresponds the *metamathematical approach* (or ‘Shoenfield approach’), while to the Suppes approach there corresponds the *Bourbaki approach*. Bourbaki succeeded in giving a reconstruction of modern mathematics in precise terms only because he did *not* aim at formalizing the whole of mathematics, as, e.g., Shoenfield did with set theory, but made extensive use instead of informal logic and informal set theory.

Insofar as the structuralist approach concerns the *mathematical* aspect of physical theories only, one cannot speak of an extension of the Bourbaki programme but simply of an *inclusion* of theories of mathematical physics into this programme. The strive for such an inclusion goes back to Suppes’ attempts to apply modern standards of rigour to axiomatizations of physical theories. (In retrospect, this looks quite natural since mathematical physics *is* mathematics too, i.e., the structures it describes *are* mathematical structures. It is an

additional merit of Suppes that he succeeded in applying the Bourbaki standard to other disciplines as well; but this is not the point of our present discussion.)

Mathematical physics is mathematics too. *But it is far more besides.* Here it becomes a great danger to regard an *operationalist ideology* as this ‘more’ and to build this ideology into the axiomatization. The pre-Suppes axiomatizations give some good examples for such ideologies. We cannot speak of an *analogue to* or an *extension of* the Bourbaki programme before informal axiomatics is supplemented by *informal model theory (informal semantics)*. Sneed and Balzer & Sneed have sketched this theory on the level of general philosophy of science and Sneed and others have applied it to various particular physical theories.

I recall¹ that the differences of opinion between Suppes and Sneed formed the clue for an accession and for a better understanding of the new approach and for the strong liberalization of the empiricist attitude implicit in it. The starting point was Suppes’ view on the purpose of a theory of fundamental measurement. As we have realized, the empiricism of Suppes is at the same time very liberal and very sophisticated insofar as the principle of empiricism reduces to the requirement that for all quantities introduced by fundamental measurement a *representation theorem* has to be proved. Even most open-minded empiricists will find this attitude of Suppes *much too liberal*. By contrast, Sneed’s position is distinguished from all the potential empiricist criticisms of Suppes since he considers the empiricism of Suppes still *much too narrow*. According to his view it is impossible to prove a representation theorem for those quantities of a theory that are theoretical with respect to this theory.

Therefore, the *Suppes-Sneed programme*, as the axiomatization of physical theories supplemented by informal model theory could be named, not only seems to be *the first existing analogue of the Bourbaki programme*. In addition it presumably is *the most liberal of all known variants of empiricism*.

In order to prevent a misunderstanding of this claim, in particular by philosophers who prefer a historical approach within philosophy of science, there must be absolute clarity with respect to the following point. *The Suppes-Sneed programme as an analogue to the Bourbaki programme will convince only those who share with those two authors and with me the following view: that the usual accounts of physical theories are quite unsatisfactory, and that we must not start with the presupposition that the physical accounts are abundant with respect to precisions and that what is missing is just an adequate ‘philosophical interpretation’ of these accounts, satisfactory as they are by themselves.*

The difference of opinions will already start with the axiomatizations. From Suppes’ standpoint the usual axiomatizations “fail, in differing degrees depend-

1 Vid. § 2.

ing upon the specific example, to meet modern standards of logical rigour. Primitive concepts and axioms are sometimes not clearly identified; questions of independence of primitives and of axioms are not carefully raised; the epistemological status of the axioms is often fuzzy; ‘physical intuition’ is sometimes employed as an inference rule in obtaining theorems”². In principle, the situation is analogous to that in mathematics. All those mathematicians, if there are any, who hold the view that the traditional presentations of topology, analysis, differential geometry etc. are satisfactory, will deny that the realization of the Bourbaki programme has contributed much to a clarification of mathematical concepts, structures, and connections.

Still, the title of this booklet will be regarded by many as too pretentious. It could be weakened to the counterfactual question: What *would* General Bourbaki do if he were forced to deal with physics, but *not only* with the mathematical aspect of physics? We have realized that many concepts to be introduced and many questions to be raised are typically philosophical, like the following ones: the question if and why the ‘one big application’ of a theory is to be replaced by *an open class of intended applications*; the concept and the problem of *theoreticity*; the distinction between *theory-elements* and their *empirical claims*; the question of how the framework can be kept open for ‘*pragmatizations*’; the *incommensurability-problems* and what-not. We began in § 1 with the possible extension of the Bourbaki programme and are now ending with speculations about possible activities (including philosophical ones!) of Bourbaki. You will perhaps ask: “Is this reasonable at all?” Well, it is justified as a permanent reminiscence of the high standard of technical *and* philosophical precision required by the Suppes-Sneed approach.

