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EXPLANATION, REDUCTION, PROGRESS

Investigations into the Unity of Physics

(Second Version)

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Contents

Ch. I Introduction

1. Key Concepts

As you all know the title of this seminar is "Explanation, Reduction, Progress". Quite a mouthful! So naturally I came to think in terms of the initials ERP. Then I realized that they were a permutation of that very famous abbreviation EPR that now stands for a fundamental issue in the foundations of quantum mechanics based on a paper by Einstein, Podolsky and Rosen. This unanticipated association has raised my hopes that my efforts, too, will excite some interest. I shall indeed be discussing the three concepts and their interrelations most of the time. Most but not all the time. For though I find each concept interesting in itself, interest in them alone or combined increases greatly when they are related to a fourth concept: the concept of the *unity of knowledge*. There are people, scientists as well as philosophers, who don't believe in the unity of knowledge and, consequently, are not interested in that concept. But surely you will agree that *if* one is interested in that concept one is likely to rank it *above* the concepts of explanation, reduction and progress in importance because interest in the latter arises out of interest in the unity of knowledge and *not vice versa*. And this indeed is exactly the way I feel about the matter myself. I have tried to express the point by giving this seminar the subtitle "Investigations into the unity of physics", signalizing thereby the subject matter for the sake of which I shall discuss the collection of concepts I have abbreviated ERP.

Let me expand a bit further on the overriding importance of the concept of the unity of knowledge. It dominates because one naturally thinks of explanation, reduction or progress as *means* of increasing the unity of our knowledge. In contrast the unity of knowledge can only serve as a goal: It makes no sense to make the unity of knowledge a servant of explanation, reduction or progress. Of course, any one of ERP *can* be considered a goal of inquiry in its own right. For instance, one can declare, as Popper did, that the aim of sience is to give explanations of whatever we consider worthy of explanation. In this case one simply abandons consideration of the unity of knowledge. But if one views the unity of knowledge as the final goal of inquiry then one cannot fail to consider ERP. Let me approach the matter at issue in more detail by considering in a preliminary way the *interrelations* of the concepts in question. It goes without saying that in these lectures their range of application is science: I shall be talking exclusively of scientific explanation, scientific reduction, scientific progress and the unity of scientific knowledge. Indeed, most of what I have to say will be confined to a single scientific discipline, namely physics. But for now, I shall be concerned with science in general, not thereby excluding physics.

Nowadays it is commonplace that the reality of science lies in its *history*. Since the revival of interest in the history of science in philosophical circles it has been suggested repeatedly that the development of a relatively autonomous scientific discipline proceeds in a manner analogous to the development of a closed physical system.[1] Given a scientific discipline this means that we must point out the parameters characterizing the momentary *state* of the discipline. Important state parameters would be, for instance, the theories developed within the discipline and still held to be viable, the constants occurring in these theories and not yet explained or even unexplainable within the discipline, the experiments, measurements and observations relevant to the empirical asessment of the theories, and so forth. In addition to these parameters there are important relational parameters characterizing the interrelations application), interrelations between the theories and the experiments, interrelations between the theories of experiments and the theories to be tested by the experiments, and so on.

All of these parameters are involved in the characterization of a momentary state of a discipline. Now some of these parameters may very well reflect the history of the discipline up to the point in time to which the state belongs. This becomes more evident when one considers the *development* of the discipline. Such a development can be viewed simply as the change of the momentary states, and it is natural therefore, to investigate the elements characterizing, not the states themselves, but the change of states. I say: the elements, because the whole matter gets considerably more complicated if it now comes to overlook not only the already fairly complex network of theories, experiments, etc. characterizing a momentary state but also the possible changes of this whole network. One would, therefore, hope that these changes can be analyzed in terms of relatively *simple elements* such as the element that one theory has got a successor in the later state, everything else remaining unchanged, *or* the element that one theory has received striking confirmation from a newly performed experiment, again everything else remaining unchanged, and so on. So though we hope that an analysis of this kind will be "adequate" at least as a first approximation to what really happens you will realize, I'm sure, that we are here dealing with a matter, just as in physics itself, in which strictly speaking everything is related to everthing else, and thus that the elementary changes may have repercussions in the whole system and, therefore, may not turn out to be as simple as was assumed. The situation is not different here than anywhere else in science: to achieve some understanding at all, one must keep the matter as simple as possible. In the worlds of Hermann Bondi, we must try to say *something* without knowing *everything*.

Something we certainly do want to say is that some of the changes in the state of a discipline constitute *progress*, and others do not. The concept of progress depends on what characteristics, what properties of changes of states we decide to count as an advance and, consequently, what changes of state we are willing to view as consituting progress. One answer to this question is that changes of state resulting in a *gain in theoretical unity* constitute progress. The one-sidedness in the relation of these two concepts is immediately evident: The idea of theoretical unity provides the opportunity to fill the idea of progress with content but not *vice versa*. It is true that we still have to say what we mean by a gain in theoretical unity. But however it is defined the concept of progress must not occur in the definiens. By contrast, an acceptable definition of progress *may* contain the idea of unity. And it is this definitional asymmetry which explains the fact that though we are interested in progress because it can increase the degree of unity of a discipline, we are not interested in theoretical unity because it constitutes progress. To think otherwise is to assume tacitly that there is such a thing as "progress in itself" or "progress as such", and this, I think, is a mistake.

The same argument applies were we to substitute *reduction* or *explanation* for theoretical unity. For there is a conception of progress containing reduction or explanation as a vital ingredient: If in the development of a discipline we succeed in reducing one of its theories to another one or in explaining the former by the latter then we are certainly prepared to call this progress. Not that everybody would accept this conception as a matter of course. Controversy over whether we have thereby caught hold of the "essence" of progress may still arise. Nevertheless everbody accepts explanation and reduction as reasonable candidates for progress in science. But the converse would not be true: a definition of reduction or explanation *in terms* of progress is quite unacceptable.

We have seen that explanation, reduction and unity may be made part of the meaning of progress but not *vice versa*. Thus progress is the *least elementary* concept in this field. If we now ask for the *most elementary* concept, the one that can be made part of the meaning of each of the others and not *vice versa*, then I think the concept of theoretical unity is the winner. This is certainly not as evident as the corresponding assertion that the concept of progress is the least elementary concept. Consider, for instance, reduction and unity. At first glance, one might be tempted to ascribe theoretical unity to a discipline if and only if all its theories can be reduced to one of them. But such an ascription, which contradicts the claim that theoretical unity is the most elementary concept, is unfounded unless we are told what "reduction" means. For the meaning of "reduction" is sure to make reference to the idea of the unification permitted by the reducing theory. The idea that reduction involves unity in some sense is a much neglected topic which deserves more attention than it has received by philosophers of science. What would have been achieved by the definition just given if the unifying theory were nothing but the *conjunction* of the theories to be reduced? All would agree with the rhetoric of Bosanquet's question: "Is there any man of science who in his daily work, and apart from philosophic controversy, will accept a bare given conjunction as conceivably ultimate truth?" Here the idea of unity is closely related to that of ultimate truth. And certainly the criterion of conceivably being an ultimate goal of science also would clearly separate the concept of unity from the other three. But even the more modest consideration of what could (or should) be defined by what distinguishes the concept of unity as the most elementary, necessarily appearing as part of the meaning of reduction and, similarly, explanation.

To sum up, ERP and theoretical unity can be arranged on three levels of generality according to the schema

progress
explanation, reduction
theoretical unity

where generality decreases from top to bottom. Secondly, like many others, I have suggested that these concepts are best viewed as aspects in the *development* of science, this development being conceived, in analogy with physics, as a change in the momentary state of a discipline. Finally, I indicated that this change may be enormously complicated and that, therefore, drastic simplifications are absolutely necessary. This last point deserves reemphasis because the concepts in question are likely to conceal it. Indeed the most common illustrations of, say, reduction are at the same time the most difficult. The term "physicalism", for instance, entertains the reduction of psychology to physics and with it the solution of the mind-body problem. The term "reductionism" used without qualification, is frequently understood to denote the position that biology is reducible to chemistry and physics, and thus a solution to "the riddle of life". Moreover, there are also the more modest but still fairly pretentious examples afforded by the reduction of chemistry to physics alledgedly accomplished by quantum mechanics, and in our century the attempted reduction of mathematics to logic.

The frequent use of the term reduction in these examples, sometimes even for introductory purposes, conceals the complications precisely by applying the term in question to the most difficult cases we know of, exciting thereby the impression that we are already in a position of virtual mastery vis á vis our knowledge not only with respect to the developmental stage of those disciplines but also with respect to the philosophical problem of how to explicate adequately the concept of reduction. Such an impression, however, is far from truth.[2] It will become clear in what follows that the topic of concern of these lectures, despite the interest it deserves, is still full of unsolved problems and that there is still a wide gap between attempted explications of ERP and unity and conceptual analysis of the situation on the one side, and the complexity of the situation itself on the other.

2. Survey of existing theories

There are at least three theories about the unity of science worth discussing vis à vis the present situation in science. And there is one adverse theory, implicitly or explicitly opposing each of the former. There is

1) the idealistic coherence theory of (truth and) the unity of knowledge, lately and most vigorously defended by Brand Blanshard,[3]

2) the unity of science program of logical empiricism, officially stated in the Vienna Circle manifesto of 1929 and henceforth promoted by some members of the Circle and other logical empiricists,[4]

3) C.F. von Weizsäcker's transcendental approach to the unity of physics starting out from Kant and drawing heavily on the recent development of physics, esp. quantum theory in its Copenhagen interpretation.[5]

The scope of these three enterprises are 1) knowledge in general, 2) science and 3) physics, respectively, and there is an obvious diminuendo in the corresponding claims. A fourth epistemology often mentioned, which opposes presumably even the weakest of 1)-3) is

4) Feyerabend's radical theoretical pluralism developed mainly in opposition to the empiricist's position 2).[6]

I shall review these four views concerning the unity of science in some detail in Ch. II.2.

Besides theories of the unity of science there are theories of *progress*. According to the foregoing analysis, each of the theories 1)-3) is associated with a theory of progress. To repeat, since unity of knowledge has not been attained even in physics useful theories of the unity of (possible restricted) knowledge have to include theories about how unity is approached. Thus we have

A) unitarian theories of goal-directed progress,

it being understood that the *goal is unity* and that the theories about unity in question are the theories 1)-3) above. There is, however, a second branch of goal-directed theories of progress which make use of the concept of *truthlikeness*. The curious thing about these theories is that, although the goal is Truth, it is Truth in the sense of nothing but a complete and correct description of one particular state of the universe. Therefore these

B) theories of progress by (increasing) truthlikeness[7]

though somewhat in the spirit of logical atomism, are *not* theories of the unity of knowledge. By contrast, the idealistic theory 1) is essentially equivalent to a coherence theory of truth.

Besides theories of goal-directed progress there are theories of what I will call *local progress*. All theories of goal-directed progress make use of the idea of local progress as a *necessary condition*. But they would not *define* progress by local properties. The theories now to be mentioned usually reject the idea of an ultimate unity or even an ultimate goal of knowledge, even when restricted to a limited domain. But they do accept the idea that the scientific enterprise has local successes: From time to time scientists come to agree that a new theory has definitely superseded a theory that had been developed and confirmed some time in the past. It is difficult to classify existing theories of loacal progress. So the following classification is only tentative and will be polished up in due course. In particular, the conceptions of local progress underlying the three theories below are not mutually exclusive. They are

C) the physicist's conception of progress[8]

D) theories of the Popper/Lakatos type[9]

E) theories of progress by reduction (or: explanation).[10]

As I said, there is some overlap between any two of these views, partly due to the fact that each of C)-E) comprises not just one but several different theories.

I can illustrate this last claim in the case of E) simply by listing the major *theories* of reduction that I shall be concerned with during the seminar. First there is

a) reduction by deduction (proper or approximative)[11]

including hypotheses about the relations between the universes of discourse of the theories involved as done, for instance, in so-called microreductions. Secondly we have what I wil call

b) reduction by explaining the explained.[12]

For the time being let me only say of this description that, whereas in deductive reduction the theory to be reduced is *itself* explained in reductions of type b) the reducing theory can only explain some of the things that can be explained by the reduced theory. So, compared with a) it is a *partial reduction*. But there are, of course, other generalizations of a) that could be called partial. This is also the case for the two remaining approaches to reduction, namely

c) the structuralistic approach to reduction[13]

and

d) Ludwig's approach to reduction.[14]

Whereas c) is part of a new movement in the philosophy of science, called "structuralism" by Stegmüller, the approach d) is due to the German theoretical physicist Günther Ludwig. As in the case of b), these approaches are generalizations of the deductive approach a), invented to cover cases falling outside the reach of a).

To recapitulate the major aims for this seminar, I want to convince you in the *first* place that despite recent influential movements pointing in the opposite direction, the idea of the *unity of science*, its meaning, its desirability and its realizability, is an important object of study. Partly for reasons of personal competence and partly because of the current research situation in the discipline I shall restrict most of my discussion to physics. Secondly, since the unity of science, indeed even of physics, is not a fact, let alone a logical fact, it cannot be reasonably discussed without introducing the idea of the *development of science*. This does not mean, either here nor elsewhere that, for instance, the expression 'the unity of science' contains as part of its meaning the concept of time. It only means that, since the unity of science is a goal, its analysis is greatly expedited by concepts which, though not temporal concepts themselves, can be given interpretations describing the *progress* toward the goal of the unity of science. Therfore a *secondary* aim must be the consideration of the concept of progress in so far as it includes advances toward the unity of science. I have already argued that talk of progress in itself is not much more than sloganeering. Even Lakatos, so possessed by the notion of progress that he gave it the dignity of a demarcation criterion, has to tell us *what* he means by it. Since one cannot be sure that reduction or explanation are the *only* kinds of progress toward the unity of science, progress must be further examined. Thus in Ch. III an account of the physicist's conception of progress is provided, an account which does not refer either to reduction or explanation. Even so, it is the notion of reduction and explanation that will occupy me most of the time: so the third goal is to survey the most important explications of these concepts insofar as they bear on the main task. I shall also compare them, and bring out their respective merits and demerits.

Some remarks concluding this section are in order concerning the question why it could be worthwhile for you to participate in this seminar in so far as it gives you the opportunity to learn something that you could not learn otherwise, for instance, by reading a book. *First* I want to give you an *overview* of the matter that, to the best of my knowledge, you could not find elsewhere. Though not complete, this overview should enable you to fill out the details. *Second*, my overview will make explicit *the view of physicists* on the topic of concern lest you think this to be a matter of course, I can tell you that it is not. The majority of the philosophers of science do not even seem to know of the existence of such a view. It is the high time to remedy this neglect, and I intend to make at least a first step. *Thirdly* I shall include contributions from some of my fellow countrymen that may not be known to you and that I think worthy of your attention. For instance, there is the work of the German physicist Günther Ludwig, especially in his book "Die Grundstrukturen einer physikalischen Theorie". A second example is epistemological structuralism. Although this movement started at Standford, thanks to the thorough collaboration between Sneed and Stegmüller it seems to have born more fruit in Germany than in this country. Finally, there is the work of my teacher C.F. von Weizsäcker. Whereas the contributions coming from Ludwig as well as from Stegmüller and his school will concern us mostly in connection with questions of reduction, the work of Weizsäcker has the unity of physics as its central theme.

3. The Method: A plan for rational reconstruction

In the previous section I presented a map of the landscape. To be sure, a map does not show which route one shall take nor the means of transportation. Indeed I have said already that I will not follow strictly the systematic order of the subject as I have laid it out. It will be sufficient to keep an eye on it from time to time just as one would use a map on a trip, looking up to see where one is whenever necessary.

The means of transportation is crucial. The question is: What *method* is most appropriate to the ultimate purpose? I do not intend all the time to declare what *I* think the best solutions to the problems I will discuss are. On the contrary, most of the time I shall be occupied with introducing the most important views and theories that *others* have held on the unity of science, on reduction and the other themes already reviewed. But then, one is not free to choose just any old method of presentation. For in giving a true account of some other person's view it is better to adopt that person's own method of treating the subject and presenting the results. Nevertheless, it may facilitate understanding if I say what method I *would* prefer if I *were* to give you a purely systematic account. For one thing, on several occasions

I will indeed present my own ideas, and even when discussing other people's views it can hardly be avoided that one's own ideas will emerge if only sotto voce. Secondly, the philosophical method I would recommend on a given occassion will not differ greatly from the method actually followed by some, if not most, of the authors to be discussed. Finally, since other authors have expressly rejected the method I will employ vis à vis the current subject matter, that is ample reason to unpack it right at the beginning.[15]

There is nothing peculiar about my preferred method of investigation: it is simply the method of *rational reconstruction*, and in some important special cases rational reconstruction *by logical analysis* or - equivalently - *logical reconstruction*. Even this does not point to a unique framework for reconstruction. For in some cases it may be legitimate to ask *which* logic is to be applied. So you see that in talking about and defending *reconstructivism* as a philosophical methodology I am inclined to leave room for different kinds of reconstruction according to the framework *within which* we reconstruct given material. As we shall see, in general there are other parameters of a reconstruction that can be fixed in different ways - parameters other than the reconstruction framework -, but the latter seems to be the most important one.

The idea of reconstruction, as I conceive it, is, therefore, rather undemanding. But it allows us to focus on one principle, of reconstructionism, its major principle if you will. That principle is an anti-totalitarian, relativistic principle rejecting absolutism in matters of interpretation or meaning. Given some material from science, e.g. some scientific theory or concept, the principle warns us to be suspicious of any claim to have developed *the* correct interpretation of the theory or concept. Rather it recommends that we look for an appropriate framework, and give a more or less explicit reconstruction of the theory or concept *within that framework*. Having done this the *only* claim that can justifiably be made is a *conditional* one: *If* one chooses such and such a framework, *then* the theory or concept comes out as so and so. Even if reconstructionism is accepted as methodology, there can still be quarrel about *which* reconstruction framework should be chosen. And there can certainly be opposing views as to the question whether science is in need of *any* reconstruction whatsoever. As to the latter question I shall argue presently that reconstruction is a matter of degree: Science itself has already started it, and this is one of the reasons why there is no sharp borderline between science and philosophy. As to the first question, problems caused by the relativism that is implied by reconstructionism have at least the advantage that they can be rationally discussed because the rival reconstruction frameworks have been made explicit. By contrast, absolutism in this respect is *the* very source of endless and futile disputes about hidden implications and other things deliberately left unclear and vague.

It was ideas like these that Carnap and others had in mind when they started reconstructionism in the late twenties. In his famous principle of tolerance Carnap came close to formulations of reconstructionism as I have just suggested them.[16] But since the freedom that was decreed by this principle was not really used and classical first order logic became the standard framework of reconstruction, the reconstructionism of logical empiricism soon displayed a special kind of absolutism and was criticized on just that account. Remember that in his 'Logical Construction of the World' of 1928, Carnap described his enterprise as being "a rational reconstruction of the entire construction of reality as it is intuitively performed in usual cognizance." Somewhat more precisely, but still quite generally, Carnap says that his constitutional system "shall not describe the process of cognizance in its actual constitution, rather it shall rationally reconstruct its formal structure."[17] Similarly, Reichenbach put the matter in his book 'Experience and Prediction' as follows:[18]

> Epistemology does not regard the processes of thinking in their actual occurence, this task is entirely left to psychology... Epistemology... considers a logical substitute rather than real processes. For this logical substitute the term *rational reconstruction* has been introduced... It is..., in a certain sense, a better way of thinking. In being set before the rational reconstruction, we have the feeling that only now do we understand what we think.

This was the position in 1936, and decades later Carnap reaffirmed his old conception. He said that in a rational reconstruction "we search for new determinations of old concepts. The old concepts usually are not created by deliberate formation but by spontaneous development. The new determinations shall supersede the old ones in clarity and accuracy... Such conceptual clarification still seems to me to be one of the most important tasks of philosophy."[19]

At the time Carnap wrote the preceding remark reconstructionism began to come under fire from the anti-unity of science movement that I mentioned in the first section. These critics held that science as reconstructed by logical empiricism no longer had anything to do with science as it develops in real history. Reconstructed science was science distorted beyond recognition. "Carnap's system of inductive logic - writes Toulmin[20] - was expounded not in terms of real life examples but in a formalized logical symbolism whose relevance to actual scientific language was always assumed, never demonstrated." Similarly, Th. Kuhn[21] wrote that "the reconstruction of science by philosophers, grounded on textbook presentations and, at best, some historical classics of natural science generally is recognizable as science neither for the historian of science nor for the scientist himself." It is interesting to observe that the *reaction* of reconstructionism to such criticism has not been to argue the principle of the matter. No honest effort has been made to defend reconstructionism by a thoroughgoing general reconsideration of its principles. As is evident in the work of the structuralists, the only reaction was to assimilate existing kinds of reconstructions to some of the more important specific demands that arose from the anti-unity of science critique.

I do not intend to present a fundamental, all encompassing defense of reconstructionism in these lectures. A few elementary remarks and one argument in its favour, both on a quite general level, will suffice here. First of all, the word *'reconstruction'* can refer either to a process - the process of reconstructing something - or to its result. This harmless ambiguity will be resolved by the context of the discussion. Moreover, there is the *original* subject matter to be reconstructed, the *framework* within which the reconstruction is performed, the *context* in which the original subject matter is located, the *principles* according to which the original subject matter is reconstructed and, finally, the *relations* in which the reconstruction stands to the original subject matter. As an illustration consider first an example outside the scientific world. The painting of a landscape or the resulting picture is a reconstruction, the real landscape being the original subject matter and nature its context. If performed by a professional artist the painting will obey certain principles of art telling the painter how to transpose a real piece of nature into a piece of real art on the canvas. Finally, there will be relations between the real landscape and the picture, similarity being a rather trivial one.

Reconstructing a scientific theory or an alleged reduction of one theory to another or whatever, in general is not essentially different from what the painter does. This, of course, is but another way of saying that what has been said so far is not very specific. But it will be sufficient in order to understand the following argument because, in a sense, it is a quite general argument. The argument is an *argument from continuity*, and its purpose is to show that science and philosophy of science are continuously connected in such a way that we can draw no borderline between philosophical reconstruction and scientific subject matter. Hence it would be completely arbitrary to isolate certain parts from the whole of science, logic and philosophy as being irrelevant to the rest. We can draw lines starting out from the concreteness of our sense impressions up to the abstractness of logical inferences, and we could stop at any given point with the same right as at any other.

If, for instance, we begin with our *impressions of heat*, they have found reconstruction in measuring instruments for *temperature*. Logical empiricists express this by saying that our common concepts warm and cold have been explicated by the concept of temperature. But this has little to do with philosophy. Rather it is *experimental physics* to which this reconstruction (or: explication) is due. Next comes theoretical physics telling us that temperature is to be identified with the *mean kinetic energy of molecules*. But it emerges that the kinetic theory of heat is connected with various other theories allowing one to measure extremely high or extremely low temperatures: we have got, that is, explications by *indirect measurements*. *Calculation* enters the picture, and now we are forced to consider the role of *mathematics* in physics, i.e. the reconstruction of the *physical* theories in question necessarily involves a reconstruction of mathematics *and* the way it is applied in those theories. Finally, there is no indirect determination of temperature or any other physical quantity without applying some piece of logic. For the calculation of the value of a quantity that had been measured indirectly is not possible without drawing inferences.

Where, I ask you, is there a natural stop in this series of reconstructions before arriving at logic. Mankind has existed for a long time without using a thermometer, even today we have experimental physicists who look at theories only with great skepticism, and again and again one encounters theoretical physicists who tolerate only the most sparing use of mathematics. Finally, many mathematicians from the time of Descartes up to this day will hear no talk of logic. So when it comes to efforts by philosophers of science to give certain metatheoretical concepts a precise logical status, nothing unheard-of happens, nothing that is in special need of justification as opposed to anything else occurring in our story. There is no sharp borderline between science and philosophy, and much reconstruction's labour is done already by men who would not dream of calling themselves philosophers.

Summing up, my *methodological credo* is rational reconstruction *if* it is done with sufficient care and awareness of what the problem to be solved by it demands of it. I readily admit that, though I could say more about reconstruction, I am unable to give an explication of a concept that in turn is meant to designate just *this* process. Many of the writers to be discussed are themselves reconstructionists, but some are not. Among the latter are the adversaries of reconstructionism. But most of their arguments are absolutistic. They argue: This or that is not an adequate reconstruction of science. But the appropriate question to be answered is: Is *this* a reconstruction of a piece of science within *that* reconstruction frame? The adversaries, therefore, do not acknowledge or are not even aware of the possibility that the latter, the relativizing question, is a question to be asked and answered *in its own right*. It is a question worth asking *for itself* whether, for instance, quantum mechanics can be reconstructed on the basis of classical logic. *It goes without saying* that such a reconstruction would far exceed what a normal scientist would accept as a complete formulation of quantum mechanics. But philosophy of science would be dull if it did nothing but *repeat* science. In some important respect it *must* go beyond it, and precisely this is done in genuine reconstructions very much like the painter transcends the landscape.