For those who remain unconvinced, the whole Bourbaki analogy will become pointless. Rejecting the need for additional clarification and precision, the controversy will amount for them to looking for the uninteresting answer to the question whether unnecessary things ought to be done within the formal language approach or in terms of informal set theory. Similarly, the comparison between the statement views and the non-statement views will, for these people and for them only, become pointless, but this time in the sense that the former views are trivially correct. For, if nothing has to be reconstructed, we are referred back to textbooks in mathematical physics. And, as is easily verified, *these books consist of pages filled with statements*.

This attitude of indifference and apathy will presumably prevail among such philosophers who consider systematic philosophy of science as being less important than psychology, sociology, and history of science. Philosophers of such persuasions can perhaps be calmed down with the remark that the controversy is not a fight for philosophical monopoly. There may be numerous

2 Moulines-Sneed, [33], p. 65.

questions to which satisfactory answers can be found only in one of these other disciplines. Only *insofar as* systematic studies are considered important, should the structuralist approach be preferred to the formal language approach.

We will not be able to evaluate the *relative* importance of *all* the various possible approaches before ‘most of the work is done’. Until then much time will elapse.

For we must not forget one thing: the Bourbaki programme is realized to a large extent, while philosophy of science is still in its infancy, no matter whether we consider it to be done in the style of the Bourbaki analogue or not. (This is why I do not have illusory hopes about the realization of the programme. There are many possible reasons for its failure: that it will not interest enough people at all; that it will attract more philosophically minded researchers without sufficient ‘scientific’ and ‘technical’ background; that qualified students will have strong disinclinations to wasting much thought and energy in a project starting from a relatively primitive basis etc.)

The foregoing polemical remarks against *st.v.*₁ must be restricted in two respects:

(1) It may turn out that certain kinds of problems can only be solved by using formalized theories. The so-called *Ramsey-eliminability* in the sense of Sneed is a case in point³. In this question, e.g., it may turn out to be of importance that what we are interested in are *negative* results, i.e., the impossibility of eliminating theoretical terms by means of finding a Ramsey-substitute⁴. For such a purpose the formalization of a small subpart of the theory in question will perhaps suffice. But this is a vague intuition and I may be mistaken.

(2) *Partial* formalizations of certain items will perhaps turn out to be advisable or even necessary. Van Fraassen’s ‘semantic analysis’ of physical theories can be taken to illustrate this point⁵. Within van Fraassen’s approach the object of formalization is not the ‘main theory’ but a *satellite theory* of ‘elementary sentences’ which are mapped on certain entities of the main theory. As the latter forms the object of reconstruction within the structuralist framework, the two approaches are, of course, wholly compatible.

The second important meanings of the phrases “statement view” and “non-statement view” were *st.v.*₂ and *non-st.v.*₂. They concern something totally

3 I remind the reader that “Ramsey-eliminability” in this sense is a technical term which, roughly speaking, means that the effect of the constraints on the theoretical functions can be reproduced by suitably devised constraints on the non-theoretical functions. This must, of course, not be confused with the position of some philosophers according to which adopting the Ramsey-view is a way of eliminating theoretical terms. Vid. Sneed, [47], p. 49, p. 52f., and p. 75.

4 For a brief explanation of the problem alluded at vid. [47], p. 52ff. and [52], p. 64ff.

5 vid. [62].

different from *st.v.₁* and *non-st.v.₁*. The new approach suggests differentiating strictly between *theories* or *theory-nets* on the one hand and *empirical claims*, associated with theories and theory nets, on the other hand. What is the logical status of these empirical claims? According to the position of the *st.v.₂*, or the traditional view, empirical claims of theories have to be represented by infinite classes of sentences. According to the position of *non-st.v.₂*, each such claim is just *one* big claim to be represented by an emended Ramsey-sentence $I \in A_e(E)$ (earlier version in terms of expanded cores) or $I \in A(N^*)$ (improved version in terms of theory-element-cores).

This time, the verdict against the statement view is even stronger than in the first case. While *st.v.₁* is at least logically possible, *st.v.₂* is *no logical possibility if theoretical terms are involved*. In other words: *st.v.₂* is wrong. This claim is also based on an empirical assumption, namely *that Sneed's conception of theoretical concepts is basically correct*. The phrase "basically correct" means that each nontrivial physical theory *T* contains *T*-theoretical terms. (It does not matter whether, in the particular case of classical particle mechanics, Sneed is right that, besides *force*, *mass* is a theoretical term as well.)

The third meaning of "*non-st.v.*" was that of *non-st.v._{2.5}*. This has to do with the way fundamental or special laws 'enter' or become 'part of' the empirical claims. It is important because it may transform the seemingly refutable into the irrefutable ('sophisticated view').

The last meanings are those of *st.v.₃* and *non-st.v.₃*. They parallel the difference between *st.v.₁* and *non-st.v.₁* on the level of general philosophy of science. Since we can at least *feign* on the general level that *st.v.₁* is feasible, the statement view of theories for the first time gets a *possible* positive chance. Which approach is to be preferred depends on the respective aims and problems. As emphasized in § 7 in many cases even on this general level the structuralist approach will turn out to be more advantageous than statement view₃. The two main reasons are the following ones: first, the greater flexibility of *non-st.v.₃*, which manifests itself in its ability to allow more and better differentiations, facilitating our understanding of the systematic-static as well as the historical-dynamic aspects; secondly, the facts, closely connected with the first point, that in this way unforced transitions to *pragmatizations* are to be more easily performed and the door is opened to an urgently needed systematic pragmatics⁶.

Philosophers who still remained unconvinced will perhaps be reassured by the following observation: The method of theory nets which replaces the former procedure of expanded cores (vid. § 4) demonstrates that the structuralist approach and the statement view are, contrary to the *prima facie* impression, not very remote from each other. For a theory net consists of members

6 This aspect has already been pointed out by Tuomela in [61].

$\langle K', I' \rangle$ and with each such member there is associated the empirical hypothesis $I' \in A(K')$. What a physical theory 'has to say' at an historical time t is, basically, just the result of 'collecting together' all these particular claims belonging to the theory-net at t .

At the beginning of this section I mentioned that the large number of details may have pushed important things into the background. There is an even greater likelihood that the reader may have meanwhile forgotten what I said in the introduction about Feyerabend's comments. I should like to emphasize once more that I have been inspired to develop many of the new ideas presented in the present paper by his highly competent remarks. Even those passages in which, in my opinion, he is not right, were very useful for me, indeed, for in these remarks he held up a mirror to my presentations from which I could read off the mistakes which I had made in the organization and formulation of my thoughts. If the present reflections have contributed to additional clarification, I owe this to a large extent to his ingenious and penetrating criticism.

It is a pity that I did not receive his remarks a few years ago. In such a case I might have written a much better book than I actually did.

I do hope that, in writing the present synopsis, I succeeded not only in removing several confusions and in closing some gaps but also in conveying to the reader the main reasons for my optimism in regard to the structuralist outlook. For this new approach seems to be a most promising tool for clarifying various prevalent but yet unexplicated notions as well as for solving quite a number of puzzles which have been haunting the philosophy of science for a long time.

Formal Appendix

Here are summarized the formal definitions of concepts discussed in the text. Only those concepts are considered that contribute to the general formal framework of theory construction and theory description.

Familiarity with standard set-theoretic notation is assumed. In addition, the following notational conventions are used. $X \in \mathfrak{M}$ means that X is a nonempty set. \subseteq is written for inclusion, \subset for proper inclusion. n -tuples are denoted by $\langle x_1, \dots, x_n \rangle$, Cartesian products of two sets by $M \times N$. If $R \subseteq M \times N$ is a relation then its domain is

$$D_I(R) := \{x | x \in M \wedge \exists y (\langle x, y \rangle \in R)\}$$

and its range is

$$D_{II}(R) := \{y | y \in N \wedge \exists x (\langle x, y \rangle \in R)\}.$$

If R is a relation then the inverse relation is denoted by \check{R} and defined by $\check{R} := \{\langle y, x \rangle | \langle x, y \rangle \in R\}$ ¹. We write $f: M \rightarrow N$ to indicate that f is a function from M into N , and $f: M \leftrightarrow N$ to indicate that f is, in addition, a bijection from M onto N . The union of sets x having some common property $A(x)$ is denoted by $\bigcup \{x | A(x)\}$. (This corresponds to the usual

$\bigcup_{A(x)} x$.) $M \setminus N$ denotes the difference set $\{x \in M | x \notin N\}$. \mathbb{N} is the set of nonnegative integers $0, 1, 2, \dots$. For $n \in \mathbb{N}$ and $M \in \mathfrak{M}$ we define $\mathfrak{P}^n(M)$ recursively by: $\mathfrak{P}^0(M) := M$ and $\mathfrak{P}^{n+1}(M) := \mathbf{Po}(\mathfrak{P}^n(M))$, where $\mathbf{Po}(M)$ is the power set of M .

Concepts from §4

D1 X is an $m+k$ matrix iff

- 1) $X \in \mathfrak{M}$;
- 2) $m, k \in \mathbb{N}$ and $0 < m$;
- 3) for all $x \in X$ there exist $n_1, \dots, n_m, t_1, \dots, t_k$ such that $x = \langle n_1, \dots, n_m, t_1, \dots, t_k \rangle$.

¹ More precisely, the definiens should of course be written as.

$\{z | \exists x \vee y (z = \langle y, x \rangle \wedge \langle x, y \rangle \in R)\}$. Similar 'abuse of language' will occur in what follows.

The n_i 's are called *non-theoretical terms*, the t_j 's are called *theoretical terms*. An n_i or a t_j may be a 'real' set or a relation or a function. If, for example, n_i is a function and there is some reason to mention explicitly the domain and the range of n_i , it is understood that both are also components of x . [This requirement is not necessary. For we can, in principle, always treat a function f as a set of ordered pairs given with its domain $D_1(f)$ and its range $D_2(f)$.]