Notes to Chapter I

1 Cf. Siemens [1971]

2 This has recently been emphasized by Hans Primas, see his [1981], Chs. 1 and 6

3 Blanshard [1939], esp. Chs. XXV - XXVII

4 Vienna Circle [1929]

5 Weizsäcker [1980], Part II

6 Feyerabend [1975]

7 Niiniluoto [1984]

8 See Ch. III

9 Popper [1963], Ch. 10; Lakatos [1970], §2 (c)

10 See below

11 See Ch. IV

12 See Ch. V.3

13 For lack of time the treatment of this approach had to be skipped

14 See Ch. VII

15 Cf. Scheibe [1984a]

16 Carnap [1937], § 17

17 Carnap [1928], pp. 139 and 191

18 Reichenbach [1938], pp. 5 f.

19 Carnap [1961], p. IX

20 Toulmin [1972], p. 62

21 Kuhn [1977], pp. 65 f.

Ch. II: Diverging Views on the Unity of Science

1. The Scientists

In the preceeding chapter I did not argue for the unity of science: Rather I took it for granted in order to see how it is related to ERP *if* assumed to exist at all. The review in this chapter will show that the unity of science issue is quite controversial not only among philosophers but also among scientists. So let me begin with the scientists and classify their views as well as the circumstances allow.

First there is the view that the unity of science is *methodologically desirable*, that it guides profitable scientific research and is pursued almost as a matter of course. This view is expressed, for example, in two papers by Max Planck[1]. In 1908 he said: "At all times natural science has seen its last and highest aim to be the comprehension of the many-coloured manifold of physical phenomena into one unifying system or even one single formula..." Similarly, in a paer of 1915, he wrote "The major aim of every science will always be the amalgamation of all its theories into one single theory in which all problems of that science find their unique place and their unique solution." The demand for unity by scientists throughout history was, according to Planck, an immensely successful one. The history of physics, Planck says, shows that "the number of separate fields of research in physics has been considerably reduced by the amalgamation of related fields... The characteristic of the whole development of theoretical physics is a unification of its system..."

A similar and more recent statement is expressed by the theoretical physicist Fritz Rohrlich[2]. In his monograph on "Classical Charged Particles" he frankly declares the "aim of theoretical physics [to be] the striving for the construction of more and more inclusive physical theories and the exploration of their ramifications." Apart from the question of desirability Rohrlich believes that, as a matter of historical fact, "progress in theoretical physics... clearly points toward the eventual construction of an *allinclusive* theory..."

Let me briefly turn to a *second* view about the unity of science frequently found in the writings of members of the Copenhagen School. Perhaps the briefest statement of this view is expressed by C.F. von Weizsäcker[3]: "physics - he says - develops from unity via plurality toward unity." This statement of the view will rise again in the next chapter. But let me quote the two most eminent members of the Copenhagen School. In a speech on the "Unity of Knowledge" Bohr said:[4]

> As our starting point we must realize that to begin with, all knowledge has to be expressd within a conceptual framework well adapted to the description of previous experiences and that every such framework in the course of time may turn out to be too narrow in order to comprehend *new* experiences. In many regions of knowledge scientific research has shown the necessity to drop viewpoints that, according to their proliferation and apparently unlimited applicability, originally have been viewed as indispensible for consistent explanation. Although this development starts from special investigations it contains a general lesson that is important for the unity of knowledge. The extension of the conceptual framework has not only created order *within* the various branches of science but has also revealed similarities with respect to our position in the analysis and synthesis of experiences belonging to seemingly *separated* regions of knowledge. It thereby has shown the possibility of an ever more comprehensive objective description.

The development of gradually extending conceptual frameworks interrupted by, but every time recovered from, phases of a plurality of disordered experiences, has been described in greater detail in an article of 1942 on "the Unity of the Scientific World View" by Heisenberg[5]. It culminates in the statement that "by [the new] atomic theory physics and chemistry have been amalgamated into one unified big science." And although Heisenberg concludes the paper with the confession (in 1942!) that "a real amalgamation of [physics, chemistry *and* biology] into a conceptual unity could be achieved only by very principal extensions of our knowledge about life processes", he leaves no doubt that he believes in the real possibility of such extensions.

A *third* view about the unity of science is the most optimistic one. In these statements the unity of science appears not only as a methodological demand or as gradually being fulfilled in larger and larger parts of science in a possibly unending process, but as the claim of a *definite completion*, if not of all science, then, at any rate, of some well defined, basic and fairly extended part of it, for instance, physics. One who firmly believes in the definite completion of physics is C.F. von Weizsäcker[6]:

> Assume that the theory of elementary particles, including also the theory of gravitation, has been completed - and couldn't this easily be the case still in our century? Then, at least in the domain

> we today call physics, no special law of nature whatever would be left - no law theoretically underivable, in principle, from the basic law.

Similarly, in his Inaugural Lecture of 1980, entitled "Is the end in sight for theoretical physics?", Stephen Hawking[7] discussed "the possibility that the goal of theoretical physics might be achieved in the not too distant future, say, by the end of the century." This goal he describes as "a complete, consistent and unified theory of the physical interactions which would describe all possible observations." And although Hawking warns us "to be very cautious about making such predictions" he sees "some grounds for cautious optimism that we may see a complete theory within the lifetime of some of those present here."

So here is a series of statements of growing optimism about the desirability and realization of the unity of science, a series that could be continued for a considerable period of time. Let me add, however, that this fact has little import for the question of the eventual unanimity of the whole scientific community on the matter. For one thing, all quotations so far presented are from physicists, *not* from chemists or biologists. Second, the idea of the unity of science is the very kind of a theme where men like Planck, Bohr, Einstein etc. do *not* speak for even the most narrowly chosen scientific community to which they belong. Rather it is a theme on which everyone, however impressed one may be with the idea, can only speak for himself. The votes in favor of the unity of science (or physics) probably do not exceed the number of its declared advocates. Moreover there is a silent majority probably indifferent about the issue. Finally there certainly are contrary voices. Here are three examples, one a physicist, one a chemist and one a biologist.

In his paper "The Lure of Completeness" the cosmologist Hermann Bondi[8] argues that "where there are empirical reasons to join together previously separate branches then this is a worthwhile enterprise likely to lead to important insights, but where there are no such indications one is probably only indulging in a mathematical game rather than in science." Indeed, according to Bondi, it is a game played by the most eminent minds in physics: "Einstein, Eddington, Schrödinger and... Heisenberg have aimed for 'world equations' giving a complete description of all forces in the form perhaps of a 'unified field theory'. A vast number of hours and indeed years of the time of these towering intellects have been spent on this enterprise, with the end result... of precisely zero." For Bondi "science is by its nature inexhaustible" if only for the reason that new technologies become available for experiment and observation leading to new discoveries not dreamt of by those who dream of a completed physics.

Mind that Bondi's objection is not directed against empirically motivated, local reduction or unification. He argues only against the global idea of a completed physics. Indeed the idea of unending reduction chains is logically possible although the idea that this could prove anything may be a bit too playful. But there are also objections to less expansive statements, for example, the statement that one discipline reduces, or has been reduced, to another one, or that such reductions are important scientific achievements. These objections usually concern the 'big cases': the reduction of chemistry to physics and of biology to chemistry and physics. Thus the physico-chemist Primas[9] warns us that:

> Reductionism, if accepted, is usually accepted on faith and without logical evidence or sound reasons. Overblow claims in the philosophical literature for the reducibility of chemistry to physics are not justified by present scientific knowledge. Most theoretical concepts of chemistry have not yet been successfully reduced to quantum mechan-

> ics, and it is an open question whether such a reduction can always be achieved.

Similarly, the biologist Ernst Mayr[10] tells us that "he is not aware of any biological theory that has ever been reduced to a physico-chemical theory. The claim that genetics has been reduced to chemistry after the discovery of the structure of DNA, RNA, and certain enzymes cannot be justified." Moreover, "attempts at a 'reduction' of purely biological phenomena or concepts to laws of the physical sciences have rarely, if ever, led to any advance in our understanding. Reduction is at best a vacuous, but more often a thoroughly misleading and futile, approach."

Occasionally one finds scientists disappointed not only with the sometime extreme skepticism often expressed about the feasibility of reduction, unification and the like but also with the very *willingness* of those fellow scientists who do indeed believe in ERP to indulge in the kind of business that eventually could lead to reductions, unifications etc. Rohrlich[11] points out:

> I belive that the generation of theoretical physicists who developed relativity theory and quantum mechanics was better educated in philosophy of science than is the present generation. They were acutely aware of the need for philosophical ques-

> tioning if they were to be good theoretical physicists... . Thus, there was little doubt among physicists at the time that different branches of theoretical physics must be interrelated. In particular, they insisted that the special theory of relativity reduce to Newtonian mechanics in a suitable limit; that general relativity reduce to special relativity, that non-relativistic quantum mechanics reduce to non-relativistic classical mechanics, and so forth. The originators of new theories nowadays, expecially in elementary particle physics, do not always pay attention to such matters, and some even doubt the existence of such relations.

And with respect to the doctrine of the reducibility of biology to physics and chemistry the philosopher-biologist Woodger[12] says:

> ...people who hold the doctrine do not in fact believe it. If they did they would not spend laborious hours experimenting in their laboratories; instead, they would spend equally laborious hours in their studies with paper and pencil reducing biology to chemistry and physics, because... reducibility is a purely paper and pencil affair... . From the fact that people do *not* do this, I venture the guess that they confuse *reducibility* of biology to physics and chemistry, with *applicability* of physics and chemistry to biological objects. The truth of the latter is open to everybody to see, the truth of the former no one knows... .

This review of the varied opinions of some scientists on the unity of science is rather convincing evidence that it would be very difficult, if not impossible, to spell out something that might be called *the* view of the scientists on the matter, except for the general belief that the unity of science is not an accomplished fact. Beyond this the common denominator of opinions is perhaps that there is a certain methodologically effective *unifying tendency* in science. It proceeds from the lowest level where various phenomena are subsumed under one empirical law and is possibly continued on higher levels by the unification of lower level theories into theories of greater universality. Anything going *beyond* this somewhat vague hierarchical structure would, I think, still be controversial, whether it be a 'simple' question of fact - is chemistry reduced to physics? - or a question as to the future attainability of a completely unified science or branch of science.

2. *The Philosophers*

We are no better off when we turn from scientists to philosohers of science. This is to be expected, if only because once something becomes the object of philosophical reflection, it rarely fails to be controversial. Indeed here we find the controversy extended even to methodology, a subject on which there seems to be some agreement among scientists. This situation prevails in current philosophy of science in spite of the efforts of a great philosophical movement of recent vintage for which the unity of science was not just *one* but rather *the* aim of the collective efforts of scientists *and* philosophers.

This movement - logical empiricism - is the second on my earlier list of views on the unity of knowledge. To keep things in order let me first say something about the *idealistic theory*.

I put the idelaistic view in first place not only because it has the widest scope but also because of its daring and even extravagant ideas and assertions. Normally it is not even mentioned in current philosophy of science. Nor would its advocates regard it as being a philosophy of science, though for different reasons. On the other hand, scientific considerations are not beyond the pale, and especially in Blanshard's work are given due regard. I am not an idealist. But in important respects the coherence theory of truth and the idealistic view of the unity of knowledge can be useful in the current endeavour to find a concept of unity appropriate to science and its goal.

Some impression of what the idealistic theory of the unity of knowledge is like can be gleaned from the following passage in Blanshard's 'The Nature of Thought'[13]:

> ... reality is a system, completely ordered and fully intelligible, with which thought in its advance is more and more identifying itself. We may look at the growth of knowledge... as an attempt by our minds to return to union with things as they are in their ordered wholeness... And if we take this view, our notion of truth is marked out for us. truth is the approximation of thought to reality... Its measure is the distance thought has travelled... toward that intelligible system... The degree of truth of a particular proposition is to be judged in the first instance by its coherence with experience as a whole, ultimately by its coherence with that further whole, all-comprehensive and fully articulated, in which thought can come to rest.

This is a quite extraordinary view, if only because of the strange idea that the growth of knowledge consists in a gradual *identification* of thought with reality. However, the notion of coherence with its insistance on unity and completeness or - equivalently - on wholeness can be useful even for describing features of physical theories and their unification. One has to suspect that the network of our physical concepts and theories is knotted more tightly than it appears. Since - as Blanshard says[14] - "everything is related in *some* way to everything else, no knowledge will reveal completely the nature of any term until it has exhausted that term's relations to everything else." This passage shows, again, the position in question to be dealing with an extreme limiting notion of completed unity of knowledge.

Idealism's conception of completely coherent knowledge cannot be exemplified. But let me show you how it can be made somewhat plausible by well-defined examples from physics. Take Newton's theory of absolute space and absolute time. From a modern viewpoint this is a paradigm of an incoherent theory, - a bare conjunction of two theories referring to two quite different subjects. In modern terms, Newton's space-time is just the direct, cartesian product of space and time. Since Newton galilean spacetime has been developed. In it the concept of space no longer occurs as an independent entity; the corresponding theory of spacetime is therefore not decomposable into two independent subtheories. But the new theory does contain a theory of absolute time as a subtheory built on a proper sublanguage. From special relativistic spacetime time has also been extirpated. So, in 1908, Minkowski could say not unjustly, that[15] "Henceforth space by itself and time by itself shall become degraded to mere shadows and only some kind of union of them shall remain independent." Thus, in physics, if anywhere at all, the kind of unity that our knowledge aims at, in the sense of Blanshard's theory, can at least be approximated.

"The goal ahead is *unified science*". With this sentence from the famous manifesto, "The Scientific Conception of the World"[16], published by the Vienna Circle in 1929, I move to *logical empiricism*, the second conception on my list. The authors of this manifesto made it quite clear that for them the unification of science was not only a *theoretical* but rather an eminently *practical* problem. They wrote:

> The endeavour is to link and harmonize the achievements of individual investigators in their various fields of science. From this aim follows the emphasis on *collective efforts*, and also the emphasis on what can be grasped intersubjectively.

I would welcome this practical emphasis. For if I were ever asked to relate just *one* outstanding experience during my career as a philosopher of science, it would certianly be the following: When I talk to scientists or students of science, I soon realize that my speech is too philosophical, and when I talk to philosophers or students of philosophy I usually make the mistake of assuming too much familiarity with physics. This double experience has had the paralysing effect that when I have to talk to both *I hardly know what to say*. And since such is the rule in Germany where philosophy of science is still rather underdeveloped, I would have been extremely grateful if that practical requirement of the Vienna Circle manifesto had received more attention than it actually has.

At any rate, later statements by members of the Vienna Circle, or their friends, are not very optimistic vis à vis the unity of science. Its further development was most efficiently directed by Carnap. In his article on the "Logical Foundations of the Unity of Science" (of 1938) Carnap summarizes his investigations as follows[17]:

> ... there is at present *no unity of laws*. The construction of one homogenous system of laws for the whole of science is an aim for the future development of science. This aim cannot be shown to be

> unattainable. But we do not, of course, know whether it will ever be reached. On the other hand - Carnap continues - there *is* a unity of language in science, namely, a common reduction basis for the terms of all branches of science, this basis consisting of a very narrow and homogeneous class of terms of the physical thing-language.

In logical empiricism the division into questions of language and questions of law, as expressed in this passage, became standard. Carnap's optimism about the unity of language is grounded in the fact that he sees the problem of the unity of science as an epistemological rather than an ontological problem. He is quite explicit about this when he writes: "The question of the unity of science is meant here as a problem of the logic of science, not of ontology." Consistent with this attitude is the *result* of the earlier quoted argument that the language of science, qua a unified linguistic entity, is not by any stretch the language, say, in which we talk about elementary particles as the ultimate constituents of matter. Rather it is a common *obeservation language* which serves as an epistemological link between the objects of science and scientists. So as far as language is concerned, the unity of science is established not by showing how we can express everything in terms of a theory of elementary particles but by a universal reduction of the language of science to a physical thing-language.

Although this approach, with its basic idea of an empirical basis, is typical logical empiricism an alternative has been laid out in an articel which, in many respects, is written in the spirit of logical empiricism. The paper entitled, 'Unity of Science as a Working Hypothesis' by Oppenheim and Putnam[18] may even be viewed as a statement of account 'after thirty years' of the Vienna Circle manifesto though the authors apparently had no such intention. However, they warn the reaer not to confuse their sense of 'Unity of Science', and related expressions, with the epistemological use of these terms. By the epistemological use of 'unity of language' they mean the (possibly) successful reduction of the language of science to one or the other universal observation language. By contrast, their own idea of a unitary language for science is ontological. To explain their intention suppose a linear hierarchy of levels with elementary particles at the bottom and social groups on top. Then the main question regarding language is this: Can the (theoretical) language in which we talk about the entities belonging to level n be reduced to the language associated with level n-1? Although it is part of their working hypothesis that the answer is in the affirmative, the authors are by no means very definite. The same is true for other logical empiricists, e.g. Hempel who, in the case of biology, repeatedly admits that "the connecting principles that are presently available do not... even remotely suffice to reduce all the laws... of current biological theory to those of current physico-chemical theory".[19]

So much for language. As to the *laws of science* recall that Carnap's assertion that unity has not been achieved. But Carnap does believe that unification of the laws of science can be approached gradually by steps of reductive explanations. In his postumous book on the "Philosophical Foundations of Physics" he points out[20] that "in the history of physics, it is always a big step forward when one branch of physics can be explained by another." After reviewing some well-known examples he goes on to say that: "Slowly the notion grew that the whole of physics might some day be unified by one great theory." From what he then has to say about such a 'great theory' it transpires that he sees it quite in the manner in which men like Einstein or Heisenberg have seen it, namely, as a unified field theory or quantum field theory, the only trouble being that "so far... no theory has been devised that is entirely satisfactory." A somewhat less optimistic view is expressed in Nagel's classical paper of 1949 on reduction.[21] He notes that "with some exceptions, no serious students today believe that some particular physical theory can be established on a priori grounds as the... fundamental theory of natural processes [as had been done in the case of mechanics in the 17th and 18th century]; and to many thinkers it is even an open question whether the ideal of a comprehensive theory which would thoroughly integrate all domains of natural science is realizable." On the other hand, Nagel clearly is a reductionist in the sense that he believes in past reductions as a fact of the history of modern science and sees "no reason to suppose that such reductions will not continue to take place in the future." Nagel thus seems to belong to those half-hearted unificationists who, though believing in successive reductions in science, are prepared to accept this as an *unending process*, for reasons of (undisplayed) principle.

The major contribution of logical empiricists to their own programme was an extensive discussion of the possible mechanisms by which language reduction and theory reduction are effected. Their approach to the unity of science is, therefore, a typical approach via reduction. Science is seen to grow in unity and seldom, if ever, is the idea of unitary theory *itself* discussed. Oppenheim and Putnam's paper characterizes the situation. In addition to Carnap's requirements concerning the unity of language and laws, Oppenheim and Putnam point out a *third* feature that, surprisingly enough, seems never to have received an explication. Yet, as I argued earlier, it is just such a feature which justifies calling science 'unified' or 'unitary' in any *proper* sense. But although mentioning it Oppenheim and Putnam don't have to say too much about it:[22]

> Unity of science in the strongest sense is realized if the laws of science are not only reduced to laws of some one discipline but the laws of that discipline are in some intuitive sense 'unified' or 'connected'. It is difficult to see how this last requirement can be made precise; and it will not be imposed here. Nevertheless, trivial realizations of 'Unity of Science' will be excluded, for example, the simple conjunction of several branches of science does not *reduce* the particular branches in the sense we shall specify.

At least Oppenheim and Putnam point out what must be avoided in unifying science, and one perhaps will not be surprised that it is bare conjunction. More about this later on.[23]

Oppenheim and Putnam do give a fairly detailed survey vis à vis the various disciplines to be unified, and having based their account on a well-known model for reduction they close with the optimistic statement:[24]

> It has been our aim... to provide precise definitions for the crucial concepts involved and to reply to the frequently made accusations that belief in the attainability of unitary sciene is 'a mere act of faith'. We hope to have shown that, on the contrary, a tentative acceptance of this belief, an acceptance of it as a working hypothesis, is *justified*, and that the hypothesis is *credible*, partly on methodological grounds... and partly because there is really a large mass of direct and indirect evidence in its favor.

This statement, and the entire paper in which it occurred, is the last sign of life in the idea of a unity of science as conceived thirty years earlier in the Vienna Circle manifesto.

The 'revolt against positivism' has many facets not the least of which is the idea of the unity of science. One particular branch of the movement has got the title, 'theoretical pluralism', a label which signalizes that the supreme idea of logical positivism is its main target. It would be more appropriate to call this anti-positivistic movement 'radical theoretical pluralism' because there is a purely methodological theoretical pluralism in which the simultaneous development of rival theories is recommended for methodological reasons only: Progress can be accelerated by a pluralistic methodology but after some struggle there will always be a unique winner superseding all its competitors. This pluralism is compatible with even the strongest versions of the unity of science. But the radical or direct form of theoretical pluralism, where not only most of the time honored reductions of science are denied as being proper reductions but it is also recommended that the ideal state of science consists of as many competing theories as possible, *is* diametrically opposed to the concept of the unity of science. To quote Feyerabend, the most prominent advocate of theoretical pluralism:[25]

> The plurality of theories must not be regarded as a preliminary stage of knowledge that will at sometime in the future be replaced by the 'one true theory'. Theoretical pluralism is assumed to be an essential feature of all knowledge that claims to be objective.

A more modest though still anti-unitarian view has been developed by Thomas Kuhn. He does not believe in theoretical pluralism, perhaps not even in its harmless methodological version. In his view a constant plurality of theories would simply paralyse the progress of science. On the other hand, Kuhn does not believe in scientific progress as a goal-directed process. For him it is of no help to imagine "that there is some one full, objective, true account of nature and that the proper measure or scientific achievement is the extent to which it brings us closer to that ultimate goal."[26] He compares the situation with Darwin's theory whose greatest achievement he sees as freeing the idea of biological evolution from a goal, maintaining, however, specifiable local advances that clearly allow to distinguish evolution from arbitrary change. Why not assume something similar also for all science? "... the entire process may have occured, as we now suppose biological evolution did, without benefit from a goal, a permanent fixed scientific truth, of which each stage in the development of scientific knowledge is a better exemplar."

The major challenge of Kuhn and Feyerabend to the concepts of the unity of science and reduction has been, then, the observation that the development of science is marked by the occasional appearance of so-called *incommensurable concepts* and *theories*. In particular, it has been claimed that this can happen even within as highly developed and firmly established a discipline as modern physics. Incommensurability is a particularly intricate kind of incompatibility that is not located in the true-false dimension but rather in the dimension of meaning.

Two theories are incommensurable not if they contradict each other but if, for some strange reason, they cannot be given the same interpretation *even though* they refer to the same objects or at least to objects which would normally be said to stand in part-whole relations. If two theories are incommensurable, for instance, quantum mechanics and classical mechanics, it is very difficult to see how one of them, e.g. classical mechanics, could be reduced to the other one, e.g. quantum mechanics, in any proper sense. In particular, there would be no question of an *elimination* of one of the theories in the sense that it simply becomes a part of the other. Instead either one of the theories is simply *replaced* by the other *or*, because replacement might involve serious losses, both theories are maintained *without* any chance of their eventual *unification*. This is the germ of a pluralistic world view with its many equally important but ununifiable aspects of the universe.

Is incommensurability really a serious difficulty, and if so, is there a way out? Many different answers have been given to these questions. Let me conclude the survey in this chapter by sketching briefly C.F. von Weizsäcker's program for the unity of physics. The program was not originally designed as a way out of the difficulties presented by incommensurabilities. But we shall see it faced them independently of the Feyerabend/Kuhn démarche, indeed long before it.

Weizsäcker, a disciple of Heisenberg, belongs to the Copenhagen School of thought, which for some time has represented the orthodoxy in questions about the interpretation of quantum mechanics. How incommensurability problems were anticipated by this School in the late twenties, I shall discuss in the next chapter. Weizsäcker, who joined the Copenhagen movement in the thirties, accpeted what he found to be Copenhagen solution of incommensurability and felt little need to refuse it. From the very beginning his principle aim has been to explain and defend the unity of physics, and he did not see this idea endangered by incommensurability.

Weizsäcker's program combines elements of Kantian transcendentalism with elements of modern empiricism. To these he adds the conviction that the historicity of all our thinking is very important. The inevitable time constrained character of human thinking and theorizing is - to put it bluntly - a flat denial of Kant's Apriorism. As with so many others, Weizsäcker's denial was provoked by Kant's complete failure to establish euclidean geometry as an *a priori* part of physics by assuming space to be a pure intuition. Weizsäcker's conviction was further enhanced by another deeply held conviction that once the Kantian failure has been exposed, it is hopeless to start running fights on the same front. In the twenties this was done by men like Herman Weyl and Carnap who, nevertheless, still attempted to find an *a priori* foundation for some weakening of euclidean geometry. But their efforts did not get much support.

According to Weizsäcker one is unlikely to find any timeless, *a priori* truths. However, what we *can* do is to integrate at least part of our history and entertain hypotheses about where it leads. If this is done for the history of the natural sciences - perhaps the only case in which it can be done - then the hypothesis that we might gradually reach a unified theory of nature suggests itself. Not now knowing, of course, *which* theory this will be, the question arises whether we have any idea what the final theory *will be like*, - what *kind* of theory it will be? Weizsäcker's answer is yes, we do, and indeed the final theory will tell us nothing but what the conditions of the possibility of experience are, and *this* will constitute its unity. Let me quote von Weizsäcker: "To render this unity of physics comprehensible is the task contemporary physics sets before philosophy. We can refuse the task as too difficult but we cannot... reduce it to a lesser task.