D2 X is a *core* iff there exist M_p, M_{pp}, M, C, m , and k such that

- 1) $X = \langle M_p, M_{pp}, M, C \rangle$;
- 2) M_p is an $m+k$ matrix;
- 3) $M_{pp} = \{ \langle n_1, \dots, n_m \rangle \mid \vee t_1, \dots, t_k (\langle n_1, \dots, n_m, t_1, \dots, t_k \rangle \in M_p) \}$;
- 4) $M \subseteq M_p$;
- 5) $C \subseteq \mathfrak{P}^1(M_p)^2$, $C \neq \emptyset, \emptyset \notin C$, $\forall x \in M_p (\{x\} \in C)$.

M_p, M_{pp}, M, C are called the sets of *potential models*, *partial potential models*, *models*, and *constraints*, respectively. Constraints satisfying the requirement,

$$\wedge X, Y (X \in C \wedge Y \subseteq X \rightarrow Y \in C),$$

are called *transitive*.

D3 If $K = \langle M_p, M_{pp}, M, C \rangle$ is a core then

- 1) the functions $r^i: \mathfrak{P}^i(M_p) \rightarrow \mathfrak{P}^i(M_{pp})$ are defined for $i \in \mathbb{N}$ inductively by $r^0(\langle n_1, \dots, n_m, t_1, \dots, t_k \rangle) := \langle n_1, \dots, n_m \rangle$; $r^{i+1}(X) := \{r^i(Y) \mid Y \in X\}$ for $X \in \mathfrak{P}^{i+1}(M_p)$;
- 2) $\mathbb{A}(K) := r^2(\mathfrak{P}^1(M) \cap C)$.

The r^m 's are the functions cutting off the theoretical terms t_1, \dots, t_k . $\mathbb{A}(K)$ is called the *empirical content* of K .

D4 T is a *theory-element* only if there exist K and I such that

- 1) $T = \langle K, I \rangle$;
- 2) K is a core;
- 3) $I \subseteq \mathfrak{P}^1(M_{pp})$.

I is called the *range of intended applications*. In contrast to earlier formulations, the elements of I are now treated as sets of partial potential models. This reflects the idea that, for an x , any system y of the same type as x is an intended application too. Thus, classes of partial potential models of the same type are called applications. This seems to be in accordance with the use of the word "application" by physicists.

In order to determine I we must use pragmatic concepts or at least concepts in which that kind of vagueness is involved which Wittgenstein called family resemblance ("Familienähnlichkeit").

2 It should be observed that \mathfrak{P}^1 is, by definition, the same as \mathbf{Po} .

D5 If $T = \langle K, I \rangle$ is a theory-element then *the empirical claim of T* is the sentence $I \subseteq \mathbb{A}(K)$.

T₁ If $T = \langle K, I \rangle$, $X \subseteq \mathbb{A}(K)$, and Y is such that $\bigwedge y \in Y \bigvee x \in X (y \subseteq x)$ then $Y \subseteq \mathbb{A}(K)$.

If $x \in M$ then $r^1(\{x\}) \in \mathbb{A}(K)$.

D6 a) If K' and K are cores then K' is a *core specialization* of K (in the naive sense) iff

$$1) M'_{pp} = M_{pp}(M'_{pp} \subseteq M_{pp});$$

$$2) M'_p = M_p(M'_p \subseteq M_p);$$

$$3) M' \subseteq M;$$

$$4) C' \subseteq C.$$

b) If T' and T are theory-elements then T' is a *specialization* of T (in the naive sense) iff

1) K' is a core specialization of K (in the naive sense);

2) $I' \subseteq I$.

The main requirement in **D6** is that $M' \subseteq M$, the normal case being that $M' \subset M$. This reflects the idea that a special law M' , if compared with the basic law (or its set of models) M , admits special models only.

Specializations in the *naive* sense are thought of as working as follows. First, subsets M'_{pp} and M'_p (of the original sets M_{pp} and M_p respectively) are picked out. M'_{pp} is the set of possible systems to which the special law can be applied. Secondly, this special law M' picks out a subset of M'_p , namely those potential models which are to satisfy the special law.

A more *sophisticated view* gives up the idea that with special laws there is connected a specific way of restricting the corresponding M_{pp} 's. $I' \subset I$ expresses the thought that some intended applications of T are no longer intended applications of T' ; in other words, they are abandoned in view of the special law. In the sophisticated view $I' \subseteq I$ cannot originate from $M'_{pp} \subset M_{pp}$ by setting $I' = I \cap M'_{pp}$. This means that the special law picks out, at least partially, its own intended applications. This phenomenon is called *autodetermination*. (Vid. [52], pp. 196 ff.)

T₂ If T' is a specialization of T then $I' \subseteq I$, $\mathbb{A}(K') \subseteq \mathbb{A}(K)$, and if $I \in \mathbb{A}(K)$ then $I' \in \mathbb{A}(K)$.