The program that Kant formulated for classical physics will to-day prove either to be unrealizable or to have been realized as soon as self-evident assertions concerning the preconditions of the possiblity of experience have led to the construction of precisely that unified physics at which the contemporary development so obviously aims."[27]

Now, seen in the light of Kant's transcendentalism, the remarkable thing about the position under discussion is that it takes up Kant's fundamental idea of the conditions of the possibility of experience *without* being committed to view that these conditions are known *a priori*. Moreover, not only do we find these conditions only in the course of experience, their complete system will not be known until physics is *finished*, and then it will be known as the system of its fundamental assumptions. Weizsäcker's position may therefore be called an "independent transcendentalism", it is a transcendentalism independent of the kantian binding to apriorism.

It is an empirically based transcendentalism. The preconditions of the possibility of experience are looked at in the manner in which Peirce looked at truth. Just as for Peirce truth was that "opinion which is fated to be ultimately agreed to by all who investigate" the preconditions of experience are viewed as the final result of basic physical research. In both cases it is, of course, allowed and possible to make hypotheses at any time not only about the kind of thing to be approached but also about its details. In Weizsäcker's case quantum theory is exploited in order to fulfill as much of the program as is possible at present.

It is obvious that independent transcendentalism implies or perhaps even *is* some kind of what conventionally is called *reductionism*. Moreover, it seems to imply a very strong reductionism that goes far beyond what *mutatis mutandis* Kant aimed at. As is well known, Kant's critical philosophy is characterized by a certain indecision as to how much of physics could (or should) be deduced from general principles of understanding. By contrast, Weizsäcker is quite explicit about what a final theory of physics in his sense would have to achieve: "Such a theory would have to allow us to deduce, in principle, the... structure of the Lorentz group and of quantum mechanics, the existence and number, the masses and interaction constants of the... elementary particles..., each and every line in the spectrum of iron, and the laws of celestial mechanics. Here we are not allowed to be modest."[28] Moreover, by a deliberate fusion of physics and philosophy, all this would not have to be deduced from a physical theory *in the ordinary sense*, but by a theory that, having *the unique status of being final*, tells us nothing but the conditions of any possible experience.

Notes to Chapter II

1 Planck [1949], pp. 28ff. and 106

2 Rohrlich [1965], p. 2f.

3 Weizsäcker [1980], p. 169

4 Bohr [1958], p. 68

5 Heisenberg [1942], pp. 20 and 28

6 Weizsäcker [1980], p. 154

7 Hawking [1980], pp. 1f.

8 Bondi [1977], pp. 7f.

9 Primas [1984], p. 2

10 Mayr [1982], pp. 62f.

11 Rohrlich [1977], pp. 358ff.

12 Woodger [1952], pp. 337ff.

13 Blanshard [1939], vol. II, p. 264

14 ibid. p. 453

15 Minkowski [1909], p.

16 Vienna Circle [1929], p. 8

17 Carnap [1938], p. 61

18 Oppenheim/Putnam [1958], p.

19 Hempel [19969], p. 189

20 Carnap [1966], pp. 243f.

21 Nagel [1949], pp. 99f.

22 Oppenheim/Putnam [1958], p. 4

23 See Ch. VIII

24 Oppenheim/Putnam [1958], p. 28

25 Feyerabend [1965], p. 149

26 Kuhn [1962], pp. 171fff.

27 v. Weizsäcker [1980], p.

28 ibid, p.

Chapter III: The Concept of Progress in Physics

Modern physical text books occasionally give an account of a peculiar relation between physical theories one of which, in the historical development of physics, has become the successor of the other one. Thus Misner, Thorne and Wheeler in their monograph on gravitation[1] expound the view that "as physics develops and expands, its unit is maintained by a network of correspondence principles, through which simpler theories maintain their vitality by links to more sophisticated but more accurate ones." As examples they mention geometrical optics, Newtonian mechanics, thermodynamics and Hamiltonian mechanics as being 'correspondence principle limits' of physical optics, relativistic mechanics, statistical mechanics and quantum mechanics respectively. Then they study in more detail the correspondence structure of General Relativity, pointing out four limits of this theory, one of which is Newton's gravitational theory. "In all these examples and others - summarize the authors - the newer, more sophisticated theory is 'better' than its predecessors because it gives a good description of a more extended domain of physics, or a more accurate description of the same domain, or both." But not only is there an empirical superiority. There is also a "correspondence between the newer theory and its predecessor [giving] one the power to recover the older theory from the newer, [a correspondence which] can be exhibited by straightforward mathematics."

A second example of such a correspondence relation between physical theories is in Rohrlich's monograph on classical charged particles.[2] In the development of physics, Rohrlich writes, "Newtonian mechanics was replaced by relativistic mechanics, thermodynamics by statistical mechanics, classical by quantum mechanics." He distinguishes between two aspects of these replacements. On the one hand, the old theories, having been proved correct over a long time, did *not* really become *wrong*. They only became restricted to a limited domain of validity. "For example, Newtonian mechanics became restricted to phenomena in which the velocities are small compared with the velocity of light. It became an *approximate theory*... But there is another aspect to a theory. While the predictions of a theory will always remain correct when used in the validity domain..., the foundations of the theory, its axioms and the underlying picture (model) *may be radically modified* by a more general theory: the notions of absolute space and absolute time are abandoned in the special theory of relativity... In this way, the *conceptual framework of every theory is eventually superseded.*" Because of these apparently opposed aspects, both realized in one and the same step from a theory to its successor, Rohrlich is somewhat more cautious in his description of the correspondence between successive theories than Misner, Thorne and Wheeler seem to be. Their optimistic view that correspondence is a straightforward mathematical affair is replaced by Rohrlich's view "that the development of physical theory... builds a *hierarchy* of theories [such that although] it is essential that the lower-level theory be derivable from the covering theory [i.e. the theory superseding it] this must be true not so much with respect to the axiomatic framework, which is in general not a special case of the covering theory, but with respect to certain basic equations and postulates which contain all the *predictive power* of the lower-level theory."

So here are two views essentially about the nature of progress in physics actually presented in modern physical texts. Although there is some difference in emphasis they are essentially the same view. So, the primary question in this chapter is: *Whence comes this view?* I contend that it has been developed *by physicists* without any recognizable influence from philosophers or historians of science. Moreover, many if not all of the ideas of central importance in matters of scientific progress recently developed by philosophers or historians of science have been *anticipated* by physicists belonging to the tradition to be discussed. Yet this tradition seems to have gone *almost unnoticed* by philosophers of science. Inspite of the vital interest in questions of scientific development that characterizes philosophy of science during the last two decades I have not found it reported in any of the numerous relevant contributions. It does not appear in the work that Thomas Kuhn has done in the field[3], and I have not found it in I.B. Cohen's recent and fairly comprehensive 'Revolution in Science'.[4] It is time, therefore, to put it before the public.

The main feature of the physicists' view on progress is already evident in a short passage in an obituary for Joseph Stefan written by Ludwig Boltzmann in 1895[5]. I shall quote the passage in two parts. In the first part Boltzmann describes the development of physical theory in the following way:

> The layman may have the idea that to the existing basic notion and basic causes of the phenomena gradually new notions and causes are added and that in this way our knowledge of nature undergoes a continuous development. This view, however, is erronous, and the development of

> theoretical physics has always been one by leaps. In many cases it took decades or even more than a century to articulate fully a theory such that a clear picture of a certain class of phenomena was accomplished. But finally new phenomena became known which were incompatible with the theory; in vain was the attempt to assimilate the former to the latter. A struggle began between the adherents of the theory and the advocates of an entirely new conception until, eventually, the latter was generally accepted.

It is obvious, then, that Boltzmann anticipates the view recently suggested by Thomas Kuhn: A physical discipline develops in alternating phases. The first phase is marked by a fairly *continuous* development. In it we find the physicists doing what they normally do. In Boltzmann's words, they gradually articulate a theory until they have achieved a clear picture of the phenomena belonging to a certain domain governed by the theory. This is *normal science* in Kuhn's sense. However, as time goes on new phenomena *incompatible* - as Boltzmann puts it - with the theory become known. That this incompatibility is not a straightforward matter becomes clear when Boltzmann says that there is a period in which physicists try to *assimilate* these recalcitrant phenomena to the theory. In Kuhn's terminology this is the *crisis* in which the physicists are uncertain about whether the deviating phenomena are proper falsifications of the theory or mere anomalies brought about by causes not responsible for a real clash between theory and experience. Finally, when Boltzmann talks about advocates of an entirely new conception' that eventually supersedes the old theory, this is essentially what Kuhn calls a '*scientific revolution*'. The crisis results in a revolution, and in spite of possibly long periods of continuous development these periods are regularly interrupted by sudden *discontinuous changes*. What recently has excited so many philosophers in Kuhn's book on the structure of scientific revolutions was already known to some physicists by the end of the 19th century.

But this is only half of the story. In the second part of his remarks Boltzmann somewhat mitigates the rupture between the old theory and its revolutionary successor. Thus he writes:

> Formerly one used to say that the old view has been recognized as false. This sounds as if the new ideas were absolutely true and, on the other hand, the old (being false) had been entirely useless. Nowadays, to avoid confusion in this respect, one is content to say: the new way of ideas is a better, a more complete and a more adequate description of the facts. Thereby it is clearly expressed 1) that the earlier theory, too, had been useful because it gave an, if only

> partially, true picture of the facts, and 2) that the possibility is not excluded that the new theory in turn will be superseded by a more suitable one.

Here Boltzmann rejects the view that the discontinuous step is a step from a theory now recognized as being entirely mistaken to another wholly true theory. Clearly, if such were the case then, relative to a given domain of phenomena, physics would not have a development in any proper sense. On the contrary, Boltzmann says that physics is an essentially changing - perhaps an ever changing - enterprise. At no moment is it quite right but - more importantly - *seldom it is quite wrong*. Thus, physical theories well confirmed and accepted for a long time, will in a restricted sense be of eternal value. For all the discontinuity characterizing the development of physics *there is some continuity* even in the sense of *bridging over* those discontinuities. To some extent a well supported theory is preserved and is recovered from its successor.

One might ask: How did Boltzmann come to his view on progress in physics? Since his view is introduced as correcting an earlier one, one might also ask: What was the earlier view like and who held it? I shall not, however, go into these questions. It may very well be the case that Boltzmann was the first to formulate the view in question, because it was only in the second half of the 19th century that theoretical physics became an independent discipline. Boltzmann's obituary praises Joseph Stefan as having been a *theoretical* physicist. He himself was a theoretical physicist, and it is theoretical physics whose development he attempted to characterize in the quoted text. So Boltzmann's characterization came at a time when theoretical physics was still badly needed to justify its independent existence.

To see this, let us take a glance at the introduction to the first edition of a text book on theoretical chemistry by Walther Nernst[6]. It came out in 1893, and begins with an 'Introduction to some basic principles of modern natural science'. In the introduction Nernst, who was awarded the Nobel prize for Chemistry in 1920, distinguishes "two widely differing methods for the discovery of a law of nature" one empirical, the other theoretical. And he is anxious to convince the reader of the importance, if not the superiority of the theoretical method. In contrast to the empirical method of fact gathering followed by inductive generalizations, there is a second way where "thoroughgoing ideas on the nature of certain phenomena [are developed] by a purely speculative activity [leading to] new knowledge whose correctness has to be tested by experiment only subsequently." The scientist searching for such theoretical hypotheses "is continuously in the danger of being led astray by the delusive light of unfortunately chosen principles." And although the development of hypotheses is necessary to deepen our knowledge of the phenomena their eventual abandonment always is to be expected. Put to test "their success, though not proving their correctness, does prove [their] usefulness, while a failure displays not only [their] uselessness but [their] falsity as well." Returning to Boltzmann, we may conclude, therefore, that in trying to get clear about the nature of theoretical physics he sees it choosing the second method. The discontinuities in its development, then, are just the price it has to pay for that choice.

The first to develop Boltzmann's view was Nernst. In the introduction to the 1911 English edition of his 'Theoretical Chemistry'[7] he reminds us first that, because of the unavoidable inadequacy of human inquiry, "many a long-recognized law has had to undergo revision to meet the requirements of the progress of knowledge." Then he writes:

> If we consider the matter more closely, it is obvious that the law in question has retained its validity over a wide range, but that the *limits*

> *of its applicability* have been more sharply defined. It can even be said that since the development of the exact natural sciences, there is scarcely one law established by an investigator of the highest rank which has not preserved *for all time* a wide range of applicability, i.e., which has not remained a servicable law of nature *within certain limits*. We cannot say, for example, that the electromagnetic theory of light has completely overthrown the older optical theory put forward by Fresnel and others. On the contrary, now as formerly, an enormous range of phenomena can be adequately dealt with by the older theory. It is only in special cases that the latter fails; and further, there are many relations between optical and electrical phenomena which certainly exist, but of which the older theory takes no account. Hence the electromagnetic theory implies a great advance, but by no means nullifies the successes of the older theory.

Generalizing Nernst concludes:

> So scientific theories, far from dropping off like withered leaves in the course of time, appear to be endowed under certain restrictions with eternal life; every famous theoretical discovery of the day will doubtless undergo certain restrictions on future development, and yet remain *for all time* the essence of a certain sum of truths.

For Nernst what happens when a theory is superseded by another one is that it is restricted to a certain domain thus preserving validity. A new piece of terminology is introduced and henceforth becomes a common place.[7a] A theory holds in a *limited* range, and the limits of this range become known only after its successor theory has been established. This is what Nernst means when he says that "the limits of its application have been more sharply defined." The limitation has two aspects, one qualitative and the other quantitative. Qualitatively, it is certainly a limitation on any theory of gravity that gravitation is not the only force acting in the universe. The theory, in other words, is limited to certain *kinds* of phenomena. Quantitatively, Newton's theory of gravity, for instance, holds only for small velocities and weak gravitational fields.

The quantitative aspect undergoes refinement in Nernst's article on 'The Domain of Validity of the General Laws of Nature'[8], and has a remarkable repercussion on the revolutionary component of theory succession. It is evident in this article, published in 1922, that Nernst knew Boltzmann's paper. He maintains first that

> the modification of general laws of nature by no means entirely overthrows the earlier laws;

> rather the latter are modified only for *more or less extreme* cases...

However, made wiser by the lesson of relativity theory, Nernst points out some difficulties that we may have to face in describing the relation of a theory to its predecessor. With Einstein's and Newton's gravitational theories in mind he writes:

> The modifications that have to be made with respect to the earlier theory are so small that according to the present state of research they can be neglected except for the computation of the orbit of Mercury. But as a matter of principle every computation that astronomers have performed so far must be changed. And it is this principal aspect of the problem not the numerical amount of the correction, that is our point. To avoid any misunderstanding: the works of Galileo and Newton are "as glorious as on that first day", but they have not brought us the final laws of the motions of the celestial bodies, and nobody would claim this for the theory of relativity...

The remarkable thing about this suggestion is that Nernst relates the two theories in question *by explicit reference to the observational data*. When he says that "in principle every computation that astronomers have performed so far must be changed", he means that the observational data used in one computation on the basis of

Newton's theory must now be matched with Einstein's theory to obtain a corresponding computation on the basis of *this* theory such that the new result coincides with the earlier one within the margins of experimental error whenever the Newtonian computation had been successful. That this business of transforming the older computations into new ones is essentially *approximate* Nernst makes clear in the following passage:

> One might think that... the laws of nature... are valid with absolute precision in certain domains and that the matter could be settled very easily by pointing out the limits within which they remain valid. For all practical applications this is true enough, and it was for this reason that we could ascribe eternal values to the discoveries of Galileo, Newton,... etc. From a strictly logical point of view, however, the matter appears much more disastrous. If a general law of nature becomes significantly inaccurate beyond certain limits, then the curse of this imprecision comes to roost on every application of the law even within these limits, though the magnitudes of the errors are below the threshold of measurement for the time being.

This remark is meant only as being a qualification of the previous one. If we could increase the accuracy of measurement indefinitely we would still have to "change the computations". There is no more than *asymptotic* *equivalence* of the theories in the limiting case. Newton's theory is at best an asymptotic limit of Einstein's nowhere precisely coinciding with it.

That a similar case can be made for quantum theory vis à vis classical mechanics had been stressed even earlier by Einstein. In his 1914 inaugural address to the Royal Prussian Academy of Sciences he says[9]:

> With his quantum hypothesis [Planck] overthrew classical mechanics in the case where sufficiently small masses move with sufficiently small velocities and sufficiently high accelerations. Consequently, we view the laws of motion established by Galileo and Newton as being *limiting laws* only.

Whereas Einstein, although allowing the old laws the status of limiting laws, talks of an *overthrow* of classical mechanics by the quantum hypothesis, Planck himself follows a much more conservative line[10]. Showing that for high temperatures his radiation law approximates that of Lord Rayleigh he emphasizes that "Rayleigh's radiation law deserves *an eminent theoretical interest* because it represents that distribution of energy that is obtained for the equilibrium of bodily molecules with radiation from classical dynamics *without the introduction of the quantum hypothesis.*" Thus Planck did not only want to characterize Rayleigh's law as a limiting case - a classical limit - of his own new radiation law. It was the new radiation law that, to a progressively minded scientist, deserved an eminent theoretical interest precisely because it was not and probably *could not* be derived by using only classical ideas. If Planck ascribes an eminent theoretical interest to Rayleigh's law it is because, he, although in a sense the inventor of quantum theory, did not like that theory and in the depth of his heart never accepted it. Once quantum mechanics had been fully established in 1927 we know that Einstein, too, was reluctant to accept it. But in the earlier statement just quoted his attitude seems to be different. It belongs to a period when he was about to revolutionize physics with his theory of General Relativity, and in a famous paper of 1905 he had introduced the idea of light quanta bearing an energy proportional to their frequency with Planck's constant as the factor of proportionality. No wonder we find him telling his colleagues that quantum theory would *overthrow* classical physics. Thus, here and elsewhere, we find physicists putting different emphases on either the conservative or the progressive element in theory change. By the end of the twenties we can roughly distinguish between a 'disproof view' and a 'conceptual change view' of progress in physics. These views need not contradict each other because typically they are applied to different theory successions. Thus it seems that astronomers, cosmologists, relativity theorists are more inclined to take the disproof view whereas quantum theorists more often adhere to the conceptual change view. Let us now have a closer look at these views in turn.

In an article on 'The Nature of Astronomical Research' of 1933, the Göttingen Astronomer Kienle shows himself to be a typical advocate of the *disproof view*[11]. It is, however, a qualified disproof view anticipating Lakatos' and also Kuhn's views on the role of anomalies in empirical science. Accordingly, Kienle's account of the view is premised by the following passage:

> No experiment realizes in pure form the idealized assumptions of a theory; consequently every test of the theory is possible only with a certain degree of accuracy. To decide whether the deviations between observation and theory are essential or not, whether they are cases of random perturbations of the experiment or of principal faults of the theory, - to decide this is not always an easy matter.

It is only after this general warning that Kienle continues:

> At any rate, progress in our knowledge of nature always is achieved where something goes wrong, where repetition and variation of the experiments always lead to deviations from the theory in the same sense. By means of a stepwise approximation we arrive at... ever simpler and more comprehensive formulations of the basic laws. In this connection time-honoured ideas sometimes must be abandoned, apparently well-founded laws must be discarded. Not, however, because they were 'false' in any absolute sense. Rather they have to yield to something new and more comprehensive in which they remain contained as approximations in their domain of validity whose limits become knowable by means of the new theory.

This statement contains several elements of the conception of progress already delineated in the preceding quotations. The view that progress involves disproof of an empirically well supported theory had in effect already been proposed by Boltzmann, and is implied by Nernst's account of the relation between Newton's theory of gravity and Einstein's. Indeed, by means of the very same example Kienle makes the point explicitly when he says that progress is invariably launched by the clash of theory and experience. Still the view is qualified, as it was in Boltzmann, by adding that it may be quite a delicate matter to decide whether the observational data are sound enough and clearly relevant to the overthrow of the theory.

Finally, that the disproof view is still alive among physicists is evident in an admirable piece of rhetoric from the pen of the cosmologist Hermann Bondi. In his article 'What is progress in science?'[12] Bondi describes the fate of Newton's theory of gravitation by comparing the enormous number of tests each of which the theory had passed brillantly with its final disproof by a slight discrepancy in the motion of the planet mercury. In Bondi's eyes:

> It had been thought that whatever in the world might be difficult, might be complex, might be hard to understand, at least Newton's theory of gravitation was good and solid, tested well over a hundred thousand times. And when such a theory falls victim to the increasing precision of observation and calculation, one certainly feels that one can never again rest assured. This is the stuff of progress. You cannot therefore speak of progress as progress in a particular direction, as a progress in which knowledge becomes more and more certain and more and more allembracing. At times we make discoveries that sharply reduce the knowledge that we have, and it is discoveries of this kind that are indeed the seminal point in science. It is they that are the real roots of progress and lead to the jumps in

> understanding, but in the first instance they reduce what we regard as assured knowledge.

Insofar as Bondi's argument is at all explicit it is concerned *only* with the observational disproof of Newton's theory, and so supports the view I am now talking about. However, between the lines something else is at work and may explain why *this* disproof is so exciting. Bondi, of course, knew what the successor of Newton's theory was, and knowing this he knew that the true importance of *this* disproof was that it led to a *jump in understanding*. However, he seems quite determined to attach great importance to the mere disproof concluding his statement by saying that "in the first instance [the new discoveries] reduce what we regard as assured knowledge." That Bondi is in the tradition in physics that I have traced back to Boltzmann, can be seen by the sequel to the passage quoted. Once again we hear the last part of Boltzmann's song. Though there are indeed these leaps in understanding, these reductions of knowledge or whatever, nevertheless:

> It is, of course, important to remember that when a theory has passed a very large number of tests, like Newton's theory, and is then disproved - and we can certainly speak of its disproof now - you would not say that everything that was tested before - all those forecasts - were wrong. They

> were right, and you know therefore that although the theory *qua* general theory is no longer tenable, yet it is something that described a significant volume of experience quite well. And indeed, although we have a newer and better theory of gravitation - Einstein's theory and one or two variants of it in addition - nevertheless, whenever we do not want to carry out calculations of the motions of planets and satellites with extreme precision - we use Newton's theory because it is simpler.

Let me now turn to the *conceptual change view*. Since there has been so much talk about meaning change during the last two decades this rubric certainly evokes all kinds of associations. And indeed, here we meet with the fact, hard to believe, that a view similar to the one developed by Kuhn and Feyerabend had been outlined by physicists, especially Bohr and Heisenberg, thirty years earlier without ever having been so much as mentioned during the whole controversy provocated by Kuhn and Feyerabend[13]. The following quotation, typical of Heisenberg, confirms the first part of this claim. Heisenberg occasionally talked about

> those strange developments, which have resulted in a change of meaning in many of the most fundamental concepts of physics... Nature has taught us..., by the unexpected phenomena in electrodynamics and atomic physics, that... words

> or concepts have only a limited range of applicability. And when we have to go beyond this range, we are left with rather abstract concepts,... which can be understood by the experts, but cannot be translated without ambiguity into the simple language of dayly life. The new phenomena can be understood, but they cannot be understood in the same sense as the phenomena of earlier physics. The word 'understanding' itself has changed its meaning...[14]

I think these words speak for themselves vis à vis the meaning change debate in philosophy. However, it is not my purpose here to compare explicitly the philosophers' and the physicists' view. Rather it is my purpose to show how the physicists' conceptual change view fits into the tradition to be developed in this chapter.

The best introduction to the conceptual change view is perhaps a passage from Heisenberg's 'Physics and Philosophy'- his Gifford-Lectures. There he introduces it by *contrasting* it with the disproof view. Having described the situation created by special relativity Heisenberg writes:[15]

> Under the impression of this completely new situation many physicists came to the following somewhat rash conclusion: Newtonian mechanics had finally been disproved. The primary reality is the field and not the body, and the structure of

> space and time is correctly described by the formulas of Lorentz and Einstein, and not by the axioms of Newton. The mechanics of Newton was a good approximation in many cases, but now it must be improved to give a more rigorous description of nature.
>
> From the point of view which we have finally reached in quantum theory such a statement would appear as a very poor description of the actual situation...

What is this "point of view... finally reached in quantum theory"? It is threefold, and all three aspects are outcomes of the new mechanics, or - more precisely - they are generalizations made on the occasion of the adventure of quantum mechanics or - even better - of the Copenhagen interpretation of quantum mechanics.