D7 If T' and T are theory-elements, then T' is a *theoretization* of T (or T' is *presupposed by* T) iff $M'_{pp} \subseteq M$.

D8 X is a *theory-net* iff there exist N, \leq such that

$$1) X = \langle N, \leq \rangle;$$

2) N is a finite set of theory-elements;

$$3) \leq \subseteq N \times N;$$

4) $\bigwedge T, T' \in N (T \leq T' \leftrightarrow T$ is a specialization of $T')$;

5) $\bigwedge \langle K, I \rangle, \langle K', I' \rangle \in N (I = I' \rightarrow K = K')$.

According to **D8** a theory-net is a set of theory-elements ‘ordered’ by the relation of specialization. This ordering has some nice properties which justify speaking of nets (vid. **T₃**). **D8-5** excludes one and the same range of intended applications in the net being treated by different cores. If such a thing happens in history with respect to, say, two cores, either one core is a core specialization of the other and the latter is dropped or the two cores are not comparable and it is likely that one of the elements is excluded from the theory.

T₃ If $\langle N, \leq \rangle$ is a theory-net then $\langle N, \leq \rangle$ is a net, i.e. for all $x, y, z \in N$

- 1) $x \leq x$;
- 2) $x \leq y \wedge y \leq z \rightarrow x \leq z$;
- 3) $\sim \subseteq N \times N$, defined by $x \sim y \leftrightarrow x \leq y \wedge y \leq x$, is an equivalence relation;
- 4) \sim is the identity on N .

This theorem says that a theory-net $\langle N, \leq \rangle$ is in fact a poset, i.e. a set partially ordered by \leq .

D9 Let $X = \langle N, \leq \rangle$ be a theory net.

Then

- a) $\wedge T, T' \in N (T' \sim T \leftrightarrow T' \leq T \wedge T \leq T')$;
- b) $\mathfrak{B}(X) := \{T \in N \mid \wedge T' \in N (T \leq T' \rightarrow T \sim T')\}$;
- c) X is *connected* iff
 $\wedge T, T' \in \mathfrak{B}(X) \vee T^* \in N (T^* \leq T \wedge T^* \leq T')$;
- d) X is a *uniquely based theory-net* iff there is a $\langle K, I \rangle \in N$ such that
 $\mathfrak{B}(X) = \{\langle K, I \rangle\}$.

If $T' \sim T$ then T' and T are called *equivalent*. The elements of $\mathfrak{B}(X)$ are called *basic*.

D10 If $X = \langle N, \leq \rangle$ and $X' = \langle N', \leq' \rangle$ are theory-nets then N is a (*proper*) *expansion* of N' [or a (*proper*) *refinement* of N'] iff

- 1) $N' \subseteq N$ ($N' \subset N$);
- 2) $\leq' = \leq \cap (N' \times N')$.

D11 If $X = \langle N, \leq \rangle$ is a theory net then *the empirical claim* of X is that
 $\wedge \langle K, I \rangle \in N (I \subseteq \mathbb{A}(K))$.

T₄ If $X = \langle N, \leq \rangle$ and $X' = \langle N', \leq' \rangle$ are theory-nets, where X is a refinement of X' then the empirical claim of X implies the empirical claim of X' , i.e. $\wedge \langle K, I \rangle \in N (I \subseteq \mathbb{A}(K)) \rightarrow \wedge \langle K, I \rangle \in N' (I \subseteq \mathbb{A}(K))$.

Concepts from §5

For technical purposes only, slight modifications of the definitions given in §4 suggest themselves.

D12 X is a *pragmatically enriched theory-element* iff there exist T , SC , h , and F such that

- 1) $X = \langle T, SC, h, F \rangle$;
- 2) T is a theory-element;
- 3) SC is a scientific community;
- 4) h is an historical interval;
- 5) $F \subseteq I$.

The elements of F are those intended applications which are acknowledged by all members of SC , i.e. the *firm* applications.

D13 $X = \langle |N|, \leq \rangle$ is a *pragmatically enriched theory-net* (p.e. net) iff

- 1) $|N|$ is a finite set of pragmatically enriched theory-elements³;
- 2) $\wedge \langle T, SC, h, F \rangle, \langle T', SC', h', F' \rangle \in |N| (SC = SC' \wedge h = h')$;
- 3) $\leq \subseteq |N| \times |N|$;
- 4) $\wedge \langle T, SC, h, F \rangle, \langle T', SC', h', F' \rangle \in |N|$
 $(\langle T, SC, h, F \rangle \leq \langle T', SC', h', F' \rangle \leftrightarrow T \text{ is a specialization of } T')$.

T₅ Every p.e. net is a net in the sense of **T₃**.

p.e. nets represent the state of affairs at particular historical times. All elements of a net have the same historical interval as third component and consequently the same scientific community as second component.

D14 Let $H \in \mathfrak{M}$ be finite and $< \subseteq H \times H$. Then

- a) $\langle H, < \rangle$ is an *historical order* iff for all $x, y, z \in H$: $\neg x < x$;
 $x < y \wedge y < z \rightarrow x < z$; $x < y \vee y < z$.
- b) If $\langle H, < \rangle$ is an historical order then
 - b₁) $\min(H) :=$ the unique $h \in H$ such that $\wedge y \in H (h < y \vee h = y)$;
 - b₂) for $h \in H$, $h \neq \min(H)$,
 $h - 1 :=$ the unique $h' \in H$ such that
 $h' < h \wedge \neg \vee x \in H (h' < x \wedge x < h)$.
- c) If X is a p.e. net then $h(X) :=$ the unique h such that
 $\wedge \langle T, SC, h', F \rangle \in |N| (h' = h)$.
- d) If X is a p.e. net then
 - d₁) $SC(X) :=$ the unique SC such that
 $\wedge \langle T, SC', h, F \rangle \in |N| (SC' = SC)$;
 - d₂) $F(X) := \bigcup \{ F \mid \vee T, SC, h (\langle T, SC, h, F \rangle \in |N|) \}$;
 - d₃) $A(X) := \bigcup \{ I \mid \vee F \mid \vee K, SC, h (\langle \langle K, I \rangle, SC, h, F \rangle \in |N|) \}$;
 - d₄) If \mathcal{N} is a set of p.e. nets then $H(\mathcal{N}) := \{ h(X) \mid X \in \mathcal{N} \}$.

D14 comprises various auxiliary definitions. The requirements of **D14-a)** are just the axioms of a linear order (for a finite H). The name “historical order” indicates that in the present context historical time intervals are

³ Whenever, in the sequel, a net or a p.e. net is called X , it is always to be understood that $|N|$ is the first member of the ordered pair X .

meant which in fact are linearly ordered by the ‘before-after’ relation, ignoring the possibility of overlapping intervals.

Every finite linear order $\langle H, < \rangle$ has a ‘first’ or ‘minimal’ element $\min(H)$, and each $h \in H$ different from the first element has a unique ‘predecessor’ $h-1$ with respect to $<$ (**D14-b**).

In a p.e. net X the historical interval and the scientific community are the same for all elements of the net. They are therefore uniquely denoted by $h(X)$ and $SC(X)$ respectively. If we have several p.e. nets then the corresponding historical intervals are collected in the set $H(\mathcal{N})$. The elements of $F(X)$ are called *firm applications* and the elements of $A(X)$ are called *assumed applications* (**D14 d**, d_2) and d_3). Note that $F(X)$ and $A(X)$ are sets of partial potential models!

D15 E is a *theory-evolution* iff there exist \mathcal{N} , $<$ such that

- 1) $E = \langle \mathcal{N}, < \rangle$;
- 2) \mathcal{N} is a finite set of p.e. nets;
- 3) $\langle H(\mathcal{N}), < \rangle$ is an historical order:
- 4) $\wedge X \in \mathcal{N} \wedge \langle T, SC, h, F \rangle \in |N|$

$$[h \neq \min(H(\mathcal{N})) \rightarrow \vee X' \in \mathcal{N} (h(X') = h - 1$$

$$\vee \langle T', SC', h', F' \rangle \in |N'|,$$

whereby T is a specialization of T'].

The concept of theory-evolution can be used to depict the development of theories over time. It consists essentially of a sequence of p.e. nets, each representing an ‘instantaneous’ description of the scientific situation at a certain historical interval. **D15-4**) says that in each historical interval, different from the first one, every theory-element is a specialization of some theory-element belonging to the historical interval ‘just before’. This does not necessarily mean that the net at time h has more elements than the net at time $h-1$. What is excluded by **D15-4**) is that new theory-elements are introduced at h which do not fit the previous situation, i.e. which are not specializations of previous theory-elements.

D16 If $E = \langle \mathcal{N}, < \rangle$ is a theory evolution then

- a) E is *progressive* iff
 $\wedge X, X' \in \mathcal{N} (h(X) < h(X') \rightarrow F(X) \subseteq F(X'))$;
- b) E is *perfect* iff
 - 1) E is progressive;
 - 2) $\wedge X \in \mathcal{N} \vee X' \in \mathcal{N} [h(X) < h(X') \wedge (A(X) = \emptyset \vee A(X) \subseteq F(X'))]$.

If we think of $F(X)$ as the set of those applications occurring in the net X which are acknowledged by all the members of the scientific community at $h(X)$, then E being *progressive* means that the number of such commonly acknowledged applications increases with time. If, in addition, all the

assumed applications of X at time $h(X)$ appear at some future time as elements of $F(X')$, i.e. as firm applications, with $h(X) < h(X')$, then E is called *perfect*. As far as we know, there are no real examples of perfect theory-evolutions. On the other hand it has become clear by now that the development of Newtonian mechanics provides an example of a progressive theory-evolution.