The *first* and most important point is captured by Heisenberg's notion of a *closed theory*. According to Heisenberg the aim of physics is the establishment of closed theories. And whatever progress may be made during the period in which a closed theory is established, *great* progress consists in the transition from one closed theory to another that becomes its successor. What is a closed theory? There are two definitions. According to one it is a theory whose basic concepts already uniquely determine the basic laws of the theory, *or* - equivalently - it is a theory such that whenever certain phenomena can be described by the basic concepts of the theory the laws of the theory will be valid for those phenomena, *or* - more precisely - it is a theory such that to the extent to which a phenomenon can be described by the basic concepts of the theory the laws of the theory will hold good for that phenomenon. The second definition alludes to the possibility of improving a theory by changing it. According to this definition a closed theory is a theory that cannot be improved by small changes. The two definitions are equivalent because of the following consideration. If a theory is closed in the sense of the first definition then the need for its improvement will only occur when its basic concepts become inapplicable. Its improvement will then involve a conceptual change and that is taken to be a great change. If, secondly, a theory is not closed in the sense of the first definition then the need of its improvement may already occur on the occasion of a falsification of its laws. And their correction will then be possible without a modification of the conceptual basis, i.e. it will be possible by a small change.

To illustrate, let us look at general Newtonian mechanics, Heisenberg's paradigm for a closed theory. He describes the situation as follows:[16]

> I believe that Newtonian mechanics cannot be improved at all; and thereby I mean the following: As far as any phenomenon can be described by the concepts of Newtonian mechanics, namely position, velocity, acceleration, mass, force etc., the Newtonian laws are also valid with absolute precision, and this will not change during the next hundred thousand years. More precisely I should perhaps say: With that degree of accuracy with which the phenomena can be described by the Newtonian concepts, also the Newtonian laws are valid.

Heisenberg's idea of a closed theory is very difficult to understand. On the one hand, it seems certain that the question what theories are closed is an empirical question. From a purely logical point of view we could change Newton's second law. In retrospect, Aristotelian mechanics, can be viewed as a modification of Newton's with force being proportional, not to the acceleration, but to the velocity of a body. But Newton's mechanics turned out to be the better theory for empirical reasons. On the other hand, Heisenberg's definitions give a closed theory a quasi-analytical status. If we look for illustration in ordinary talk we would have to resort to meaning postulates. Our ordinary concepts are very flexible, and we can describe widely differing situations with them without changing their meanings. But even in our daily use of language we sometimes are forced to violate meaning postulates. The analogy is not complete because in ordinary talk there seems to be no third category between the contingent statements usually made and the conventional rules of language. By contrast, in Newton's mechanics we have the various specilaizations given by the various dynamical laws, and they even are the typical *non*-closed theories: we can pass from one force law to another one without changing our basic mechanical concepts.

According to Heisenberg there are four closed theories of physics: Newtonian mechanics, statistical thermodynamics, classical electrodynamics (including special relativity) and quantum mechanics. Heisenberg conjectured that a fifth closed theory will arise in the development of a final theory of elementary particles. Of these theories classical electrodynamics and quantum mechanics are successors of Newtonian mechanics, the former with respect to relativistic mechanics of charged particles, the latter in an obvious sense. The still missing fifth closed theory will be the successor to each of these three theories. Presently we know of only one closed theory having been superseded, and since the notion of a closed theory having been superseded, can be understood better the more cases of supersession we know, the material at hand is not very illuminating.

The situation is aggravated when we turn to the *second* aspect of the "point of view finally reached in quantum mechanics". The question arises what the *relation* between *any* two closed theories, one of which is the successor of the other, is. Of course, there is the relation in time that one theory succeeds the other, and there is all the historical stuff that such a case involves. But Bohr and Heisenberg were convinced that there also must be some *logical* relation between the theories expressing a definite correspondence of their respective *contents*. Heisenberg is very definite about this.[17]

> Apparently progress in science could not always be achieved by using the known laws of nature for explaining new phenomena. In some cases new phenomena that had been observed could only be understood by new concepts which were adapted to the new phenomena in the same way as Newton's concepts were to the mechanical events. These new concepts again could be connected in a closed system and represented by mathematical symbols. But if physics or, more generally, natural science proceeded in this way, the question

> arose: What is the relation between the different sets of concepts? If, for instance, the same concepts or words occur in two different sets and are defined differently with regard to their connection and mathematical representation, in what sense do the concepts represent reality?

Heisenberg obviously intends that the relation in question is *not* just that the two theories *contradict* each other and that, therefore, the old theory is 'disproved' not only by the empirical evidence but by the new theory. In Heisenberg's words:[18]

> The behaviour of the atom in many experiments can be described by means of the concepts of mechanics - and in these experiments also the laws of classical mechanics correctly represent the behaviour in question... There are, however, other experiments in which other, non-mechanical concepts are necessary for the description of the atomic state, e.g. concepts that express the chemical behaviour of the atom. In these cases no idea of the atom using mechanical pictures can be given. Therefore, *not even the question comes up whether the laws of mechanics are valid.*

In other words: There can be no disproof; we were not mistaken vis à vis the truth of Newtonian mechanics.

More intricate or, at any rate, much richer and more complete conceptual connections have been assumed to hold between classical and quantum mechanics since the days of Bohr's theory of the atom. To give the best known example, it was already assumed that for high quantum numbers the orbital frequencies of an electron in the atom would approximate the radiation frequencies. This was the original assumption that became the paradigm for various *correspondences* between classical and quantum concepts. The existence of such correspondences was postulated in Bohr's *correspondence principle* that was to display "quantum theory as a rational generalization of the classical theories"[19]. Although this seems to express a quite positive attitude as to the prevailing importance of the classical theories Bohr warns us against oversimplifications of the correspondence in question. The generalization achieved in quantum theory "does not mean... that classical electron theory may be regarded simply as the limiting case of a vanishing quantum of action."[20] Thus Bohr says:[21]

> The [asymptotic connection of atomic properties with classical electrodynamics, demanded by the correspondence principle] means that in the limit of large quantum numbers, where the relative difference between adjacent stationary states vanishes asymptotically, mechanical pictures of electronic motion may be rationally utilized. It must be emphasized, however, that this connection cannot be regarded as a gradual transition

> towards classical theory in the sense that the quantum postulate would loose its significance for high quantum numbers.

These rather technical remarks of Bohr harmonize with his more general remarks in which he emphasizes the principal impact that quantum theory has had on human knowledge. A la Heisenberg, Bohr says, for instance, that "the extension of physical experience in our days has... necessitated a *radical revision* of the foundation for the unambiguous use of our most elementary concepts."[22] Thus, although the correspondence principle certainly represents the conservative component in the Copenhagen conception of progress - the bow, so to speak, to the time-honoured but finally superseded predecessor -, yet all of Bohr's attempts at an adequate general formulation of the principle emphasize that the correspondence is between two fundamentally *different* theories. In one of these attempts he even *separates* "the demand of a direct concurrence of the quantum mechanical description with the customary [classical] description in the border region where the quantum of action may be neglected." And then the correspondence principle proper expresses "the endeavours to utilize in the quantum theory every classical concept in a reinterpretation which fulfills this demand *without* being at variance with the postulate of the indivisibility of the quantum of action."[23]

So far I have let Bohr answer Heisenberg's question about the relation between a progressive succession of closed theories. And I will leave it at that because in the book I have quoted from, Heisenberg's answer is very brief: he just uses the formula of the limiting case. Since Bohr warns us to apply this formula only with great care, it might appear that the two men had different opinions on the matter. But this is not the case, and apparent differences are only due to occasional variations of expression.[24] These variations are quite excusable in view of the tremendous difficulties presented by the case. The concept of closed theory and the relation of their progressive successions notwithstanding, the difficulties arising from the case of quantum theory still reflect the two major parts of progress in physics that were introduced by Boltzmann: Progess proceeds by leaps, by jumps in understanding, but there is also an element of continuity taking into consideration the merits of the superseded theory. In the case of quantum mechanics, however, a third element came into play, an element which leads directly to the third aspect of the "point of view reached in quantum theory". This aspect makes it difficult if not impossible to characterize theory progression by means of disproof and contradiction or at least: *merely* by means of disproof and contradiction.

The third aspect has been called a *paradox* by Heisenberg.[25] The paradox is that, according to the Copenhagen interpretation, the experiments designed to test quantum mechanics must be described in classical concepts, or - more generally - the experiments designed to test a closed theory must be described in terms of its predecessor, i.e. in terms whose limitations in principal are revealed by the successor theory. Thus the terms whose limited applicability has been shown by a new closed theory belong in a sense to the preconditions of its own applicability. Bohr has repeated over and over again that "however far the phenomena transcend the scope of classical physical explanation, the account of all evidence must be expressed in classical terms. The argument is simply that by the word 'experiment' we refer to a situation where we can tell others what we have done... and that, therefore, the account of the experimental arrangement and of the results of the observations must be expressed in unambiguous language with suitable application of the terminology of classical physics."[26] Heisenberg has generalized this by saying:

> Even if the border lines of a 'closed theory' have been passed, i.e. if new regions of experience have been systematized by new concepts, still the conceptual system of the old closed theory constitutes an indispensable part of the language in which we talk about nature. The closed theory belongs to the preconditions of further research: We can express the result of an experiment only in terms of a previous closed theory."[27]

Although the third aspect of the Copenhagen interpretation of quantum mechanics and hence of the Copenhagen view of progress in physics is quite controversial even among physicists, I have included it because it certainly is an integral part of the Bohr-Heisenberg conception which along with the other two aspects, obviously belongs to the tradition I am describing in this chapter. Indeed it is its climax. So I will conclude this review with two more recent citations to round off the presentation. There is, first, a particulary well balanced description by von Weizsäcker who, having been a disciple of Heisenberg, belongs to the Copenhagen Circle in a wider sense. Speaking of the development of physics toward unity Weizsäcker says:[28]

> In this process, the earlier theories are modified by the later ones. But they are not really overthrown; rather their domain of

> validity is delimited. ... we can describe this successive self-correction of physics roughly as follows: an older closed theory - classical mechanics, for instance - adequately describes a certain domain of experience. This domain, we later learn, is limited. But so long as the particular theory is all physics can say about that domain of experience, physics simply does not know its borders; the theory does not delimit its own validity. For this very reason, the completed theory serves also as the initial scheme for the opening up of a much wider domain of experiences. Somewhere within this wide domain it then comes up against the limits of what it can grasp with its concepts. Out of this crisis in the initial basic scheme a new completed theory finally arises - special relativity, for example. This theory now includes the older one as a special case, and thereby delimits the accuracy within which the older theory applies in particular instances: only the new theory 'knows' the limits of the old. The new theory in turn is an initial scheme with regard to a still wider domain of experiences, whose borders it might intuit but cannot sharply delimit.

Weizsäcker uses this picture in order to convince his reader that "physics is characterized by a greater *conceptual unity* today than at any time in its history." Essentially the same picture is used by Sambursky to demarcate the development of modern physcis against the situation in antiquity. In his "The physical world of the Greeks" Sambursky, himself a trained physicist, says: [29]

> Whoever adopts the theory of relativity may in every actual case fall back on Newton's theory as a first approximation to reality, an approximation which is frequently quite sufficient for the description of the facts. In spite of the theoretical and philosophical difference between classical mechanics and the new theory, and in spite of the formal difference in their mathematical method, the former is still included in the latter as a first approximation. History of science in the past three hundred years is characterized by a chronological and almost organic sequence in its development which was not found to anything like the same extent in Greek science. It is above all the history of physics from Galileo to our time that makes us realize that science advances towards reality so to say by concentric approximations - each theory containing its forerunner as a 'special case'. On the other hand there is nothing to bridge the gulf, e.g. between Aristotle's conception and that of Democritus before him. On the whole, one would rather say that many of Aristotle's ideas, as much as one admires their intellectual acumen, were in the nature of a regression from those of the early atomists, ...

With this citation my account of the physicists' conception of progress ends. What is such an account good for? The answer is that, as far as there are historical facts, some of them are worth knowing, and in my view the tradition I have outlined deserves to be better known than it is. The physicists tradition is badly in need of further analysis, both logical and philosophical. Whereas Heisenberg certainly was convinced that what he said about the notion of a closed theory was, at a general level, all that conceivably could be said about the matter, and similarily for Bohr vis à vis his correspondence principle and so also for the declaration of every other physicist I have mentioned, a philosopher of science would still ask questions such as: *what* on earth *is* the relation between concepts and laws as conceived in the notion of a closed theory? *What is* the relation between two successive closed theories? And so on with respect to many other notions involved in physicists' conceptions of progress. Indeed, from the view point of philosophy of science, the physicists' account is quite incomplete, and this remark detracts not one bit from their reputation as physicists or belittles the merits of the admittedly incompletely developed conceptions in question. It is the philosopher's task after all to answer philosophical questions.

The analysis would, of course, require in part a comparison of the physicists' view with the corresponding ideas developed in philosophy of science. We have seen that Boltzmann makes two fundamental distinctions in the description of physical progress. He distinguishes first between two alternating phases in the development of theoretical physics, one normal and the other revolutionary. Second, in the revolutionary phase he distinguishes between an element of discontinuity and one of continuity. The first distinction does not seem to have been further developed by physicists after Boltzmann. But I have already noted that Boltzmann thereby anticipated Thomas Kuhn. And similarly with the various branches into which Boltzmann's second distinction has been developed by physicists. Reading Kemeny and Oppenheim's famous paper "On Reduction"[30] one is, if only dimly, reminded of what Nernst in a more appropriate way had to say on the role of observational data in progressive theory succession. The disproof view taken together with the limiting case idea reappears in Popper's work, and Popper even seems to be semi-aware of his indeptness to Bohr.[31] Finally, the concept of theory incommensurability as suggested by Feyerabend and Kuhn[32] seems quite close to Heisenberg and Bohr's conceptual change view. Although Feyerabend and Kuhn know about the Copenhagen interpretation of quantum mechanics they do not discuss the relation between their concept of incommensurability and that of Heisenberg and Bohr.

This is all the more surprising since here perhaps the most interesting perspectives open up. For one thing, the usual incommensurability and, in particular, complementarity between quantum mechanical observables essentially satisfy the conditions defining Feyerabend's language incommensurability. The latter is an interpretational incompatibility between two statements in the sense that under no circumstances can the statements be simultaneously meaningful. According to the Copenhagen interpretation a statement of the form an electron has position x' or 'an electron has momentum p' becomes meaningful only under the presupposition that a position or momentum measurement has been made. Since these measurements exclude each other we here have a case of meaning incompatibility before us. One would even be inclined to say that it is the most elaborated case we know of and that, therefore, incommensurable languages *can* be united in one theory. Consequently, there is no principal reason to be horrified by incommensurabilities. Moreover, as I understand him, Bohr always meant his concept of complementarity to become a fundamental epistemological concept without which the striving for unity would be doomed to failure. Further investigations on the relation between the two conceptions may, therefore, throw more light on the important issue of theoretical pluralism as an alleged antithesis to the unity of science.

Notes to Ch. III

1 Misner, Thorne, and Wheeler [1973], § 17.4

2 Rohrlich [1965], Ch. 1

3 Kuhn [1970] and [1977]

4 Cohen [1985]

5 Boltzmann [1905], pp. 94 f.

6 Nernst [1893], pp. 2 f.

7 Nernst [1911], pp. 4 f.

7a The earliest occurrence of the term 'Grenzfall' (limiting case) known to me is in Hertz [1892] pp. 21-31.

8 Nernst [1922], p. 489 and pp. 491 f.

9 Einstein [1914], pp. 740 f.

10 Planck [1913], p. 164

11 Kienle [1933], pp. 115 f

12 Bondi [1983], pp. 89 f. It has to be noticed that Bondi is influenced by Popper's philosophy of science. I nonetheless quote him as a spokesman of physics because I feel certain that he is physicist enough to have rejected Popper's view had he found obvious reasons for that from physics

13 One exception is Feyerabend, but only in a very incidental manner. See his [1965], p. 271 and [1970], p. 300.

14 Heisenberg [1975], p. 161

15 Heisenberg [1958], pp. 96 f.

16 Heisenberg [1969], p. 135

17 Heisenberg [1958], pp. 97 f.

18 Heisenberg [1942], pp. 18 f.

19 Bohr [1934], p. 70

20 ibid. p. 87

21 ibid p. 85

22 Bohr [1963], p. 9

23 Heisenberg [1958], p. 110

24 On matters controversial between Bohr and Heisenberg at times see Folse [1958], Ch. 3, § 7

25 Heisenberg [1958], Ch. III

26 Bohr [1958], p. 39

27 Heisenberg [1948], p. 335

28 Weizsäcker [1980], pp. 169 f.

29 Sambursky [1956], p. 97

30 Kemeny and Oppenheim [1956]

31 Popper [1972]. On p. 202 Popper suggests to call "the demand that a new theory should contain the old one approximately (following Bohr) the 'principle of correspondence'".

32 Kuhn [1970] and [1977], Feyerabend [1965] and [1970].

Ch. IV: Deduction

This chapter is the first of four chapters in which various conceptions of progress, reduction and explanation developed by philosophers of science will be presented. You will recall Weizsäcker's summary of the conception of progress in physics at the end of the previous chapter. Immediately following that passage Weizsäcker says:[1]

> What I have just described is commonly accepted by today's physicists in their methodological reflections; I do, however, wish to parenthetically voice my suspicion that the philosophy of science has not yet developed the concepts required for a description of these structures.

He is right, philosophers of science not only did not try to explicate physicists' talk, with few exceptions, they were not even influenced by them. Some philosophers may have developed conceptions of progress approximating the conception of certain physicists, but, on the whole, I think, Weizsäcker's judgement is correct although some suggestions by philosophers of science are not irrelevant to the methodological reflections of physicists. Moreover, as I said earlier, when compared with physicists, philosophers' contributions are much more *explicit* about what the relation between successive theories constituting progress really is. In this chapter I shall discuss the view according to which the relation in question is established essentially by means of *logical deduction*. While nothing of this sort was part of the physicists' conception of progress, *if* logical deduction is made an integral part of philosophers' conception of progress, a very strong bond is introduced in physics tying together the theories involved in a theory progression. Mind, however, from the beginning that almost all concepts of explanation etc. to be discussed in these lectures include logical deduction *in some sense or other*. This chapter is called 'deduction' because in it explanations or reductions will be defined by logical deduction in a fairly *direct* sense.

Let me begin by returning to remarks made in the introduction on the current use of the words 'explanation', 'reduction', 'progress'. The previous chapter avoided the first two altogether because physicists, though they frequently talk of explanations and occasionally even of reductions, seldom if ever use these words in the context of theory change and progress. This usage - or rather lack of usage - does not mean that a physicist would eschew such terms in talk about progress. It only means that, *as far as a physicist's conception of progress goes*, words like 'explanation' or 'reduction' do not naturally lend themselves as names for that conception or any part of it. Now, as substantial characterizations of theory progress are to be introduced, the situation changes: Philosophers are used to talking about explanations or reductions especially if logical deduction is brought directly into play. However, one should not thereby infer that the following explications of the ideas of explanation and reduction are meant to *exhaust* the idea of progress in the sense that progress *always* is progress by explanation or reduction in the rather narrow sense to be defined. Such an inference is neither justified by the nature of the case nor by what the authors to be discussed intended to achieve.[1a]

IV.1. Deduction Proper: Homogeneous Case

In this section of this chapter I shall restrict "deduction" to "deduction proper" or "deduction in the strict sense". My aim is to argue, or establish the underpinnings of an argument to the effect, that concepts of explanation or reduction according to which an explanation or reduction simply *is* a deduction *strictu sensu* are *not* particularly helpful in the description of scientific progress: Although these views of explanation and reduction cover a few historical cases they certainly are not sufficient to treat the big cases physicists talk about. But I shall also argue that explanation or reduction as deduction proper, *if* rightly conceived, is a powerful *heuristic* tool by means of which to develop and to understand concepts of explanation and reduction that are *more* adequate for the description of progress.[1b]

Probably the best known treatment of explanation or reduction via deduction proper is the *deductive-nomological (D-N) model* of scientific explanation. Here what is to be explained is an event described by a singular sentence E, and the explanation is the deduction of E from premises some of which are the laws
L_1, L_2, ..., L_m, and the others the singular statements

C_1, C_2, ..., C_n like E. Thus the pattern of explanation is as follows:

$$\left.\begin{array}{ll} L_1, L_2, \ldots, L_m & \text{laws} \\ C_1, C_2, \ldots, C_n & \text{sing. statements} \end{array}\right\} \text{explanans}$$
$$\begin{array}{ll} E & \text{explanandum} \end{array}$$

In the words of Hempel, this scheme yields an explanation in the sense that it "answers the question 'Why did the explanandum-phenomenon occur?' by showing that the phenomenon to be explained results from certain particular circumstances, specified in C_1, C_2, ..., C_n in accordance with the laws L_1, L_2, ..., L_m." According to Hempel, "the argument shows that, given the particular circumstances and the laws in question, the occurrence of the phenomenon *was to be expected*, and it is in this sense that the explanation enables us to *understand why* the phenomenon occurred."[2]

In their original publication of 1948 outlining the D-N model, Hempel and Oppenheim did *not* in their conditions of adequacy restrict explanations to cases where the explanandum is a singular event, but, in their final definition, they *did* so restrict it. The reason was that although they did not *want* such a restriction in general[3], they had to admit that "the precise rational reconstruction of explanation as applied to general regularities presents peculiar problems for which we can offer no solution at present."[4] The difficulty was that in a *definition* where the conditions listed in the definiens not only are necessary, as they are in conditions of adequacy, but also sufficient, all undesirable cases have to be excluded. But the authors could not figure out how to exclude cases of trivial self-explanation such as the explanation of Kepler's laws via a deduction from the conjunction of Kepler's laws and Boyle's law. To avoid such cases Hempel and Oppenheim thought it necessary to distinguish in general between different levels of statements as to their comprehensiveness. The L-sentences would then have to be "on a higher level" than the explanandum sentence E which is automatically the case if E is singular and the L-sentences are lawlike.

Despite the problem of self-explanation which, as far as I know, is still an open problem, other philosophers of science have developed a deductive concept of explanation where the explanandum is *required* to be not a singular statement but a *higher level law* or even a whole *theory*. Only one year after the Hempel/Oppenheim paper Ernest Nagel proposed a deductive or derivational model of theory explanation or, as he preferred to call it, theory *reduction*. The shift in terminology is not important, however, because for Nagel a reduction *is* an explanation. In his later book of 1961 he says:

> Reduction ... is the *explanation* of a theory or a set of experimental laws established in one area of inquiry, by a theory usually though not invariably formulated for some other domain.[5]

Thus reduction is explanation and this view is reinforced by the deductivist account that Nagel gives of reduction. Already in the 1949 paper he says:

> The objective of the reduction is to show that the laws or general principles of the secondary science [i.e. the one to be reduced] are simply *logical consequences* of the assumptions of the primary [i.e. the reducing] science.[6]

Leaving the question in what sense such a reductive relation is an explanation for later discussion let me now finish Nagel's account by drawing attention to an apparent discrepancy between the previous two quotations: how can a theory be logically deduced from another one if the two refer to non-intersecting domains and, therefore, talk about different things? Nagel's answer is:

> A necessary condition for the derivation is the explicit formulation of *suitable relations* between such expressions in the secondary science [that do not occur in the primary] and expressions occurring in the premises of the primary discipline.[7]

The sentences expressing the relations in questions would then have to be added to the assumptions of the reducing theory, and the total set of premises thus obtained would have to imply the axioms of the reduced theory. Nagel distinguishes between *homogeneous* reductions, where the vocabulary of the reduced theory is part of the vocabulary of the reducing theory, which preclude the difficulty under discussion, and *inhomogeneous* reductions, where the vocabulary of the reduced theory, or, at least, key portions of it, are not part of the reducing theory, in which cases additional premises of the kind described - socalled 'conditions of connectability' or 'bridge laws' or 'reduction sentences' - have to be provided. According to Nagel, only the inhomogenous case is worth discussing.

This is an overstatement, as will emerge shortly. But let me first give two examples of proper homogeneous reduction that can be viewed as historical cases of explanation. First, there is the case of Galileo's law of free fall. Though rather trivial, this law cannot be explained by proper reduction to Newton's mechanics. However, another law found by Galileo and sometimes considered part of the law of freely falling bodies says that the acceleration a body gets at a given place on the surface of the earth is the same for any two bodies irrespective of their masses. This law of acceleration follows strictly from Newton's theory. For, according to Newton's Second Law, we have

$$(1) \qquad K_i = m_i b_i \qquad (i = 1,2)$$

for the two bodies. According to the gravitational law, we have

$$(2) \qquad K_i = \gamma \cdot m_i \cdot \frac{M}{R^2}$$

where M and R are the mass and the radius of the earth respectively. From the two equations it *follows* immediately that

$$(3) \qquad b_1 = b_2 = \gamma \cdot \frac{M}{R^2} \quad .$$

So here we have a historical example of some importance that is a case of homogeneous reduction.

Second, there is the case of Planck's famous formula

$$(4) \qquad \varrho = \frac{8\pi\nu^3}{c^3} \cdot \frac{1}{\exp\{\frac{h\nu}{kT}\} - 1}$$

for the spectral space density ϱ in black body radiation. Now, the Stefan-Boltzmann law

(5) $$u = \alpha \cdot T^4$$

for the energy density u was known before Planck discovered the law in (4). Since the relation between ϱ and u is

(6) $$u = \int_0^\infty \varrho(\nu)\, d\nu.$$

the Stefan-Boltzmann law *follows* immediately from Planck's law. So again we have a case of an hitherto unexplained empirical law being explained by a more comprehensive law simply by showing the former to be a necessary consequence of the latter. And like the Galileo/Newton case it is homogeneous if (5) is seen to be developed in a theory with ϱ as one of its primitive concepts and u is *defined* by (6).