Concepts from § 6

D17 Let $E = \langle \mathcal{N}, < \rangle$ be a theory-evolution, $h, h' \in H(\mathcal{N})$ such that $h = h' - 1$ and $X, X' \in \mathcal{N}$ with $h(X) = h, h(X') = h'$.

a) E is *theoretically progressing at h* iff

$$\forall \langle \langle K, I \rangle, SC, h, F \rangle \in |N| \vee K' \vee F'$$

$(F' \subseteq F \wedge \langle K', I \rangle \text{ is a specialization of } \langle K, I \rangle$

$$\wedge |N'| = |N| \cup \{ \langle K', I, SC, h', F' \rangle \}.$$

b) E is *applicative progressing at h* iff

$$\forall \langle \langle K, I \rangle, SC, h, F \rangle \in |N| \vee I' \vee F'$$

$$\cdot (I \subset I' \wedge F \subseteq F' \wedge |N'| = |N| \cup \{ \langle \langle K, I' \rangle, SC, h', F' \rangle \}).$$

c) E is *confirmatory progressing at h* iff

$$\forall \langle K, I, SC, h, F \rangle \in |N| \vee F' (F \subset F' \wedge |N'| = |N| \cup \{ \langle \langle K, I \rangle, SC, h', F' \rangle \}).$$

D17 is an attempt to use our formalism for differentiating between various concepts of progress. Thereby, only two successive historical intervals are considered because we will hardly find examples of theory-evolutions progressing in *only one* of the ways all the time.

Theoretical progress is characterized by an increase of the number of theory-elements such that the new theory-element comes from a core specialization of an element already available, while the intended applications remain the same. (Of course, one might liberalize **D17-a**) by allowing $|N'|$ to consist of $|N|$ plus several new specializations instead of only one.) If the theoretical apparatus is specialized, we cannot be sure that all firm applications remain firm with respect to this new apparatus. This is why the definition contains the additional clause $F' \subseteq F$.

Progress with respect to application which may be called *empirical progress*, means that the set of intended applications is enlarged, possibly combined with a simultaneous increase of firm applications, while the cores remain unchanged.

Finally, *progress with respect to confirmation* is explicated as an increase of firm applications. In this type of case the cores as well as the intended applications remain the same.

D18 Let $Q, Q' \in \mathfrak{M}$ and $T = \langle M_p, M_{pp}, M, C, I \rangle$, $T' = \langle M'_p, M'_{pp}, M', C', I' \rangle$ be theory-elements⁴.

a) R *reductively corresponds* Q' with Q ($rd(R, Q', Q)$) iff

- 1) $R \subseteq Q' \times Q$;
- 2) $D_I(R) = Q'$;
- 3) $\tilde{R} : D_{II}(R) \rightarrow Q'$.

b) If $rd(R, Q', Q)$ then

$$\tilde{R} := \{ \langle X, Y \rangle \in \mathfrak{P}^1(Q') \times \mathfrak{P}^1(Q) \mid \vee c : X \leftrightarrow Y \wedge \\ \wedge x \in X (\langle x, c(x) \rangle \in R) \}.$$

c) If $R \subseteq M'_p \times M_p$ then

$$\hat{R} := \{ \langle x', x \rangle \in M'_{pp} \times M_{pp} \mid \vee \langle y', y \rangle \in R (x' = r^0(y')) \wedge \\ x = r^0(y)) \} \quad (\text{comp. D3-1}).$$

d) R *reduces* T' to T iff

- 1) $rd(R, M'_{pp}, M_{pp})$;
- 2) $\wedge \langle X', X \rangle \in \mathfrak{P}^1(M'_{pp}) \times \mathfrak{P}^1(M_{pp}) (X \subseteq A(K) \wedge \\ X' \in \mathfrak{M} \wedge \langle X', X \rangle \in \tilde{R} \rightarrow X' \subseteq A(K'))$;
- 3) $\wedge x' \wedge X' (x' \in X' \wedge X' \in I' \rightarrow \vee X \vee x (x \in X \wedge X \in I \wedge \langle x', x \rangle \in R))$

e) R *strongly reduces* T' to T iff

- 1) $rd(R, M'_p, M_p)$ and $rd(\hat{R}, M'_{pp}, M_{pp})$;
- 2) $\wedge \langle Y', Y \rangle \in \mathfrak{P}^1(M'_p) \times \mathfrak{P}^1(M_p) (Y \in \mathfrak{P}^1(M) \cap C \\ \wedge Y' \in \mathfrak{M} \wedge \langle Y', Y \rangle \in \tilde{\hat{R}} \rightarrow Y' \in \mathfrak{P}^1(M') \cap C')$;
- 3) $\wedge \langle x', x \rangle \in \hat{R} \wedge y \in M (x = r^0(y) \rightarrow \vee y' \in M' \\ (\langle y', y \rangle \in R \wedge r^0(y') = x'))$;
- 4) $\wedge x' \wedge X' (x' \in X' \wedge X' \in I' \rightarrow \vee x \vee X (x \in X \wedge X \in I \wedge \langle x', x \rangle \in \hat{R}))$

f) T' *reduces to* T iff there is an R such that R reduces T' to T .

g) T' *strongly reduces to* T iff there is an R such that R strongly reduces T' to T .