After these examples I am anxious to emphasize the point that I find badly neglected in the literature. I will treat it in some detail neglecting other important aspects of (homogeneous) deductive theory explanation which are considered by other authors. In view of Nagel's description of theory explanation (or reduction) the general *logical form* of an explanation would be

(7) $A' \wedge c \vdash A$

where A' and A are the basic assumptions - the axioms - of the explaining and the explained theory respectively, and c are the conditions of connectability or bridge laws needed for the inhomogeneous case. Nagel's account implies that no other premises, in addition to the axioms A' of the explaining theory, besides c, are necessary or indeed even *allowed* in theory explanation. In particular, the homogeneous case, belittled by Nagel, reduces to a "direct" implication

(8) $A' \vdash A$

of the axioms of the explained theory by those of the explaining theory. And indeed the general impression one receives from the literature is that *apart* from the bridge laws it is *left open* whether theory explanation has the general form of (7), perhaps even *must* have this form, or rather of (8). Sometimes this issue is discussed, and some have suggested that (7) is the required form of theory reduction because in reduction a less comprehensive theory is reduced to a more comprehensive theory. One scholar even says: "... some have thought it worthwhile to discuss the question of whether the less comprehensive is derivable from the more comprehensive theory *alone*. No one would or should be so foolish as to hold such a view."[8] However, in general the question is left open at least in the sense, that general formulations clearly point to (8) whereas the examples suggest (7).

I shall give two arguments in favour of (7) which require auxiliary assumptions other than bridge laws. I am quite aware, of course, that it is *very* difficult to reasonably *restrict* the auxiliary assumptions of (7) in such a way that $A' \wedge c$ still can be viewed as an extension of the explaining theory as opposed to a *new* theory, or - equivalently - that the explaining theory is prohibited from explaing *almost everything*. This problem clearly is an instance of the problem - already mentioned - of an adequate concept of levels of comprehensiveness of laws or theories. Moreover, the restriction will have to be a restriction *in kind*. For otherwise the decision between (7) and (8) would not be well defined, (7) being a special case of (8) either by reformulation, as in

$$A' \vdash c \rightarrow A \ ,$$

or by taking $A' \wedge c$ to be the explaining theory. I shall discuss the problem of admissible theory extension in greater detail in section 3. The following arguments depend only on intuitive considerations, and proceed by analogy and by examples.

On an intuitive understanding of explanation, I will argue that the additional premises in question are part of the *meaning of explanation*. The model is event explanation or explanation of singular phenomena. In this kind of explanation - the one with which this chapter began - the explaining theory, represented by the laws $L_1, \ldots, L_m$ in (HO), has something *absolute* about it. Though one may know a theory's alternatives, in its rôle as an *explaining* theory none of these alternatives is taken into consideration. It is quite different with the phenomena *to be explained*. A theory covers a whole region of phenomena each of which is *contingent* upon the theory, i.e. although the occurrence of everyone of these phenomena is compatible with the theory none follows from it. Thus, as far as the theory goes none of the phenomena will occur of necessity. If one of them actually occurs, all the others *could* have occurred as well and are possible alternatives to it in this sense. Accordingly, if a phenomenon is to be explained by the theory we have to look for conditions C_k, likewise contingent with respect to the theory, such that *if* they are added to the laws L_i the sentence expressing that phenomenon follows. But then it also becomes apparent that if *other* conditions should hold, *not* the occurrence of the phenomenon in question, *but one of its alternatives*, would have been explained by means of the *same* theory. Here is a clear case of explanation where, within the scope of one theory that does the job of explanation, the explanandum has alternatives, and it is, therefore, only with respect to certain *additional contingent conditions* that we understand, by implication, that the explanandum in question and *not one of its alternatives* actually occurred.

The argument in favour of (7), then, is that there is an important, if not decisive, feature of event explanation worth keeping in theory explanation: Explanation is only where alternatives to the explanandum are taken into account. It may be objected that in event explanation there is simply no choice: the things we want to explain simply are not consequences of the theory. By contrast every theory certainly has many interesting direct general consequences explained just by this logical fact. True enough. But the goal of my argument is simply the weaker claim that *there are* important cases of theory explanation which require auxiliary assumptions and therefore we must not restrict explanation of theories to the form of (8). I readily admit that this proposal is beset by all the difficulties besetting the distinction between (7) and (8). So, for the moment I shall take refuge in examples.

Recall the relation between Kepler's laws of planetary motion and Newton's field theory of gravitation as applied to the solar system. By the latter I mean the theory where the planets are treated as independent test particles moving in the central field of the sun, the latter being assumed to be at rest. From this theory only Kepler's second law follows. So there is no question of an explanation of type (8). Rather Kepler's laws have alternatives within Newton's theory, represented by hyperbolic or parabolic, as distinct from elliptic, motions. Accordingly we can find conditions which together with Newton's theory exclude these cases and have all three laws of Kepler as consequences. One such condition is that the total energy of the planet in the field of the sun be negative. Since this is certainly a contingent condition, we have a clear example showing the necessity of auxiliary assumptions in some theory explanation. As to the stronger claim that arguments having the form of (8) should be excluded - ceteris paribus - as explanations, I have nothing to say that goes beyond what I have said in the case of event explanation. At the same time I can see no reason not to maintain the argument given also for the explanation of theories. On the contrary, if one leaves consideration of explanations given within one explaining theory and considers the very common situation of a theory that is, so to speak, the 'last word' known about a certain subject matter, this implies that no alternative to such a theory is known. But this situation raises the question: How do we explain the theory if we don't even know how things could be otherwise than this theory states? This question supposes clearly that alternatives to an explanandum are part of the meaning of explanation. And once this is admitted, the use of additional premises is an immediate consequence.

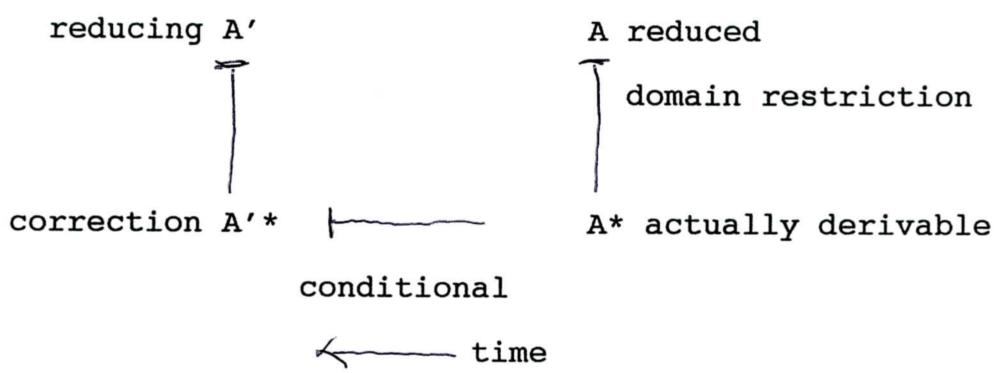
The *second* argument in favour of the view that explanations of theories have the form of (7) is an *argument from progress*. In the introduction it was pointed out that progress in science *may* be progress by explanation or reduction. How can that be and, in particular, how can it be for the kind of reduction under discussion in this section? This question has many aspects, only one of which is important here. In a reduction primarily two theories are involved: the theory to be reduced and the reducing one. The usual developmental interpretation of reduction, then is that the theory to be reduced is the earlier one and the reducing theory its temporal successor. The successor theory may be better than its predecessor in many respects. One such aspect emerges from the question: What has happened *to the earlier theory,* to the theory to be reduced? The answer that the earlier theory has become the later one is not satisfactory. Rather it must be *specified into which part* of the later theory its predecessor has developed. Or: how has the earlier theory been reconstructed within its successor? How can we recover it from the latter? One possible answer to these questions is: The theory has not changed at all. The reduction has left it unchanged in each and every respect. Moreover, this is very likely to happen in deductive reduction. For what else should be derived from the reducing theory but the theory to be reduced? And in fact precisely this happened in our two examples. But if this is all there is to it, then *progress,* under any guise, simply is not possible with respect to the theory to be reduced. Because this would have been assimilated unchanged, there can be *no* advance in the process concerning *it.* Thus the urgent question is: Is this the whole story, or can we hope for improvements in theory in the sense of reductions by homogeneous proper deduction?

Now, as I already said, reduction, if identified with proper deduction, does not take one very far in matters of scientific progress. And we have just seen the reason why. There is, however, one way out, which, though not very important *by itself*, should be mentioned for *heuristic* reason. Whereas exact reproduction of the reduced theory can be achieved by unconditional deduction (as in (8)) it can also be achieved by conditional deduction (as in (7)). The way out I have in mind applies only to exact reproductions achieved by means of conditional deduction.

The argumet is simple, but despite its heuristic importance, it is completely absent in the literature. Let me begin the argument with a remark very familiar *in a different context*: If in (8) the basic assumptions A are empirically refuted then the same holds for A'. By contrast, if A in (7) is empirically refuted, then A' can be saved from refutation simply by pinning the blame on the additional assumption c. My point is an application made of this remark. In the relevant literature one often finds it said, and indeed I said it a moment ago, that in deductive reduction it is the theory to be reduced that has to be derived from the reducing theory and - possibly - certain additional assumptions. The crucial question now is what counts as 'the theory to be reduced'. Surely it includes at least *what* the axioms of the theory say: You can't claim to have reduced Kepler's first law to Newton's theory when you have actually derived his second laws from that theory. What the theory *claimed to be reduced* says must be the same as what the theory *actually derived* says. Another feature of what counts in the theory to be reduced is its universe of discourse. Here, it seems to me, we must say that the theory actually derived is about a universe of discourse that is only *part* of the universe of discourse of the original theory to be reduced. And this is the focal point of the promised way out.

Consider the following partly fictitious example. Suppose, contrary-to-fact, that Kepler's laws were applied to the planets as known in Kepler's time *and* to other celestial bodies one of which turned out to have a hyperbolic orbit. Now, Newton's theory, taken in its central field approximation, can cope with this situation but Kepler's cannot. Secondly, as already mentioned, Newton's theory *restricted* to bodies having negative energy implies Kepler's. Indeed, under this restriction it is identical with the latter. We have, therefore, an *improvement* of Kepler's theory in the following sense: Originally assumed to hold for any celestial body in the solar system, Kepler's theory, including its universe of discourse, is reduced *not* by deriving it *itself*, but rather by deriving a certain restriction of it effected by an additional contingent assumption to Newton's theory. It is this contingent assumption that *cuts off* the cases that could falsify Kepler's laws, that is, celestial bodies with nonelliptical orbits.

Modest though such an improvement in theory may be, it is not easy to see how it could be accomplished by means of unconditional deduction. The general situation may be described as follows:



The improved version of A is A'* because it is the weakest consequence of A' necessary for a derivation of the domain restricted version A* of A by addition of an appropriate premise. Indeed, as the following abstract example shows, in properly deductive theory reduction, reducing and reduced theory may *contradict each other*. Let A, the theory to be reduced, say

$$A[Y;t]: \qquad Y = t$$

Let the reducing theory A′ say:

$$\begin{aligned} A'[Y;s,s_1,t]: \quad & s,s_1,t \leq Y \wedge s \leq t \\ & \wedge s_1 \cap t = \emptyset \wedge s_1 \neq \emptyset. \end{aligned}$$

Because $s_1 \neq \emptyset$ the two theories A and A′ *contradict each other*. But if A′* and A* are, respectively,

$$A'^*[Y;s,t] : s,t \leq Y \wedge s \leq t,$$

and

$$A^*[s,t] \quad : A\,[s;t],$$

then it follows both that

$$A'[Y;s,s_1,t] \vdash A'^*[Y;s,t] \;,$$

and

$$A'^*[Y;s,t] \wedge t \leq s \vdash A^*[s;t] \;.$$

IV.2 Approximative Deduction: Homogeneous Case

There is a straightforward argument that strict deduction cannot account for a very important kind of improvement in theory succession. The previous section made evident that improvements by restriction of the universe of discourse are possible *provided* one allows for auxiliary assumptions effecting this restriction. However, these corrections do *not* concern what the theory to be reduced says. What a theory says is exactly reproduced even in these domain restricting deductions. Now, as already indicated, proper deduction does not suffice to describe theory progress in physics, and it is insufficient precisely for the following reason. Progress in physics requires that a theory T′ superseding another T corrects T *including* what T says. Indeed this may even be the most important improvement in the transition from T to T′.

Apparently the first philosopher of science to have noticed this fact was Popper. Looking, as usual, for arguments against induction, Popper, in a 1949 paper, adopted an argument by Duhem against the view that Newton's theory is an inductive generalization of the laws of Galileo and Kepler and then extended Duhem's argument by suggesting that these laws should be viewed rather as *approximate consequences* of Newton's theory. Popper says:[9]

> A *good* example from the history of science that may be used to illustrate my analysis is the transition from the theories of Kepler and Galileo to the theory of Newton.
> That this transition has nothing whatever to do with induction, and that Newton's theory cannot be regarded as anything like a generalization of those two earlier theories may be seen from the undeniable [and important] fact that Newton's theory *contradicts* them. *Thus Kepler's laws cannot be deduced from Newton's* [although it has been often asserted that they can be so deduced, and even that Newton's can be deduced from Kepler's]: Kepler's laws can be obtained from Newton's only *approximately*, by making the [false] assumption that the masses of the various planets are negligible compared with the mass of the sun. Similarly, Galileo's law of free falling bodies cannot be deduced from Newton's theory: on the contrary, it contradicts it. Only by making the [false] assumption that the total length of all falls is negligible compared with the length of the radius of the earth can we obtain Galileo's law *approximately* from Newton's theory.

In a later paper of 1957 Popper argued 1) that an approximate deduction can be viewed as an *explanation*, and 2) that *correction* of the theory to be explained is an essential feature of such explanation and plays a considerable role in that explanation. Popper says:[10]

> Newton's theory unifies Galileo's and Kepler's. But far from being a mere conjunction of these two theories - which play the part of *explicanda* for Newton's - *it corrects them while explaining them.* The original explanatory task was the deduction of the earlier results. Yet this task is discharged, not by deducing these earlier results but by deducing something better in their place: New results which, under the special conditions of the older

> results, come numerically very close to these older results, and at the same time correct them.

We know from the previous chapter that since the beginning of the century physicists talked of one law being a limiting case of another one. We also saw that the word 'limit' in this context means two things simultaneously: the limit of a mathematical approximation and the limit as the border line of the domain of validity of a theory. That Popper must have had at least *some* familiarity with these ideas is seen by the following two quotations. The first is from the earlier publication and reads:[11]

> Newton's theory is an example of an attempt to explain certain older theories of a lower degree of universality, which not only leads to a kind of unification of these older theories but at the same time to their falsification (and so to their correction by restricting or determining the domain within which they are, in good approximation, valid). A case which occurs perhaps more often is this: an old theory is first falsified; and the new theory arises later, as an attempt to explain the partial success of the old theory as well as its failure.

I shall return to the last phrase of this passage later arguing that the new theory explains the *success* of the old theory *as well as its failure*. This may remind you of what was said on explanation in the previous section, and may suggest that an approximate derivation as envisaged by Popper necessarily involves auxiliary assumptions. The second passage reads:[12]

> I suggest that whenever in the empirical sciences a new theory of a higher level of universality successfully explains some older theory *by correcting it*, then this is a sure sign that the new theory has penetrated deeper than the older one. The demand that a new theory should contain the old one approximately, for appropriate values of the parameters of the new theory may be called (following Bohr) the '*principle of correspondence*'.

So here Bohr's correspondence principle is specifically mentioned, and in light of the previous chapter common ground seems to have been established. Popper is not trying to interpret the views of certain physicists, but rather is simply talking about the same or similar things talked about by physicists.

A few words about the context in which Popper's notion appears (in both papers) is appropriate. One would think that his concept of an approximative explanation arises as an alternative to, or a generalization of, the deductive-nomological concept of explanation. And indeed both papers begin with an exposition of precisely the latter concept. But having stated that the aim of science is to find satisfactory explanations of whatever is in need of an explanation, and finding that as science progresses the need to explain laws of nature becomes more pressing yielding higher and higher levels of explanation, Popper sees himself gradually forced to a notion of explanation which *violates* more and more of his original explicitly posited and presupposed conditions of adequacy. Though originally the explanandum had to follow from the explanans, now they are incompatible. Again, though originally explanans and explanandum were required to be recognized as true, now they may be false, and so on. But Popper never explicitly recognizes that the concept of explanation with which he has saddled himself and which *contradicts* the one with which he began.

In fact, neither in the papers under discussion nor in later writings does Popper *ever* give a precise statement of approximative deduction. In the early sixties the term was adopted in quite different quarters; people like Feyerabend, Hempel, Nagel and others used it. Hempel even coins the term 'approximative D-N explanation'[13], and it is somewhat surprising to see Nagel talking about the matter as if this was the sort of explanation he was talking about all the time. He says[14], for instance, that "it is to be expected that the laws derivable from Newtonian theory do not coincide with some of the previously entertained hypotheses ..." For it is "a widely recognized function of comprehensive theories ... to specify the conditions under which antecedently established regularities hold, and to indicate, in the light of those conditions, the modifications that may have to be made in the initial hypotheses ..." Nagel further reminds us that "there are relatively few deductions from the mathematically formulated theories of modern physics in which ... approximations are not made, so that many if not all the laws commonly said by scientists to be deducible from some theory are not strictly entailed by it."

Now, this may sound good, but it is simply not part of Nagel's original notion of reduction. The notion of an approximative explanation or reduction is distinct from the original notion, and although the difference must not be exaggerated it has to be taken seriously. The new notion, badly in need of explication, will be discussed in a later chapter. For the moment I shall confine myself to some illustrations and some general comments.

A well known improvement of the oldest gas law, the law

(1) $$pv = RT$$

for the ideal gas, is van der Waals' law

(2) $$(p + \frac{a}{v^2})(v - b) = R \cdot T$$

Since the van der Waals law takes into account the finite volume of the molecules and an internal force affecting the pressure, it should be noticed first that as long as

(3) $a > 0 \quad \text{or} \quad b > 0$

the ideal gas law and van der Waals' contradict each other: there is *no* state (p, v, T) of the gas common to both laws. However, it can readily be seen that for

(4) $a \ll pv^2 \quad , \quad b \ll v$

a triple of values (p, v, T) satisfying (2) will very closely approximate a triple (p', v', T') satisfying (1). This approximation is *asymptotic* in the sense that no definite limits for the state parameters p, v, T are reached. But the better the approximation conditions (4) are satisfied, the smaller will be the mistake in adopting solutions to (1) instead of solutions to (2). In the language introduced in the previous chapter, the ideal gas law is a *limiting case* of the van der Waals law.

A physical law may have more than one interesting limiting case. Such is the case with Planck's radiation law

(5)
$$\varrho = \frac{8\pi h\nu^3}{c^3} \cdot \frac{1}{\exp\left\{\frac{h\nu}{kT}\right\}-1}$$

mentioned earlier. Before Planck quite different radiation laws existed: Rayleigh's law

(6)
$$\varrho = \frac{8\pi\nu^2}{c^3} \cdot kT$$

and Wien's law

(7)
$$\varrho = \frac{8\pi h\nu^3}{c^3} \cdot \exp\left\{-\frac{h\nu}{kT}\right\}.$$

The former was well confirmed for small frequencies

(6a) $h\nu \ll kT$,

the latter for large frequencies

(7a) $h\nu \gg kT$.

Again it is easily seen that these are approximation conditions showing the two earlier laws to be limiting cases of Planck's law. (As to the constants contained in them, (6) and (7) are reformulations adapted to the state of affairs *after* Planck.)

Given two examples, one may ask: In what sense are these examples approximative explanations? And do they exemplify what Popper has said of this kind of explanations? Without pretending to give a precise explication of approximative explanation I shall make some general remarks in response to these questions. This is to set the background for the general definitions to be given later.

Let me begin by saying what I mean when I say that the van der Waals law and the Planck law are *minimal covering laws* of their limiting cases. Roughly, a law is a covering law of another or - equivalently (!) - the latter is a limiting case of the former if the first approximates a *considerable portion* of the second, even becoming *asymptotically equivalent* to the latter within that portion. A covering law is minimal if it is a *weakest* law covering the limiting case.

If L^*_1 is a minimal covering law of L one could equally well say that L^*_1 is a *correction* of L. In the literature one occasionally finds this terminology, and what people supposedly want to express is essentially what I prefer to call a minimal covering law, borrowing an expression from the physicist Fritz Rohrlich and explicating what he presumably meant by it.[14a] The first to have introduced the term 'corrected law' seems to have been Kenneth Schaffner in a paper of 1967. He says there [symbols changed]:[15]

> L^*_1 corrects L in the sense of providing more accurate experimentally verifiable predictions than L in almost all cases ... and should also indicate why L was incorrect ... and why it worked as well as it did.

Schaffner even makes the concept of a corrected secondary theory part of an interpretation of Popper's idea - and, by the way, also of Feyerabend's and Kuhn's thus unwarrantedly lumping together these authors' views in a "PFK paradigm" for reduction. The idea is that approximate reduction proceeds in two steps: The first step is an *ordinary* deduction

(8a) $$L_1 \wedge C_1 \vdash L^*_1$$

of the corrected law L^*_1 from the reducing law L_1. The second is the decisive *approximative* deduction

(8b) $$L^*_1 \wedge C \mid\!\!\approx L$$

of L, the law to be reduced, from the corrected law L^*_1 Thus the corrected law is *inserted* between the reducing and the reduced law, achieving an interpolation between them. This certainly is in the spirit of Popper whom I have already quoted:

> The original explanatory task was the deduction of the earlier results [L]. Yet this task is discharged, not by deducing these earlier results but by deducing [(8a)] something better in their place [L^*_1], new results which, under the special conditions of the older results [C], come numerically very close (8b) to these older results, and at the same time correcting them.

So, according to Schaffner's explication, reduction or explanation is a *three termed relation*: apart from the (usual) obscurity of the auxiliary assumptions C and C_1, in Schaffner's account the three terms to the explanation/reduction relation are the reducing law, the reduced law, and, in addition, the corrected law, - the law correcting the reduced law in the light of the reducing law. This account answers the question raised in the previous section: What happens to the reduced theory during reduction, or: how is a reduced theory reconstructed within the reducing theory? In approximate deduction it is *a priori* much more plausible that the minimal covering or corrected theory will be different from the theory to be reduced.

Against this background of general considerations, if one looks back at the two examples, one is in for a surprise. For there one of the participants in reduction is simply missing: there is no third law interpolating between the van der Waals law and the ideal gas law, or between Planck's law and Rayleigh's or Wien's. But there is a simple reason for this ommission. In these cases the reducing law is just *identical* with the corrected law: it *is* the correction of the reduced law. In proper theory deduction this is impossible except in the trivial case in which nothing happens *at all*. This shows clearly the difference between exact and approximative reduction even in this special case. For approximative reduction examples illustrating the general case are readily at hand. Favorite examples are Galileo's law of free fall and Kepler's 3^d law. Galileo's law may be written

$$(9) \qquad v = - [2g\ (h - x)]^{\frac{1}{2}}$$

for the velocity v of a body falling free from the initial height h, i.e. $x = h$, to the bottom, i.e. $x = 0$. This is the law to be explained. The explaining theory is the very comprehensive theory consisting of Newton's general mechanics and his gravitational law. This theory may be be symbolized as

$$(10) \qquad CM \wedge GL \quad .$$

The question then is: What follows from *this* theory vis a vis a freely falling body? The answer is: *Not* Galileo's law *but*, via suitable auxiliary assumptions expressing the special conditions, the corrected law

(11) $$v = -\,[2g\,(h - x)\,(1 + \tfrac{x}{R})^{-1}\,(1 + \tfrac{h}{R})^{-1}]^{½} \quad ,$$

where R is the radius of the earth. So if

(12) $$h \ll R \qquad [\text{mind } 0 \leq x \leq h]$$

we get the second half of the approximative deduction, the deduction from (11) to (9). Similarly, Kepler's third law is

(13) $$\frac{a^3}{T^2} = \lambda$$

where λ is a constant, independent of the planet. If, however, the case is treated as a 2-body problem in the theory (10), then we get the corrected law

(14) $$\frac{a^3}{T^2} = \lambda\,(1+\tfrac{m}{M})^{-2}$$

with m the mass of the planet and M that of the sun.

(14) approximates (13) if

(15) $m \ll M$.

So far as the relation between the corrected, or minimal covering, law and the law to be explained is concerned, the new examples share all essential features with the earlier examples. In partciular we have a covering law explanation in the strong sense that, although the relation is only asymptotic, the two laws are *practically identical* on a topologically *thick* subset of their theoretical domain of application. This is an immensely important feature of theory explanation as will be clear in the next chapter, *if* theory explanation is to become a concept essential for the description of scientific progress. On an intuitive level of understanding the notion in question can be illustrated by a striking counterexample to strictly deductive theory explanation. Contrary to what you will find in the literature, Newton's theory and Kepler's laws *admit common solutions* if the number of bodies participating in the total motion is greater than 2. This means that from Newton's theory and appropriate initial conditions (leading to a common solution) Kepler's laws can be deduced in the strict sense. When I offered Hempel these cases as examples of strictly deductive theory explanation he rejected them, arguing that they only show that under certain special circumstances Newton's and Kepler's theories can be used to explain the same phenomena. Probably the reason why we eschew these cases as theory explanations, despite their quite ordinary deductive character, is because they do *not* cover a sufficiently large portion of the theoretical domain of application of Kepler's laws.[16]

The requirement that a sufficiently large region of the domain of application must be covered by the limiting case law is typical of theory explanation in contrast to event explanation. It makes no sense for the latter. The idea of a minimal covering theory or correction was outlined at the end of the previous section. The schema describing domain restricting deduction would now have to be modified to accommodate the idea of approximation. If approximation is taken literally the new schema would be simpler because in the asymptotic limit domain restriction yields a *vanishing* domain of validity for the reduced theory. In an approximative deduction, which is, as will emerge shortly, necessarily conditional, we have

$$\begin{array}{l} L_1 \\ \downarrow \\ L_1^* \rightsquigarrow L \end{array}$$

Only if the approximation is interrupted we again have the square:

$$\begin{array}{ccc} L & & L \\ \top & & \top \\ | & & | \\ L^*_I & (\mid\!\sim) & L^* \end{array}$$

Here L* is a domain restricted L, and ($\mid\!\sim$) means that a (conditional) deduction is possible only up to the limit of accuracy chosen for the approximation. It is this situation that characterizes physical practice.

The previous section stressed the importance of explicit auxiliary assumptions in the explanation of theory. The circumstances typical in approximative deduction convincingly show such auxiliary assumptions to be *inevitable*. Assume that in the approximative case

(8b) $\qquad L^*_I \wedge c \mid\!\sim L$

c does not occur. Then we have

(16) $\qquad L^*_I \mid\!\sim L \quad .$

But, as will emerge in greater detail later, this is essentially an ordinary implication

(17a) $$L_1^* \vdash L\,[u]$$

with an approximation parameter u such that

(17b) $$L \vdash L\,[u]$$

for every u. Since (17a) holds for every u, in virtue of (16), then, since L_1^* does not depend on u, it will strictly imply L in view of (17b). But this is contrary to the assumption of a genuine case of approximation. So we see that only in the presence of an additional premise c, depending on another approximation parameter ε, does there exist a genuine approximative explanation. The meaning of (8b) will then be that however 'small' the parameter ε may be chosen there exists u such that

$$L_1^* \wedge c\,[\varepsilon] \vdash L_1\,[u]$$

in the sense of an ordinary implication.

I shall conclude this section by drawing attention to an apparently serious problem besetting approximative explanation. It is the problem that approximations can have different orders: there are more or less *good* approximations. And if we allow every order, including very bad approximations, it seems that we can explain more than we would want to. The examples considered so far are *very* good, very smooth approximations. But imagine somebody, less inspired than Galileo, coming to believe that in free fall not acceleration but velocity is constant. Although globally this is grossly false even this belief can be approximated by Newton's theory *if* sufficiently small time intervals are allowed. And *some* such restriction of the domain of applicability had to be made in *all* cases previously discussed. Moreover, the last consideration has shown that this kind of situation has to be expected in general. So where are we to draw a borderline between the restrictions to be permitted and those to be forbidden? I think the answer is that we cannot without introducing an element of arbitrariness. We are, therefore, much better off to allow all order of approximation based on the following justification, full appreciation of which must be postponed until the next chapter. As already noted, theories to be explained approximately are *known* to be false. What we want to, and actually can, explain is their *empirical success*. But that is very much a matter of degree, the most important parameter being the accuracy of measurement underlying success. Different orders of approximative explanations, therefore, should not be avoided or suppressed. Rather they are to be made explicit according to the circumstances of the case.

IV.3 Inhomogeneous Reduction

In the two preceding sections of this chapter deductions, proper or approximative, were the main theme, and it was possible to repress conceptual or definitional problems for the time being. The investigation was deliberately confined to homogeneous reduction where all primitive concepts of the reduced theory are primitive concepts of the reducing theory. If this condition is not fulfilled, if there are primitive concepts of the reduced theories not occurring among the primitive concepts of the reducing theory, then the following two problems immediately arise:

First, how can the reduced theory be *derived* from the reducing theory?

Second, in what sense, if any, can those critical concepts of the reduced theory be *affiliated* with or assimilated to the reducing theory?

It were problems such as these that made Nagel think that only the inhomogeneous reductions present any interesting philosophical problems.[17] His solution to the first problem was the introduction of *bridge laws* containing concepts from both theories and added to the basic assumptions of the reducing theory. But are bridge laws also the answer to the second question? They would no doubt *connect* the primitive concepts of the reduced theory that are not among the primitive concepts of the reducing theory with primitive concepts that are in the reducing theory. But is this *sufficient* for a genuine reduction? Can just *any* empirical statement correlating the concepts in question suffice for reduction, or would not one have rather to *define* the critical concepts of the reduced theory via concepts of the reducing theory in order to affiliate them with it? Nagel's discussion of these questions has been criticized by several authors, among them Schaffner, Sklar, Causey and, recently, Aronson.[18] The suggestion commonly adopted is that something between the two possibilities just mentioned would do the job. The bridge laws would have to be 'synthetic identities', à la Frege's statement of identity between the evening star and the morning star. Some people have carried matters so far as to *replace* deductive reduction by so-called 'ontological' reduction in the belief that ontological reduction and deduction are mutually exclusive.

It seems clear from the extant relevant literature that inhomogeneous reduction is in particularly poor shape. The literature is so confused and muddled that I will not attempt to review it in greater detail here. Rather

I will confine myself simply to expressing my own view on the subject. For clarity I will assume that the theories with which one must deal are manysorted theories of higher order type logic. This, in fact, was implicit in the discussion in the previous two sections. But since I want to include the homogeneous case in the following treatment I now want to make this assumption explicit.

In what follows the main idea is to treat the problem of reduction in terms of *theory extensions*. Extending a theory can mean either

1) extending its *language* by introducing new symbols

or

2) extending its *axioms* by adding new ones.

If theories are interpreted in the way we normally assume in the case of physical theories, then step 1) would include also the *interpretation of the new symbols*. With the concept of theory extension in hand, the concept of reduction can be rephrased in the following way. Let T′ be the reducing theory, T the theory to be reduced. Then T reduces to T′ if and only if the axioms of T follow, if only approximately, from an extension of T′. The main problem of theory reduction then can be reformulated as the question: What extensions of the reducing theory T′ lead to genuine reductions? And this in turn can be reformulated as the question: What extensions do not affect the reducing theory T′?

With this understanding of reduction in hand, I shall begin with an observation which I will not, however, further elaborate here. Let us assume that the reducing theory is *pure* in the sense that its axioms do not contain non-definable descriptive constants of the type of a variable that is quantified over in the axioms. Now the debate over what a *lawlike* sentence is is well known. Though I don't know what the complete answer to the question "When is a sentence lawlike?" is, I think a *necessary* condition of being a lawlike sentence is its purity in the sense just defined. So it is natural to assume that the reducing theory is pure. But if it *is* pure, then strictly speaking there is *no homogeneous reduction* with such a theory taken as the reducing theory. For the homogeneous adding of axioms (a simple extension as it is sometimes called) *would* change the theory. One might try to escape this result by trying to find a class of absolutely contingent statements in the language of the theory such that if *they* are added, the lawlike status would get lost and would thus show that no change has taken place. Perhaps this is possible. But the examples from physics point rather in the opposite direction. This is, of course, but another way of saying that there seems to be no *syntactical* condition beyond purity that is helpful in characterizing lawlikeness. However this may be, in the homogeneous reductions of the previous sections - the reducing theories were *applied* theories. And for them extensions can be homogeneous in the sense that the underlying *pure* theory is not changed also by this second extension *if* such extentions are required to be *conservative*, i.e. that no new theorems formulated in the *original* pure language become provable. So it is certainly a necessary condition for an extension to yield a proper inhomogeneous reduction that it be a conservative extension. That it is not a sufficient condition is shown by the following example.

Strictly speaking an event explanation is the paradigm of an inhomogeneous explanation. The introduction of singular statements to describe events or phenomena requires the introduction of names for particular individuals that are as a matter of principle excluded from universal theories. Event explanation is, therefore, definitely conditioned by *language extension*, and the statement to be explained contains expressions *that do not belong to the language of the explaining theory*. So here we have *clear cases* of inhomogeneous reductions that can be handled with the method of theory extensions: The explained singular statements are conse-

quences of (inhomogeneous) extensions of the explaining theory. On the other hand, it is easy to find examples to the contrary. Classical mechanics, for instance, clearly is an extension of euclidean geometry: we add both to the language and the axioms of euclidean geometry when we do mechanics. But nobody would dream of thinking that the theorems of mechanics had been reduced to geometry. Why? Because the language extension goes too far. We have simply
- *an escapade into a different matter*. In mechanics, one talks not only about geometrical objects, e.g. distances, but also about masses and forces, objects which simply do not belong to the subject matter of geometry. By contrast, in making those singular statements occurring in event explanation we did *not* leave the universe of discourse of the explaining theory. We just decided to talk about some *particular* items belonging to that universe.

Does this illustration suggest how the wanted explanation has to go? Probably not. But it is something to build upon. Before taking the next step let me tell you where it leads. As is well known, a much deplored fact in scientific theorizing is that the theories of science are *interpretationally underdetermined*. When a given empirical theory of scientific stature is analyzed in the usual way separating form and content, one can be pretty sure that there will be meaning gaps: the formalism of the theory can be only partially interpreted. No problem has occupied philosophers of science, especially logical empiricists, more than the 'problem of theoretical terms'. In the orthodox problem of inhomogeneous reduction we are faced with precisely the opposite situation: we are faced with *interpretational overdetermination*. Interpretational overdetermination arises from the restrictions imposed on inhomogeneous theory extension in order to yield genuine reductions. This is to be expected because those restrictions *force* concepts excluded from the set of primitive concepts of the reducing theory on the basis of *this very same theory*. How could it be otherwise than by overdoing the interpretation, by letting these concepts yield unneeded additional information vis a vis the initially assumed interpretation of the reduced theory?

One kind of language extension not introducing any new subject matter and *therefore* leading to proper reductions consists in adding primitive constants of the type of the variables in the reducing theory and giving these constants referents that are elements of the domains over which the corresponding variables are permitted to range. This is a straightforward generalization of what is usually done in event explanation. We are doing what is usually done in applying a theory. Pure theories of science are characterized by containing no such constants in the primitive framework. But their applications usually are accompanied by extensions containing constants of the described kind. The sense in which the newly introduced concepts belong to the reducing theory is quite clear. Moreover, since progress usually involves generalizations it is also clear that the new concepts, and with them the statements to be reduced, probably have existed *before* the reducing theory became known. Finally, the extensions in question *may* lead to interpretational overdeterminations in the sense that the initial theory *reduces* contingent information expressed by means of the added constants by *relating* otherwise unrelated statements, enabling the elimination of some of them.

To see how this comes about consider next extensions by *definitions*. A definitional extension involves the introduction of a new term (and possibly an associated axiom) such that the *interpretation* of this term is brought about by the very procedure of its formal introduction, a procedure which reduces its meaning to meanings already available. The answer to the question what the defined term means can *only* be given by pointing to its definition. It is for this reason that, contrary to what some people seem to think, definitions *cannot* be used to be bridge laws. The terms of the theory to be reduced, in so far as they are not among the primitive terms of the reducing theory, are terms *independent* of the reducing theory. So they cannot have been established by a definitional extension of theory. Such definitions are, of course, dependent on the theory in the strong sense that they cannot be given unless some existence and uniqueness theorems are provable in the theory. A definition is meant to *confer* a meaning to a term, not to reduce an independently given meaning.

Although definitions by themselves cannot function *as* bridge laws they can function *in* them. In these cases reside the kind of extensions that *do* lead to proper inhomogeneous reductions. Let us assume $\tau\ [\alpha_1, \ldots, \alpha_m]$ to be a defined term in the reducing theory. Let β be a constant of the same type as τ. Finally, let us assume that β has been given an *independent* interpretation. Then the equation

$$\beta = \tau\ [\alpha_1, \ldots, \alpha_m]$$

is a possible bridge law. As already mentioned bridge laws of this kind are sometimes called 'synthetic identities'; they are synthetic because the meaning of ß is independently established from the meanings of the α's. It is, however, more important to point out their interpretational overdetermination.

Every empirical correlation is also synthetic, and in *some* sense can be given by an identity. Carnap, for instance, is thinking of simple empirical laws expressed by an equation when he says that "not only can an electric current be measured by measuring a temperature but also, conversely, a temperature can be measured by measuring the electric current produced by a thermoelectric element. Therefore there is mutual reducibility between the terms of the theory of electricitiy, on the one hand, and those of the theory of heat, on the other." This kind of reduction is, of course, not what I am now talking about when I speak of interpretational overdetermination. Rather I have in mind 'interpretational overdetermination' in the sense that, given the meanings of the primitives $\alpha_1, \ldots, \alpha_m$ the term $\tau [\alpha_1, \ldots, \alpha_m]$ *also* has a meaning and *could*, therefore, in principle do the job that ß does. We *could* have defined ß but actually it has an independent interpretation.

Let me illustrate in the very common business of forecasting. If we predict the planet Mars to be in a position x at some future time t then we have to distinguish the statement about the *predicted* position from the statement about the *actual* position of the planet at time t. If the statement about the predicted position at time t and the statement about the actual position at time t had the same meaning we would never be able to *test* the prediction. The equation equating the position delineated by the prediction and the actual position relates two different meanings because they are thought to have the same referent. Whether in fact they have the same referent or not is an empirical question, but the meanings are different and clearly illustrate an interpretational overdetermination. On the assumption that the theory is correct we would not need more information than that given by the theory and a complete set of initial data.

In forecasting, or for that matter in the corresponding explanations, the definitions entering the bridge laws depend very sensitively on the explaining theory. This is, however, not the rule. For instance, the definition of the mean kinetic energy of gas molecules entering the famous synthetic identity between it and temperature does not essentially depend on the assumptions made by the kinetic theory of gases. The validity but not the synthetic character of the identities in question is dependent on the theory. Interpretational overdeterminations abound in physics just as do underdeterminations. They are mixed up with each other almost inextricably. It is particularly important for philosophers of science to restore this unpleasant symmetry constantly broken in the usual presentations of physical theory. In textbooks of theoretical physics there even occurs the opposite symmetry, that is, the symmetry that both kinds of determination are absent. The theories are presented via certain primitive concepts the sound meaning of which is never questioned. Correspondingly, the axioms of the theories are presented in the reduced form, the bridge laws being eliminated by means of, as they are sometimes called, 'redefinitions'. It should be pointed out, however, that physicists have every reason to resort to such simplifications. This section concludes by explaining why this is so.

One of the more difficult comparisons in physics is that between pre-relativistic and relativistic spacetime. Pre-relativistic or galilean spacetime may be formulated by taking the concept of absolute time as a primitive concept. But there is no corresponding formulation of relativistic or minkowskian spacetime, the concept of absolute time is simply absent. In view of the possible reduction of galilean spacetime theory to the minkowskian theory there seems to be here a particularly strong case for inhomogeneity. But, in fact, given *any* formulation of the Minkowski theory the concept of absolute time cannot be regained by a synthetic identity *strictu sensu*. On the other hand, there *are* reformulations of either theory using the *same* primitive concepts, i.e. we have the situation

$$S(\alpha) \simeq S'(\gamma) \quad \text{(galilean)}$$
$$T(\beta) \simeq T'(\gamma) \quad \text{(minkowskian)}$$

with strict (!) equivalences $\simeq$, and γ being the new common primitive basis. Such reaxiomatizations are among the most powerful tools in current theory reduction concerning the more sophisticated cases. They allow us to eliminate conceptual disparities or, rather, to understand them on the ground of a common conceptual basis. In the case before us we see, for instance, that the concept of absolute time can be reintroduced in galilean spacetime because the axioms $S'(\gamma)$ fulfill the necessary presupposition for this definition. By contrast, the very same concept *cannot* be introduced into Minkowski theory because $T'(\gamma)$ *doesn't* fulfill those conditions. This is precisely one of the things that make the two theories $S'(\gamma)$ and $T'(\gamma)$ contradictory to each other.

Now, this kind of conceptual assimilation certainly is a success in the comparative analyses of theories. But for the time being the price we have to pay for it is the complete lack of understanding about what the equivalences mean when it comes to interpretational problems. I will return to this problem.

NOTES

[1] Weizsäcker [1980], p. 120
[1a] Cf. Siemens [1971], pp. 532 f.
[1b] Besides the references given in this section see also Braithwaite [1953], Ch. XI, Woodger [1952], pp. 271 ff. and Eberle [1971].
[2] Hempel [1965], p. 337
[3] ibid. p. 248
[4] ibid. p. 273
[5] Nagel [1961], p. 338
[6] Nagel [1949], p. 119
[7] ibid.
[8] Yoshida [1977], p. 1
[9] Popper [1974], p. 357
[10] ibid. p. 202
[11] ibid. pp. 357 f.
[12] ibid. p. 202
[13] Hempel [1965], p. 344
[14] Nagel [1970], p. 120 f.
[14a] Rohrlich [1965], ch. 1
[15] Schaffner [1967], p. 140 and 144
[16] Scheibe [1973a]
[17] Nagel [1961], pp. 338 f.
[18] Schaffner [1967], Sklar [1967], Causey [1977], Aronson [1985], Chs. 7 and 8

V. The Impact of Experience [and Facts]

In the previous chapter it was shown that whatever is achieved by a proper deductive theory reduction, it is not a genuine correction of the reduced theory. This could be established independently of any questions of confirmation or disconfirmation of the theories involved. It is sufficient to observe that in proper deductive reduction the theory to be reduced either is exactly reproduced or, by conditional deduction, is restricted to a subdomain of the original universe of discourse. The first case needs no comment. The second case may involve a slight improvement of the theory in the sense that its reconstruction within the reducing theory avoids falsifying instances; the restriction is a restriction to its proper domain of validity.

In the second section of the previous chapter reductions by approximative deduction were admitted. Approximative deduction necessarily is conditional and therefore the theory to be reduced is modified of necessity. Its 'natural' successor is the minimal covering theory contradicting the original theory, and in this strong sense approximative reduction makes room for empirical improvements. But it does not guarantee them. There is no reason whatever to expect an approximative reduction to improve on the reduced theory irrespective of previous experience or the facts. A theory reduction needs empirical test for essentially the same reasons the theories themselves require test.

But though theory testing is commonplace, the empirical import of reductions plunges one into territory that is much less well mapped. Indeed there are few if any papers in which the problems in this connection are plainly stated let alone their solutions. This chapter is an honest effort to rectify the neglect.[1]

One could approach the matter by imitating the wellknown conceptions of theory confirmation. A deductive reduction would then be confronted with observational data and one would have to say what it means for the reduction, viewed in the opposite direction, to lead to empirical improvement of the reduced theory based on those observational data. I choose a different approach which consists first in forgetting about deductive reduction for a while and then looking for a different kind of reduction in which the empirical or factual situation is explicitly taken into consideration. One could even develop nothing but concepts of empirical theory comparison, i.e. concepts explaining what it means to say that given such and such empirical evidence, one theory is better confirmed than another.[2] But then it would be a bit odd to speak of reduction. On the other hand, it might still be possible to develop relations strong enough to be called reductions while making use explicitly and exclusively of some given corpus of empirical data or facts. However it is done, it will be only after it is done that deductive reduction will be reexamined and the wanted empirical justification produced. To this end it would have to be shown that deductive reduction implies the new kind of data-related reduction. In actual historical cases where deductive reduction is possible we would then know to what extent we can rely empirically on the reduced theory from the viewpoint of the reducing theory. The suggested procedure also has the following pleasant side effect. We are not married to deductive reduction even in its approximative version. This is pleasant because for all of its importance even this concept of reduction is too narrow to be successfully applied to extant historical cases.

Theory reduction explicitly involving the empirical situation brings to light a new kind of parameter. In the previous chapter I argued that it is mistaken to think of theory reduction as a two-termed relation; there is always a third parameter relating the domains of application, or - equivalently - an additional assumption mediating the deduction, proper or approximate, of one theory from the other. Euclidean and hyperbolic geometry, for instance, do not just stand in the relation of contradicting each other. They do stand in this relation if their basic terms are given the same interpretation, and this is what we tacitly assume whenever we think of these theories as contradicting each other. However, we need not relate their domains of application in this way. Rather there are interpretations of hyperbolic geometry in a model of euclidean geometry. And if related in that way hyperbolic geometry <u>follows</u> from euclidean geometry together with an auxiliary assumption expressing the mentioned embedding.

The new parameter in theory reduction then is the description of the relevant empirical data or, if you will, of the relevant facts. The ambiguity of such expressions as 'empirical data' and 'facts' gives rise to various possibilities as to what is actually meant by new parameter. Unfortunately, it is not possible here to make these concepts much more precise though there is the distinction between facts on the one side and empirical data on the other.

If the word 'fact' is allowed to stand for a structure or a common possible (!) model of the two theories to be compared, then <u>the comparison</u> would be one of their <u>truthlikeness</u> or <u>verisimilitude</u> with respect to that universe of discourse. There have been a great number of explications of the idea of truthlikeness lately.[3] But they have one thing in common: Since they deal with truth and falsity of the basic assumptions of the theories, the <u>whole</u> universe of discourse must be taken into account and not only that part of it which we happen to know by experience. This is obvious because in general the truth and falsity of a statement depends on what the facts are everywhere in that universe.
The inclusion of truthlikeness in matters of progress is suggested by the fact that the existing explications of it were meant to be used in the explication of the idea that the progress of science

consists in a gradual approximation of the truth or of what the world is really like or of what have you of this kind of metaphysical stuff. On the other hand, concepts of progress by empirically justifiable reductions, although not expressing it, would, I think, not exclude the possibility that as a matter of brute fact we approach the truth also by them. It seems, therefore, worth asking whether conceptions that make this idea explicit can be viewed, so to speak, as metaphysical completions of those other closer-to-earth concepts.

Turning now to the case where the new parameter is empirical in nature, it seems expedient to distinguish roughly the following two subcases. The original idea of invoking experience in matters of reduction is, of course, the same as anywhere else: the experiential material to be invoked should be as immediate or as elementary as the theoretical context allows it. In other words, we would introduce the experiential material consisting of what is usually called the observational data. This is one of the two subcases. The second subcase, occasionally found in the literature is the case where the new parameter in theory reduction is not the observational data but some well confirmed lower-level theories derivable from the theory to be reduced. The reduction then amounts only to a reduction, in a sense to be specified, of those lower-level theories. With respect to the theory about which one would, perhaps improperly, say that is has been reduced, this reduction obviously would only be partial. As it will turn out the two empirical cases thus distinguished will together make up what in the introduction was called 'reduction by explaining the explained.'

V. 1. Reference to observational data: Historical Account

Let us begin with reduction, or more generally,progress seen in the light of observational data. Remember that the account inchapter II of the progress in physics was not given in terms of reduction or explanation. Of course, these terms were not purposely avoided. Rather they are not to be found in physicists' talk. As already noted these accounts are hopelessly incomplete from the standpoint of philosophy of science. But since it was decided to ignore established conceptions of reduction for a while, there is every reason to look back at what the physicists have taught us.

Recall Nernst's assessment of the advance made by Einstein's gravitational theory when compared with Newton's.[4] He assured us that the modifications to be made with respect to the latter theory are so small that they can be neglected for all pratical applications "except for the computation of the orbit of Mercury." He then said that "as a matter of principal every computation that astronomers have performed so far must be changed." Putting aside Nernst's emphasis, for the moment the important thing is that he is here talking about the application that is made of the two theories in question to the particular case of the solar system. And with regard to this special application he says that the two theories lead to approximately the same predictions which according to the available accuracy of observation can be discerned only for one case. (The angular displacement of the perihelion of Mercury, is 40" per century.)

The principal idea behind this remark of Nernst's is:

1) Under certain conditions of application a new theory does not appreciably improve on the successful predictions already made by an older theory under challenge by the former. Under those conditions, that is, the new theory can do no more, <u>but also no less</u>, than reproduce the empirical successes of its predecessor within the margin of observational error.

2) Under certain conditions <u>different</u> from those mentioned in 1) the predictions of the new theory <u>are</u> appreciably closer to the observational data than were those of the old theory.

So here we have a tentative formulation of the relation between two theories one of which agrees better <u>with a given bunch of observational data</u>. The formulation implies that such agreement is only approximate according to the available accuracy of measurement. Correspondingly, the relation between the two theories mediated by the observational data is also only <u>approximate</u>.