⁴ For simplicity, theory-elements are formally represented here as quintuples. A suitable formal definition of n -tuples would give this representation anyway.

- h) If $X = \langle |N|, \leq \rangle$ and $X' = \langle |N'|, \leq' \rangle$ are theory-nets with $\mathfrak{B}(X) = \{T\}$ and $\mathfrak{B}(X') = \{T'\}$ then R (strongly) reduces X' to X iff
- 1) R (strongly) reduces T' to T ;
 - 2) $\wedge T_1 \in |N'| \vee T_2 \in |N| [R \text{ (strongly) reduces } T_1 \text{ to } T_2]$.

D18 summarizes the formalism for the treatment of reduction. If R reductively corresponds Q' with Q we also say that R is a *reduction relation*. This means that to each element of Q' there are associated via R one or more elements of Q and that, in addition, the converse relation \hat{R} is a function (**D18-a**). **D18-b** ‘transports’ the reduction relation R to the level of power sets. In order to restrict the possible extensions it is required that both sets X and Y standing in the \hat{R} -relation have the same cardinality. This excludes some undesired results. **D18-c** ‘projects’ a reduction relation from the theoretical onto the non-theoretical level. **D18-d**) and **D18-e**) characterize the reduction on the non-theoretical and on the theoretical level respectively. The main difference is that in **D18-d**) the original reduction relation R works on the level of partial potential models while in **D18-e**) it works on the M_p ’s. In both cases the requirement 2) says that the laws of the reduced theory T' follow from the laws of the reducing theory T ⁵. **D18-d3**) and **D18-e4**) express that to each single intended application of the reduced theory there corresponds, via R , some intended application of the reducing theory. In other words, all applications ‘explained’ by the reduced theory are ‘explained’ in the form of corresponding applications by the reducing theory. In **D18-e**) it is required, in addition, that \hat{R} reductively corresponds M'_{pp} with M_{pp} . This, together with **D18-e3**), guarantees that a strong reduction of T' to T by R implies a reduction of T' to T by \hat{R} .

T₆ If R strongly reduces T' to T then \hat{R} reduces T' to T .

It can be proved that the reduction relation among two theory-elements extends to theory-nets, provided that, first, the nets are ‘built on’ these two theory-elements respectively, and that, secondly, one allows for an arbitrary net on the reducing side.

T₇ If T' and T are theory-elements and R (strongly) reduces T' to T then for all specializations T_1 of T' there is some specialization T_2 of T such that R (strongly) reduces T_1 to T_2 .

T₈ If X' is a theory-net based on T' and R (strongly) reduces T' to T then there exists a theory-net X based on T such that R (strongly) reduces X' to X in the sense of **D18-h**).

5 Within the present intuitive context we use, for simplicity, the word “theory” instead of “theory-element”.

If, on the other hand, X and X' are given from the beginning then a given reduction between the basic elements cannot automatically be extended to a reduction obtaining between the whole nets.

It seems worth mentioning that the three intertheoretic relations described: *specialization*, *theoretization* (presupposition), and *reduction*, are logically independent.

T₉ If α and β are two distinct relations from the set {specialization, theoretization, reduction} then there exist theory-elements T and T' such that $T\alpha T'$ and neither $T\beta T'$ nor $T'\beta T$.

Concepts from §9

D19 If $E = \langle \mathcal{N}, < \rangle$ is a theory-evolution then X is a *paradigm for E* iff there exist K_0, I_0 such that

- 1) $X = \langle K_0, I_0 \rangle$ is a theory-element;
- 2) $\wedge Y \in \mathcal{N} \wedge \langle K, I, SC, h, F \rangle \in |N|^6$:
 - 2.1) K is a core specialization of K_0 ;
 - 2.2) $\wedge x \in I \vee y \in I_0$ ($y \subseteq x \wedge SC$ acknowledges y to be a paradigm subset of x).

D19 allows one to ‘construct’ for a given theory-evolution a paradigm X as a kind of ‘theoretical entity’. It is unnecessary to mention the ‘founders’ of the theory in connection with the paradigm. X need not even be a theory-element occurring in one of the nets of E ! Rather, X is built for a given E by finding a core K_0 , general enough to have all the cores of nets of E as specializations, and by lumping together suitable intended applications of the theory-elements of E into a set I_0 . **D19-2.2)** prevents the construction of a paradigm in a purely formal way.

D20 E is a *Kuhnian theory-evolution* iff

- 1) E is a theory-evolution;
- 2) $\vee \langle K_0, I_0 \rangle$ ($\langle K_0, I_0 \rangle$ is a paradigm for E).

6 This time, $|N|$ is, of course, the first member of Y .

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