Given this discussion among certain physicists, it is quite curious that the only philosophers of science who explicitly face the problem of empirical justification soon try to rid themselves of it. Moreover, although aware of the necessity to take experimental error with all its theoretical consequences into account, they do not do it on the ground that "then the problem of reduction becomes hopelessly complex". Such is the case in Kemeny and Oppenheim's famous paper 'On Reduction'[5]. In their paper Kemeny and Oppenheim start from a critical examination of Nagel's (and Woodger's) definition of reduction. Most importantly they mention "two oversimplifications in both the Woodger and Nagel definitions". One is the neglect of the approximative aspect of theory reduction. But, as already mentioned, Kemeny and Oppenheim are themselves guilty of the very same oversimplification whose disastrous consequences is even more evident in their case. The second "most important" point has to do with Nagel's and Woodger's restriction of the bridge laws to biconditionals. This criticism is justified, but it is a trivial matter just to omit the requirement of biconditionality. Moreover, the authors exaggerate the importance of this move when they say that "most examples will under no circumstances fall under this pattern."

The essence of Kemeny's and Oppenheim's attempted improvement is contained in the sentences that open their section 'New Definitions'. There they say first:

> As we see it, the essence of reduction cannot be understood by comparing only the two theories; we must bring in the observations.

The curious thing about this promising suggestion is that the authors do <u>not</u> emphasize the necessity of an empirical justification of theory reduction as I have done it at the beginning of this chapter. Instead they continue:

> It is not the case that the vocabulary of T_2 [the reduced theory] is in any simple way connected with the vocabulary of T_1 [the reducing theory], but only that T_1 can fulfill the role T_2 played, i.e. that it can explain all that T_2 can and normally more.

Now the second part of this sentence brings in the observations in a manner that one would expect if one wanted to have an empirically satisfactory concept of theory reduction. For it turns out that, given some corpus of observational data O, the second part of the sentence means something like

1') $$T_2 \wedge O' \vdash O'' \Rightarrow T_1 \wedge O' \vdash O''$$

for all subsets O', O" $\subseteq$ O for which the implications $\vdash$ above lead to explanations in the sense of Hempel and Oppenheim;

2') $T_1 \wedge O_0' \vdash O_0''$ but not $T_2 \wedge O_0' \vdash O_0''$ (perhaps even $T_2 \wedge O_0' \vdash \neg O_0''$ in at least one proper explanatory case where $O_0', O_0'' \subseteq O$.

Now, although this obviously is one way to make the reduction of T_1 to T_2 dependent on a set O of observational data, in a sense it commits the original sin as developed in the previous chapter.

Before reexamining the original sin in the present case, it is better first to understand what the first part of the sentence quoted above means in the current context. The main point Kemeny and Oppenheim want to make is as follows: For them the two theories T_1 and T_2 are formulated in theoretical languages which may be quite different. By contrast, the two theories are assumed to have the same observation language to which they are related by suitable correspondence rules. The introduction of observational data is, therefore, only a means to an end: the two, possibly widely differring theories shall be made comparable by their respective 'observational consequences'. Now this is a praiseworthy enterprise in itself. But unfortunately the authors allow themselves to be carried away by it: After having given the 'definition' 1') and 2') of data dependent reduction they say:

> The above is a very restricted notion of reduction, but no doubt some authors have this in mind. If we do not want to put a particular value of O into our definition we must eliminate the undesired variable by quantification. And this seems to lead to a very fruitful approach.

Thus if Red (T_1, T_2, O) abbreviates 1') the new concept of reduction would be given by

(KO) Red (T_1, T_2) iff for all O: Red (T_1, T_2, O)

Now quantification as such certainly is not the principal mistake made in this approach. But as soon as we start varying, as we do in quantification, the set O of observational data, we must compensate for a possible loss: the set O in 1') and 2') was assumed to consist of true observational statements or, if not true, then at any rate of statements, we have strong reasons to accept as being approximately true. If this is presupposed then the statements 1') and 2') are sound expressions of a relation between T_1 and T_2 with respect to the real state of affairs as far as is described by O. But if the set O is allowed to be varied within the set of all logically possible sets of observational data - and this is what Kemeny and Oppenheim allow - then in statement 1') the empirical failures of theory T_2 also come into play, and every such failure will be foisted upon the reducing theory T_1. If, for instance, Kepler's theory were applied to the moon its predictions would certainly come out false. But, being logically possible observation statements, via condition (KO) every theory reducing Kepler's would relentlessly be infected by those false predictions. If this be progress, make the most of it!

The way out of this impasse is to conditionalize the statement 1') when arbitrary observational reports O are considered. I say: "again" because we now have simply the empirical counterpart of the situation in which we found ourselves vis a vis deductive theory reduction. There, too, resort to conditional deduction was necessary to make room for actual improvements in the reduced theory. This connection is highlighted by theorem 2 in Kemeny and Oppenheim's paper. Although, as I said, the empirical justification of deductive reduction is not their major concern, much of what they do is precisely the kind of thing that one would do if that concern were one's major concern. Theorem 2 says that the Nagel-Woodger reduction, i.e. deductive reduction, is just a special case of the Kemeny-Oppenheim reduction (KO). Since the Nagel-Woodger reduction, apart from bridge laws, is unconditional deduction, the theorem reinforces the charge that Kemeny and Oppenheim commit the original sin.

The remedy which would make 1') an acceptable explication of 1) is to restrict the sets O', O" to cases that are compatible with a certain contingent condition c that would have to be added to T_1 as an additional premise if T_2 is to be deducible from T_1. This condition restricts the domain of validity of T_1 to a domain of validity of T_2. It is for this reason that the condition of factual truth of O can be waived provided 1') includes the compatibility condition formulated above.

Summing up, Kemeny and Oppenheim come close to presenting a theory having the form of an empirical justification of deductive reduction: they develop a concept of data dependent reduction and compare it with the Nagel-Woodger reduction. Secondly, the essential mistake in their work and its remedy was pointed out. To be sure, the all important approximative aspect must still be accommodated. But first the work of Popper and Lakatos must be mentioned.

Concerning the main problem of empirically justified deductive reduction, Popper and Lakatos do not even come as close to the matter as did Kemeny and Oppenheim. Indeed in his earlier papers on the matter, e.g. the one quoted in Ch. IV.2, Popper's way of expressing the matter is ambiguous; one doesn't know whether he is talking about reduction by deduction or reduction by explaining the explained phenomena. But in his later papers he seems to have in mind only the latter, data dependent, concept. Similarly, Lakatos' formulations are too vague to decide which of the two conceptions he is talking about. There is, therefore, little inspiration vis à vis important ideas on the justification problem to be derived from either Popper or Lakatos. But a brief look into their concept of explanation is called for because it is at least interesting to see how the two conceptions run together in each other's minds.

A fairly clear formulation of Popper can be mapped on the two-part formulation of progress of the physicists presented in Ch. II. In the first place Popper says:[6]

> ... in order that a new theory should constitute a discovery or a step forward it should conflict with its predecessor; that is to say, it should lead to at least some conflicting results. But this means, from a logical point of view, that it should contradict its predesessor: it should overthrow it.
> In this sense, progress in science - or at least striking progress - is always revolutionary.

Then there follows essentially the Boltzmann conception of the continuity of science:

> My second point is that progress in science, although revolutionary rather than merely cumulative, is in a certain sense always conservative: a new theory, however revolutionary, must always be able to explain fully the suc-

> cess of its predecessor. In all those cases in which its predecessor was successful, it must yield results at least as good as those of its predecessor and, if possible, better results. Thus in these cases the predecessor theory must appear as a good approximation to the new theory;

Lakatos formulates essentially the same relation when he says[7]:

> T_1 is 'superseded' and eliminated from the body of science ... on the appearance of a new theory [T_2] which has corroborated excess content over T_1 while T_1 has no corroborated excess content over T_2.

In another allegedly equivalent formulation the second half of this definition comes out as[8]

> T_2 explains the previous success of T_1, that is, all the unrefuted content of T_1 is contained (within the limits of observational error) in the content of T_2.

Both formulations imply (what Popper makes explicit), namely, that the two theories contradict each other. Correspondingly, as expressed explicitly by Popper but only implied by Lakatos, the relation between the theories involved is meant to be merely an approximate one. Finally, it is obvious that both authors, as do Kemeny and Oppenheim, cite the importance of observations. Such phrases, however, as 'corroborated excell content', 'unrefuted content' and 'unrefuted content being contained in another one within the limits of observational error' are hopelessly ambiguous because they contain elements of both deductive and data dependent explanations. What is needed, however, is a purely data dependent concept of explanation, one that might be helpful in an empirical justification of deductive explanation.

V. 2. Reference to observational data: Systematic Account

The following is a more systematic discussion of the problem.[9] What is to be addressed first is the concept of deductive reduction of a theory Θ by means of theory Θ_1 and the additional conditions C. I am thinking primarily of approximate and homogeneous reduction as discussed (and illustrated) in the previous chapter. I shall write

$$R[\Theta, \Theta_1, C]$$

for this relation. Second, the idea that Θ and Θ_1 can also be empirically related by a set Ob of actually performed observations must be addressed. The idea is that because of Ob the theory Θ_1 is empirically at least as well supported as is Θ. One could imagine a stronger relation to the effect that Θ_1 is indeed better supported than Θ, but what I want to try to make clear is already reflected in the weaker relation. I shall write

$$E(\Theta, \Theta_1, Ob)$$

for this relation. Since R included approximations, E should in general include the inaccuracy of measurements.

Although no particular relation E has been chosen (in contrast to R) we may provisionally ask: What is the problem of an empirical justification of R? Isn't it simply ridiculous to ask for an empirical justification of a deduction, if only an approximative one?

Here I shall repeat what I emphasized in the introduction to this chapter, namely, the excellence of deductive reduction as a theoretical means to express progress has only been assumed and not argued for. The question why the transition from a theory Θ to a theory Θ_1, related to Θ via conditions C by R is or may be progress stands unanswered. The step from proper to approximative reduction was taken to make room for a condition necessary for progress, namely change in what the theory says. But it goes without saying that not any change of this kind will be progress. It may be quite the contrary. Thus the call for a justification is inescapable, and by calling it 'empirical' I restrict its scope to empirical progress.

How does such a situation come about? The virtue of deduction is that it is a truth-preserving procedure at least in the following sense: If the axioms of theory Θ_1 are true, and also the sentences expressing the conditions C, then of necessity Θ is true. And even for axpproximative deduction hopefully we would have the corresponding inference with respect to approximative truth. This, however, still waits a very careful investigation. At any rate, exact or approximative deductions are interesting by themselves as establishing truth or approximate truth in the case where truth or approximate truth of some other statements is already at hand. But, this is not the function, at least not the primary function, of deduction in empirical science. If it were then no empirical justification would be needed. It would itself be empirical because it could only function if its premises were empirically given. There is, however, another function of deduction, and it is this: If all questions of truth or approximate truth are assumed to be settled independently of the deduction in question, then the latter would show the <u>necessity</u> of the state of affairs as described by θ under the circumstances described by θ_1 and C. This seems to be the case of reduction and explanation for which independent confirmation of the statements involved is assumed and sometimes strongly emphasized, e.g. by Popper.

So the difficulties to be confronted in the usual confirmation theories are part of reduction theory anyway. But a new one comes when one becomes interested in the <u>relative</u> merits and demerits of θ_1 and θ as expressed by relation E <u>and</u> in the question what they have to do with the reduction relation R. More specifically, we are interested in getting R to be a <u>sufficient condition for E</u>. Evidently, R could never be a necessary condition for E: There is no way for such a substantial relation as R is to follow from the data dependent E. If, on the other hand, R can be made a sufficient condition for E it would also guarantee that θ_1 is at least as well confirmed as θ and probably better. But even this weak relationship is beset with difficulties because the relation E, besides relating θ and θ_1, depends on two further parameters: a set Ob of observations <u>and</u> a confirmation theory. The latter dependence may also be expressed by saying:

We still have to choose the relation E itself, and we shall have to do this while keeping an eye on the confirmation theory that we would like to use with respect to θ and θ_1 taken separately. Now it would be preposterous to expect that the wanted justification could be given in such a way that it will do its job whatever relation E is chosen. We do have some hope of matching the parameter Ob with the conditions C in R, but there is no hope of achieving anything at all without choosing a particular confirmation theory. Which one should it be?

In this vein it should also be noted that apart from the principal question of empirical justification of R by means of E there is the following question of an eminent practical importance: Assume that the theory θ had been a particularly well established theory and that hundreds and thousands of observations were used during the process of its empirical establishment. When the theory θ_1 now supersedes θ in a domain where the latter failed, we shall have to make sure that θ_1 does not fail where θ was so exceptionally successful. Certainly one could accomplish this by going back to the records and proving that the hundreds and thousands of observations that were in favour of θ also are confirming instances of θ_1 . Since this is rather impractical, the question arises whether one could not find a single overall procedure which would achieve at one blow what otherwise could only be done piecemeal. And certainly, a relation between R and E that would justify R by E in the sense indicated would at the same time have the pleasant consequence of relieving us of the above computational nuisance.

It is now time to attempt a solution of the problem outlined above. I begin with the relation E. Roughly it says that in light of the observations collected in the set Ob, the theory Θ_1 is at least as well supported as is Θ. More explicitly:

E $[\Theta, \Theta_1, Ob]$ if and only of every empirical success of Θ in Ob is also an empirical success of Θ_1.

Many questions come vis à vis the assumption that the observational statements at hand for the two theories are the same. But this is a distraction because the main problem of this section is saying what it means, given E as defined, that a couple of statements from Ob constitute an empirical success of Θ or Θ_1. So I shall define the notion in question als follows:

If M is a class of finite sets of theoretically possible observation statements for, say, Θ such that there are sets in M that contradict Θ, then if $e \in M$, $e \subseteq Ob$ and e does <u>not</u> contradict Θ e is an empirical success in Ob for Θ.

This concept of empirical success includes those hypothetico-deductive cases where one observation follows from the remaining ones in a set together with the theory. But it is more general; for example if a theory says that a certain particle moves on a circle under certain circumstances and if it is found in more than three places on that circle, then that counts as a success for the theory <u>without</u> one of the position statements following from the others and the theory.

Having so fixed E we come to the crucial question: What will make the reduction relation R a <u>sufficient</u> condition for E? As already indicated one possibility is to link the observations actually made and collected in Ob to the additional conditions C in the deductive reduction. It has been my <u>ceterum</u> <u>censeo</u> since the chapter on the physicist's view on progress that in theory succession one comes to know of a theoretical description of the domain of validity of the older theory in the light of the new one. In the reduction relation R this description appears as the additional conditions C. We could, therefore, try

$A[C, \Theta, Ob]$ if and only if every success of Θ in Ob is compatible with C.

This condition still allows empirical failures of Θ in Ob to be compatible with C, while the following assumption excludes this:

$A'[C, \Theta, Ob]$ if and only if every failure of Θ in Ob is incompatible with C.

A failure of θ is (assumed to be) a finite set of possible observation statements contradicting θ . These conditions include the approximative case which is particularly important in any linkage between C and Ob; for both of them depend explicitly on approximate circumstances.

Let us now look at the statement

$$(*) \qquad R[\theta, \theta_1, C] \text{ and } A[C, \theta, Ob] \Rightarrow E[\theta, \theta_1, Ob].$$

Will $(*)$ become a logical truth after the gaps have been filled in this sketchy presentation? If $(*)$ were logically true then we would have achieved the empirical justification of R. For it then would <u>follow</u> from R that θ_1 is empirically at least as good as is θ, provided the empirical success of θ actually obtained and recorded in Ob fulfills the limiting case condition C as expressed in A. Unfortunately, however, there is no reason to expect $(*)$ to be logically true. The reason is that the implication involved in E is converse to the implication involved in R. The latter is essentially of the form

$$(R) \qquad \theta_1 \wedge C \;\vdash\!\sim\; \theta .$$

Contrary to Kemeny and Oppenheim I have not made the observational consequences of θ its possible empirical success. An extreme (and unrealizable) prototype of a success in my sense would be given by a model of Θ. Even if C were true in such a model we would not be entitled by R to infer that that model is also a model of Θ_1; indeed it is just the other way round. So (*) cannot yet be the sort of justification needed.

Here is a more concrete illustration better suited to show what is wrong and the possible way out. Suppose Θ says that (under certain circumstances) a particle moves in a circle. Assume that this has been confirmed by an uncountable number observations. Now let us try to improve Θ by a much more advanced theory having a veritable system of differential equations

$$\dot{x} = \omega y, \quad \dot{y} = -\omega x.$$

as its equations of motion. From these equations it follow that the particle moves on a circle with constant angular velocity. Now when we test this theory it may even turn out that we can use for this purpose the old material because some clever person has recorded the positive measurements including the corresponding times. But now assume that these completed statements, telling us at which times which positions our particle has had, although, confirming the initial statement about the circle, contradict the new stronger theory Θ_1. Not only is this not excluded; one may, as a result, even become just as certain that the particle does not move with angular velocity ω as we were that it did move on a circle. As seen from theory Θ_1 it is clear that the very same oberservational material confirming Θ beyond doubt refutes Θ_1 beyond doubt.

In a sense this is an explicit counterexample to a simplified version of ($\ast$): Although Θ follows from Θ_1 it is not the case that every successful empirical outcome of Θ also is a successful empirical outcome of Θ_1. Indeed some and perhaps all are failures. Now if this is a counterexample there is the implication that the statement 'not all successes of Θ are successes of Θ_1' (always restricted to the collection Ob) is the negation of the statement E (Θ , Θ_1 , Ob) which is the conclusion in ($\ast$). However this tacit assumption may be denied. One might object that it is simply unfair to require of the time dependent material that is irrelevant in testing Θ so completely that, since the relevant parts are successes of Θ , the whole should also be a success of Θ_1 . If the old theory Θ is reproduced you cannot blame it for failures of the new adventures involved in Θ_1. But precisely this would be the outcome if our example were taken as a counter example to the general validity of ($\ast$). Rather, so the counterargument goes, the successes of Θ mentioned in condition E are required to be reproduced by Θ_1 only insofar as they concern Θ . And the kind of reproduction in question obviously is possible in the case before us.

Although this counterargument has to be taken seriously it remains, of course,true that ($\ast$) is not logically true. The question naturally arises: What in heaven's name is the reductive relation good for if not for the validity of ($\ast$)? The answer seems very simple, if a bit disappointing: it is to show that under the circumstances given by condition C the theory Θ_1 is not really better than Θ . To make this more precise let us define

> $E'[\Theta, \Theta_1, C, Ob]$ if and only if every success of Θ_1 in Ob compatible with C is also a success of Θ .

This obviously expresses in more detail that theory Θ_1 within C doesn't do better than Θ . One would then have the greater expectation that

$$(1) \qquad R[\Theta, \Theta_1, C] \Rightarrow E'[\Theta, \Theta_1, C, Ob]$$

is a logical truth than in the case of (*). Take the earlier examples of the two gas laws. R would then be the approximate implication

$$(R) \quad \left\{ \begin{array}{ll} (p + \frac{a}{v^2})(v - b) = RT & \Theta_1 \\ a \ll pv^2, \quad b \ll v & C \\ \hline pv = RT & \Theta \end{array} \right.$$

Assume now a success of Θ_1 to be given that fulfills C. In its most elementary form this would just be a triple p,v,T fulfilling the two first lines. The reduction R would then yield a triple p',v',T' fulfilling Θ . And this precisely is what was required to have E'. At the same time you can see, given this simple example, that again we would be helpless to infer E from

R and A which here would be

(A) Every success (p,v,T) of Θ in Ob fulfills

$$a \ll pv^2, \quad b \ll v.$$

If a success (p,v,T) of Θ were also given we could use (A) to get C. But although it is now true that (p,v,T) is also a success of Θ_1 this fact does not follow from (R). Rather it would follow from

$$\begin{array}{l} pv = RT \\ a \ll pv^2, \quad b \ll v \\ \hline \left(p + \frac{a}{v^2}\right)(v-b) = RT. \end{array}$$

So in addition to (1) the other general result to be anticipated would be

(2) $$R'[\Theta, \Theta_1, C] \text{ and } A[\Theta, C, Ob] \Rightarrow E[\Theta, \Theta_1, Ob]$$

instead of (*) where R' is a conversion of R with respect to Θ and Θ_1. The crucial question, to be sure, would then be: Does R' hold? In the case before us it does. But what about the general case?

Thus we arrive at the idea of a minimal covering theory. For a minimal covering theory the validity of R' is essentially its defining condition. At the same time Θ and Θ_1 are then asymptotically equivalent under the limiting case conditions, and (*) and (2) become equivalent. So here we have (*) - <u>but</u> for special reasons. In the general case the problem is whether a minimal covering theory <u>always</u> exists. This is an interesting though difficult and unsolved problem. In the case before us of a minimal covering theory <u>the condition E ought to be formulated with respect to it</u>. We could then have (*) in a form where θ_1 is the minimal covering theory.

It should now be clear what happened to our counter example, even though it was not an approximative case. On the one hand, since it was purposed as a counter example θ was a much weaker statement than θ_1. Consequently, we had no difficulty in finding observational material confirming θ but contradicting θ_1. On the other hand, since the counterargument for this procedure is also justified, θ_1 has to reproduce the success of θ only with respect to θ's successor in θ_1. The method of the minimal covering theory (which here trivially would be θ_1) combines both aspects: The weakness of θ and the demand for a reproduction of its successes.

V. 3. Partial Reduction

Concepts of reduction explicitly referring to observational data are special cases of the much wider concept of partial reduction.[10] In fact the latter not only covers the former as a kind of extreme case, it also covers the deductive reductions of the previous chapter as another extreme subcase. In a sense, therefore, it at least blurs the difference between the two so much stressed in the previous section. One might, therefore, think that the concept of partial reduction would allow one to reduce too much and so cannot be very interesting. True enough, but it must be examined briefly for the following reasons: First, it has still other interesting subcases and might, therefore, nicely highlight some features common to all of them. Second, some of these other subcases are important because they clearly are genuine partial reductions, i.e. reductions that cannot be embedded in reductions we would acknowledge as being complete. This has already been hinted at in the form of the data dependent reductions of the previous section: not all of these reductions can be embedded in complete deductive reductions in the sense of Chapter IV. But there are more important cases where the statements actually deducible are nearer to the theory properly to be reduced than in the case of common observational data.

The principal idea in a partial reduction is that from the reducing theory Θ_1 the theory Θ to be reduced can not (even not approximately) be deduced but only some weaker statement belonging to an admissible extension of Θ. Within the realm of proper deduction and apart from data dependent reduction there is a special case at the end of Chapter IV.1. There domain restricting reductions were cases in point because it was not the theory to be reduced but a certain restriction of it that could properly be deduced. If we allow for approximate reduction there are still more interesting cases. In the physical literature both kinds of partial reduction - by proper or approximative deduction - are mentioned, though often phrased in different terminology.

Let me again quote from Rohrlich's book 'Charged Classical Particles'. Talking about the concept of levels of theories Rohrlich says quite informally:[11]

> The study of the relationship between two successive levels of theory, a theory and its minimal covering theory, is of great importance ... it is essential that the lower-level theory be derivable from the covering theory. This must be true not so much with respect to the axiomatic framework, which is in general not a special case of the framework of the covering theory, but with respect to certain basic equations and postulates which contain all the predictive power of the lower-level theory.

This apparently is an instrumentalistic statement saying that in theory reduction it is sufficient to reproduce anything that is representative of the predictive power of a theory. The instrumentalism inherent in Rohrlich's words is not necessarily a tacit admission that eventually one must be satisfied with partial reductions incapable of reproducing the ontology of the earlier theory as expressed in its 'axiomatic framework'. Rather he seems to think that the axiomatic framework adds nothing to the equations representing the full predictive power of a theory. But despite the instrumentalistic tinge of Rohrlich's remarks, certainly he not only talks about partial reductions but accepts them. This does not actually commit one to instrumentalism provided one acknowledges from time to time that even our ontology has to be revised.

Instrumentalism in the sense that not all physical theories necessarily have ontological commitments is clearly displayed by Truesdell and Noll in their encyclopedia article on "Non-linear field theories of mechanics".[12] Distinguishing between structural and continuum theories of matter they claim the latter to be free from any statements about the structure of matter:

> Widespread is the misconception that those who formulate continuum theories believe matter "really is" continuous, denying the existence of molecules. This is not so. Continuum physics presumes nothing regarding the structure of matter. It confines itself to relations among gross phenomena, neglecting the structure of the material on a smaller scale.

On the other hand, the authors contrast continuum mechanics in this regard with quantum mechanics as a structural theory when they say:

> The reader of this treatise is not asked to question the "real" existence of atoms or subatomic particles ... However, we cannot give him assurance that quantum mechanics ... yields the same results. Any claim of this kind must await such a time as physicists turn back to gross phenomena and demonstrate that their theories do in fact predict them ...

Assuming that quantum mechanics can reproduce the experimentally confirmed results of continuum mechanics a partial reduction would exist and vis à vis the basic assumptions of continuum mechanics <u>could be no more</u> than that.

Let me now briefly say something about the concept of partial reduction in general and then give a few elucidating examples. Since 'a picture is worth a thousand words', I will explain that concept by means of what might be called the reduction square:

$$\begin{array}{ccc} \Theta_1 & \sim\sim\sim\sim & \Theta \\ \top & (2) & \top \\ | & & | \\ \alpha_1 & \sim\sim\sim\sim & \alpha \\ & (1) & \end{array}$$

(additional assumptions possible in all four cases)

(α_1 individual covering law of α vis à vis Θ_1)

Here Θ_1 and Θ are the two theories, Θ_1 the reducing one and Θ the reduced one. α_1 and α are propositions occurring in (exact!) admissible extensions of Θ_1 and Θ respectively. It is assumed that α can be properly or approximately deduced from α_1 and, as usual, possible additional assumptions. This is the central assumption of partial reduction. The crucial question is whether the same relation holds between Θ_1 and Θ. If not then we have a genuine case of partial reduction that cannot be completed 'by closing the square'. But at any rate α is reduced to Θ_1 <u>via</u> α_1. α_1 may be regarded as the <u>minimal</u> covering theory of α.

Domain restricting and data dependent reductions can easily be reformulated to fit the reduction square. The following are concrete examples of partial reduction. The first example is as follows:

$$\begin{tikzcd} \text{Newton's theory (for 2 bodies)} \arrow[r, squiggly] \arrow[d, dash] & \text{Kepler's laws (for one planet)} \arrow[d, dash] \\ \text{improved third law} \arrow[r, squiggly] & \text{Kepler's third law} \end{tikzcd}$$

Though the theories of the first row will not be amplyfied it must be mentioned that Kepler's laws for <u>one</u> planet can be approximately deduced from Newton's gravitational theory for the sun and that planet.[13] So we have here a case of <u>closed</u>

reduction square, and in that respect the example does not illustrate a genuine partial reduction. But it illustrates the possibilities of completion: One may imagine that we knew first only the approximation in the second row and the vertical connections $\top$. Then it would have been an important question whether the stronger and more complicated approximation of the first row holds, a question to be answered in the affirmative. Furthermore, the square nicely illustrates the difference between a minimal and a non-minimal covering theory. Thus let me write down the derivation in the second row. It is

$$\frac{a^3}{T^2} = (2\pi)^{-2}\gamma M\left(1+\frac{m}{M}\right)^{-2}$$

$$m \ll M$$

$$a^3/T^2 = (2\pi)^{-2}\gamma M$$

where the usual notation is used. Evidently, the improved 3^{rd} law differs from Kepler's 3^{rd} law only by a correction term. By contrast, Newton's gravitational equations for a 2-body system are

$$\ddot{r}_1 = -\gamma m\,(r_1 - r_2)\,|r_1 - r_2|^{-3}$$

$$\ddot{r}_2 = -\gamma M\,(r_2 - r_1)\,|r_2 - r_1|^{-3},$$

This is also a covering theory of Kepler's 3^{rd} law. But it is obviously not minimal.

The second example illustrates a reduction square that probably is not closed:

$$\begin{tikzcd}[column sep=huge, row sep=large]
\text{QM} \arrow[r, squiggly, no head] \arrow[d, dash] \arrow[dr, phantom, "?"] & \text{CSM} \arrow[d, dash] \\
\begin{array}{l}\text{improved distr.}\\ \text{of harmon. osc.}\end{array} \arrow[r, squiggly, no head] & \begin{array}{l}\text{canon. mom. distrib.}\\ \text{of harmon. osc.}\end{array}
\end{tikzcd}$$

Here quantum mechanics and classical statistical mechanics appear in the first row. That is why I had to say that the square probably is not closed: It is not completely clear whether the relations known to exist between these two theories boil down to an approximative reduction in our sense. By contrast, two very special but easily compared laws occur on the lower row: the momentum distributions of a canonical ensemble of harmonic oscillators as they follow from quantum mechanics and classical statistical mechanics respectively. The approximate deduction in this case is:

$$\rho(p) = \left\{ \frac{1}{\pi\hbar\omega} \cdot \tanh\left(\frac{\hbar\omega}{2kT}\right) \right\}^{1/2} \exp\left\{ -\frac{p^2}{\hbar\omega} \tanh\left(\frac{\hbar\omega}{2kT}\right) \right\}$$

$$\hbar\omega \ll kT$$

$$\rightsquigarrow$$

$$\rho(p) = (2\pi kT)^{-1/2} \exp\left\{ -\frac{p^2}{2kT} \right\}$$

where ω is the frequency of the oscillator. This approximate deduction of course is trivial. But whereas in the Kepler/Newton case one could only say that the first row deduction is somewhat less trivial, in the current example it is highly non-trivial and probably even non-existent. At all events, the partial reduction under discussion would, of course, not be the only partial reduction in which quantum mechanics and classical statistical mechanics are connected. On the other hand, it would be very difficult to point out the totality of laws each of which establishes a partial reduction. There certainly are many such laws, indeed even exact ones. But their totality can hardly be grasped. Even more difficult would be a partial reduction of the assumptions representative of the predictive power of classical statistical mechanics as Rohrlich construes it.

Notes to Ch. V

1 I have done this already in Scheibe [1983]

2 This was done (in a clearly insufficient way) in Carnap [1962], Ch. VII, esp. § 86

3 A good survey is given in Kuypers [1987]

4 Cf. Ch. III

5 Kemeny and Oppenheim [1956]

6 Popper [1975], pp. 82 f.

7 Lakatos [1968], p. 383

8 Lakatos [1970], p. 116

9 At the same time it is an improvement of Scheibe [1983]

10 This concept was defined in Scheibe [1984 b]

11 Rohrlich [1965], p. 5

12 Truesdell and Noll [1965], pp. 5 ff.

13 For the details see Scheibe [1973 a] and [1973 b]

VI. The Replacement View

The following chapter on the (sometimes) socalled "replacement view" on scientific progress, like Ch. VIII, is very speculative. Although it is not meant to be an introduction to the view, associated mainly with Feyerabend and Kuhn, I will give it the conventional title. Indeed in an orderly introduction I would have to touch upon the complete context of the treatment of progress in Feyerabend and in Kuhn.[1] But since they have quite different views in some respects this would far exceed my current goal. Moreover, thoroughness would require a discussion of the whole controversy following the major publication of the two authors,[2] and that would not be profitable here given my limited goal in this chapter. So I will only take the liberty of emphasizing some features of the replacement view that I find interesting, though I admit that I do not yet understand the matter sufficiently and that I see unsolved and non-trivial problems that irritate me in particular.

What I want to do in this chapter is to bring together some concepts that have been developed independently and up to this day have remained separated although their integration is urgent for progress in the field. I cheerfully admit that I am a bit doubtful about the importance of the points I will stress because it does not seem to fit the impressions of others and in particular of Feyerabend and Kuhn. My impression is that the concept of incommensurability of theories as it has been developed by Feyerabend as the central idea of his radical theoretical pluralism is closely related to the concept of complementarity in the sense of Bohr. Although not quite correct, as a first approximation one may say that the common source of the concepts in question is quantum mechanics, and we have already seen that it is quantum mechanics that led Heisenberg to his concept of closed theories and to a replacement view or - more specifically - a conceptual change view of scientific progress. There is an important difference between closed theories in Heisenberg's sense and universal theories in the sense of Feyerabend. On the whole, however, the similarities between the two ideas are dominating, and it is here that one finds the connecting link between incommensurability and complementarity.

In the introduction I presented Feyerabend as - so to speak - the arch-enemy of the idea of the unity of science. His radical theoretical pluralism is the contrary view to any unitary conception of knowledge and science in the usual sense. Of course Feyerabend's pluralism has its impact also on the ideas of progress and reduction. Recall that we started two chapters back with strictly deductive reductions but soon had to generalize them because progress with respect to the reduced theory could not generally be achieved in this way. Approximative deductions were much better because drastic changes <u>and</u> improvements could be successfully treated with the associated concept of reduction. Then, when the question of the empirical import of deductive reduction arose we were naturally led to a concept of partial reduction, and it was stressed that retreat even to this weak a concept of reduction might be necessary in some cases. Here is where Feyerabend entered the picture. Although he often talks about the importance of contradictions in theory succession, he disliked approximate reductions and pointed out their insufficiancy in some cases. He didn't accept partial reduction either but he repeatedly emphasized that though a theory can explain all the empirical successes of its predecessor, this means <u>nothing</u> vis à vis the question whether it can also reproduce its "ontology". Rather he pointed out cases where <u>this</u> seems to be impossible, and instead of looking to see what is still possible in the sense of partial reduction he developed the idea of theories with different ontologies or, as they came to be called, <u>incommensurable</u> theories. Incommensurability was also the point of contact with Kuhn. As Feyerabend and Kuhn see it, the turning point is in the matters of progress and reduction: There seemed to be cases, marked by incommensurability, that would defy all attempts at subsumption under any of the concepts of progress or reduction so far developed. Indeed there simply is no longer any question of reduction. Rather one can say at the most that in these cases a superseded theory is just <u>replaced</u> by its successor.

VI. 1 Incommensurability

I have already indicated in the introduction that Feyerabend's position in many respects is a much more radical position than Kuhn's. For one thing, Feyerabend's theoretical pluralism is a philosophical credo. By contrast, Kuhn has a theory on the development of science. _A priori_ the two things may, therefore, not be related _at all_. They _are_, in fact, related mainly through the concept of incommensurability. But again, even here there is a difference. For Kuhn incommensurability is a means to describe what happens in a scientific revolution. For Feyerabend incommensurability is the central relation tieing together the different components of a truely pluralistic world view. This difference, of course, is nothing but the consequence, as regards incommensurability, of the first mentioned difference. Thirdly, it is true that for both authors incommensurability is _partly_ a _sociological_ concept. It was meant to express partly the difficulties preoccupying the communication between the adherents of a traditional view point and the advocates of new revolutionary ideas. In part it was even _psychological_ in that it tried to grasp the conviction that the revolutionaries have with respect to new ideas _in view of_ the well confirmed theories of the past. There is also the "_objective_" component present in both Feyerabend and Kuhn. I mean that part of incommensurability between an old and a new theory that would remain alive even if the new theory itself has become an old one. The difference now to be mentioned is that this component is much more important for Feyerabend than it is for Kuhn. In the following I shall confine my presentation to this "objective" component. Finally, even under this restriction Feyerabend's "objective" concept of incommensurability is much more radical than is Kuhn's. I am now going right into the matter by taking this last difference as my starting point.

In a paper[3] that came out 20 years after Kuhn's book "The structure of scientific revolutions" Kuhn informs us that his paradigm for incommensurability had always been the geometrical concept according to which, for instance, the circumference of a circle is incommensurable with its radius. Kuhn is anxious to add that lack of a common measure does not make comparison impossible. In the metaphorical use he was to make of the concept the phrase "no common measure" became "no common language". I now quote:

> The claim that two theories are incommensurable is then the claim that there is no language, neutral or otherwise, into which both theories, conceived as sets of sentences, can be translated without residue or loss.

But again he immediately adds

> No more in its metaphorical than its literal form does incommensurability imply incomparability, and for much the same reason.

And this is made more explicit in the following passage:

> Most of the terms common to the two theories function the same way in both; their meanings, whatever those may be, are preserved; their translation is simply homophonic. Only for a small subgroup of (usually interdefined) terms and for sentences containing them do problems of translatability arise. The claim that two theories are incommensurable is more modest than many of its critics have supposed.

In the next section on 'conceptual assimilation' I will try to make explicit what this view of local incommensurability, as Kuhn calls it, amounts to. For the moment it suffices to say that Kuhn obviously makes incommensurability a matter of degree when he tries to localize or to isolate it in the language used. For it may then be a larger or smaller part of the language that can not be intertranslated.

It is quite different with Feyerabend. To be sure, he, too, does not seem to understand incommensurability as incomparability. In a review of Laudan's "Progress and its Problems" he blames the author to have imposed the view on him and Kuhn[4],

> ... that we wanted to compare theories, were misguided by some feature of science into believing that a comparative evaluation was impossible and joylessly published this disagreeable consequence.

And he continues:

> A look at our work reveals an entirely different story. What we 'discovered' and tried to show was that scientific discourse which contains detailed and highly sophisticated discussions concerning the comparative advantages of paradigms obeys laws and standards that have only little to do with the naive models which philosophers of science have designed for that purpose. There is

> comparison, even 'objective' comparison, but it is a much more complex and delicate procedure than is assumed by rationalists.

Now, to be told this is, of course, not yet to be told how theory comparison is possible if only total incommensurability obtains. But at least we are informed about the <u>intention</u> to keep incomparability apart from incommensurability. However, the latter <u>is</u> meant by Feyerabend in the extreme sense "that the use of <u>any</u> concept of one of the theories makes inapplicable the concepts of the other."[5] And Feyerabend is aware of <u>some</u> difference between his and Kuhn's position in this respect: "The mere difference of [concepts] does not suffice to make the theories incommensurable. This point is occasionally overlooked by Kuhn." Feyerabend is here critisizing Kuhn not just because of the latter's weaker concept of local incommensurability, but apparently is trying to emphasize that his conception is the more radical one. It is, of course, not important that a man called 'Feyerabend' has a more radical view than a man called 'Kuhn'. What is important is the systematic aspect, vis à vis incommensurability, in which a conception of wider scope is given than that of total incommensurability. It is very unlikely that all historical cases either are examples of total incommensurability or of total commensurability. Rather it is likely that we will find a spectrum between and perhaps inclusive of these extreme cases.

Let us look a bit closer at the definitions Feyerabend gives of his total incommensurability. One typical passage reads:[6]

> ... there are theories about which we would say intuitively that they "talk about the same things" and yet have not one sentence in common. This happens not simply because the theories describe different domains ... but because the use of the conceptual apparatus of one of them imposes conditions that paralyse the use of the conceptual apparatus of the other theory ...

In another place the relation between classical and relativistic mechanics is described as follows:[7]

> The new laws will not only <u>read</u> differently, they will also conflict <u>in content</u> with the preceding classical laws. And this is not just a matter of words. ... <u>Not a single primitive descriptive term of T can be incorporated into</u> T'. ... we may express this by saying that the change of rules accompanying the transition T→T' is a fundamental change, and that the meanings of all descriptive terms of the two theories, primitive as well as defined terms, will be different: T and T' are <u>incommensurable theories</u>.

Finally, a very concise formulation, even having the form of a definition, is:[8]

> Two theories will be called incommensurable when the meanings of their main descriptive terms depend on mutually inconsistent principles.

It transpires from these general descriptions of incommensurability that this relation is intended to describe a much more radical disparity between theories than logical incompatibility. Logical incompatibility implies a common element of the theories that is denied by incommensurability. It implies that the languages of the theories have some aspect in common. It is that aspect in which you find statements α such that α follows from one of the theories and $\neg\alpha$ from the other. Such overlapping, which implies the existence of common meanings, is what is denied by incommensurability. Logically incompatible theories must have a common interpretation but that interpretation cannot make all of the statements of both theories true. Incommensurable theories cannot have even a common interpretation. Feyerabend tells us there are to be principles of language formation such that, given two incommensurable theories T_1 and T_2, the principles of T_1 would not only tell us how to establish the language of T_1, they would also prevent the language T_2 being combined with that of T_1 and *vice versa*. How can such a thing happen?

As I understand him Feyerabend denies that this question is primarily a question of *meaning*. All talk about meaning change, therefore, is more or less beside the point. For Feyerabend the principal problem of incommensurability is an *ontological* one. Two incommensurable theories have different ontologies. So if you refuse to accept ontologies at all you can have no problems, or at least no serious problems, with incommensurability. Feyerabend makes it quite clear that for any given theory there is a latitude of its interpretation as to how much of its formalism is assumed to have referents. For some theories this latitude is considerable, and you can make use of it either to avoid or to advance incommensurability. If you are an instrumentalist you will have it easier than a realist. In the previous chapter the case of classical continuum mechanics versus quantum mechanics has already been touched upon. We have seen the physicist's statement that whoever subscribes to classical continuum mechanics does not thereby deny the atomistic structure of matter. This is an instrumentalistic position at least with respect to this particular theory. The basic categories on which CCM is built are not viewed as involving any ontological commitment. Rather the ontology for both theories is restricted to their common part. Once this position is assumed, one has <u>made</u> CCM <u>commensurable</u> with QM. In this sense of 'emasculating' theories - as Feyerabend likes to express himself - there is even for him a <u>grading</u> in matters of incommensurability. But the position that he finds the more interesting one is the realistic position. We shall now examine whether this necessarily forces one to embrace total incommensurability. I shall show that this is not so.

VI. 2. Conceptual Assimilation

Let me first provide an example of incommensurability. The example ist not Feyerabend's but I think that he would accept it at least for the purpose of demonstration. It arose in quantum mechanics and is commmonly called the dualism of wave and particle. The wellknown background is that free electrons behave like particles or like waves depending on how they were treated. This seemed paradoxical because from the classical view point an electron could either be a particle or a wave but by no means <u>both</u>. Why could it not be both? Because the 'pictures' of a particle and a wave respectively exclude or even "contradict" each other. It seemed to be impossible to combine the two pictures in one new picture containing the original ones. On the other hand it seemed necessary to retain both pictures because electrons just showed particle-like and wave-like features. The way out of these difficulties was quantum mechanics from which, according to Bohr, the two pictures could be recovered as being complementary descriptions of the same reality.

This probably <u>was</u> the best formulation. For one thing, from a strictly logical view-point there is <u>no</u> justification whatsoever in talking about the particle and the wave pictures as <u>contradicting</u> each other.[10] Rather we have instead a good example of incommensurability, if not total incommensurability. To be more definite, assume that on the basis of the same theory of spacetime, e.g. Newton's, we have a theory of one particle moving through space according to some equation of motion, and - on the other hand - we have a theory of a scalar field in spacetime obeying some wave equation. These two theories would not be totally incommensurable because they have the spacetime theory in common. But neither can they be commensurable, _if_ they are meant to be theories about the same subject matter. The principle of the particle language that makes the wave interpretation impossible and, _vice versa_, the principle of the wave language that makes a particle interpretation impossible concerns the use of different _types_ of descriptive predicates. In a many-sorted presentation of the language and with Newton's spacetime theory as the foundation the particle predicate would be a first-order and _two-termed_ predicate, linking time and space. The wave predicate would be a first-order but _three-termed_ predicate, linking time, space _and_ a field strength. These type rules - these prescriptions of which types our descriptive predicates should be - are syntactical rules that are not expressed in the object languages, and it is for this reason that we have here, not a logical but a metalogical, incompatibility before us. Consequently, it is not the common truth of object statements that is prevented but rather the common interpretation of the languages. Moreover, it is the difference in type that corresponds to the impossibility to visualize an object in spacetime that would be a particle _and_ a wave at the same time.

Pointing out the type difference of descriptive predicates occurring in one formulation of each theory is, however, not sufficient to secure incommensurability. Nothing of what was said so far in commenting on the case before us prevents the two theories from having equivalent reformulations that do have type-similar conceptual bases. Allowing for a sufficiently wide concept of theory equivalence there are equivalent reformulations of a theory using predicates having types quite different from the types of the original predicates. A wellknown example are the different but equivalent versions of euclidean geometry. Euclidean geometry can be formulated on the basis of a distance function taken modulo a real factor or of a congruence and betweenness relation or a set of euclidean coordinate systems or what have you. With M as space and $\mathbb{R}$ the set of real numbers the types of the descriptive predicates of geometry would be in set theoretical notation

$$r \subseteq Pow(M^2 \times \mathbb{R})$$

$$s_1 \subseteq M^4, \quad s_2 \subseteq M^3$$

$$t \subseteq Pow(M \times \mathbb{R}^3)$$

respectively. Evidently they are quite different. Nonetheless the theories are equivalent on the basis of an interdefinability of the predicates r, s_1 and s_2, as well as t.

So there is the possibility of equivalence transformations changing the similarity type of a theory. Consequently, two theories of different similarity types could have equivalents with the same similarity type. But to return to the wave-particle example you can be pretty sure that there are no (equivalent) reformulations having type-similar conceptual bases that would allow one and the same interpretation of the theories. I cannot prove this conjecture, and it is hard to see how a proof would go. And yet the conjecture seems to be meaningful because it is quite clear what its refutation would mean: One would only have to perform the sort of reformulation described above.

So here we seem to have a method for coming to grips with incommensurability, and it is easy to see that there are intermediate stages in the sense of Kuhn.[11] Starting from two theories S' and T' formulated in terms α and β respectively we are asked to look for equivalent theories S and T respectively that are formulated in common terms γ. We would then have

$$(1) \quad \begin{cases} S'(\alpha) \wedge \gamma = P(\alpha) \vdash S(\gamma) \\ S(\gamma) \wedge \alpha = P^{-1}(\gamma) \vdash S'(\alpha) \end{cases}$$

together with

$$(1') \quad \begin{cases} S'(\alpha) \vdash P^{-1}(P(\alpha)) = \alpha \\ S(\gamma) \vdash P(P^{-1}(\gamma)) = \gamma \end{cases}$$

and similarly

$$(2) \quad \begin{cases} T'(\beta) \wedge \gamma = Q(\beta) \vdash T(\gamma) \\ T(\gamma) \wedge \beta = Q^{-1}(\gamma) \vdash T'(\beta) \end{cases}$$

with

$$(2') \quad \begin{cases} T'(\beta) \vdash Q^{-1}(Q(\beta)) = \beta \\ T(\gamma) \vdash Q(Q^{-1}(\gamma)) = \gamma \end{cases}$$

Further more, the existence of the equivalence terms P and Q as well as their counterparts P^{-1} and Q^{-1} may depend on the validity of the theories S', T', S and T respectively. This list of assumptions may be abbreviated as follows:

$$(3') \quad \begin{cases} S'(\alpha) \simeq S(\gamma) \\ T'(\beta) \simeq T(\gamma) \end{cases}$$

If the terms P, Q etc. do not exist we would have a case of *rather strong incommensurability*. I say 'rather strong' because conditions (3') may be weakened and thus lead to even stronger incommensurabilities. But for the time being the negation of a *conceptual uniformization* (3) may suffice.

Even if (3') is possible various cases may be distinguished that are interesting. As already mentioned the equivalence of S' and T' is the extreme case of commensurability. We would then have

(4) $$P(\alpha) \equiv \alpha, \quad P^{-1}(\alpha) \equiv \alpha, \quad T \equiv S$$

and consequently

(4') $$T'(\beta) \simeq S'(\alpha).$$

Within (3) the other extreme would be that S and T *contradict* each other:

(5) $$S(\gamma) \perp T(\gamma).$$

For questions of incommensurability this is uninteresting if

(5') $$\alpha \equiv \beta, \quad S'(\alpha) \perp T'(\alpha)$$

Interesting cases come up whenever at least one of the equivalence transformations is non-trivial <u>and</u> theory dependent.

A case in point are the theories of Galilean and Minkowskian spacetime. The former may be formulated by making the concept of absolute time a primitive concept. This even seems to be a very appropriate formulation. If both theories are formulated as theories about a set of spacetime points (or events) then absolute time in the Galilean theory could be introduced as a set of subsets of spacetime, the subsets being totalities of simultaneous events. This 'sliced' structure of Galilean spacetime is absent from Minkowskian spacetime. <u>No</u> reformulation with a concept of absolute time as a primitive concept is possible in the Minkowskian case. The corresponding structure simply does not exist. Therefore, <u>if</u> a common conceptual basis can be found at all it is clear that the equivalence transformations for the Galilean case would fulfill the two conditions mentioned: it would have to be non-trivial and theory dependent. The reintroduction of absolute time as seen from the common basis is possible on specific Galilean assumptions that are not given in the Minkowskian theory. In fact <u>there are uniformizations</u> of this case and they are even such that the two theories are the same except for the value of a numerical variable:

(6)
$$\begin{cases} S(\gamma) \equiv R(\gamma) \wedge \gamma_0 = 0 \\ T(\gamma) \equiv R(\gamma) \wedge \gamma_0 = c^{-2} \end{cases}$$

where γ_0 is one of the γ and c is the velocity of light.[12]

Incommensurabilities of the type under discussion illustrate very well Kuhn's local incommensurability and yet partly fulfill Feyerabend's conditions. They illustrate Kuhn because the conceptual apparatus of the theories have identical as well as diverging parts. And Feyerabend's conditions are partly fulfilled in the sense that the axioms of the theories themselves become presuppositions of concept formation. Taken by itself this is nothing new. But it appears in a new context: It can make theories locally incommensurable. In the spacetime illustration the axioms of special relativity simply do not allow the definition of an absolute time whereas the Galilean axioms do allow it. In many cases Feyerabend and Kuhn do not even point out these incommensurabilities. Rather they say things such as the relativistic concept of mass is different from the Newtonian concept of mass, the relativistic concept of energy is different from the Newtonian and so on. Their <u>argument</u> for these differences then is always that the concepts in question have features in one of the theories that they don't have in the other one. This is a very weak argument because it boils down to simply pointing out <u>contradictions</u> with respect to statements constructed from identical terms. These cases can always be interpreted to say that the two theories try to comprise the <u>same referents</u> but make contradicting statements about them. If <u>this</u> were all that incommensurability is there would be no serious problems with it. We would be back with Popper's contradiction requirement and approximate reduction. But in view what Feyerabend, at least, is about it is a mistake to confront concepts that are possible elements of a common conceptual basis of the two theories.

In fact, the foregoing reveals a disparity of structures, belonging to the equivalence classes created by each of two theories, as a measure of the latter's incommensurability. Though I shall devote greater attention to the structural view point in the last lecture, the following account,which sums up the foregoing considerations, will be a useful introduction to the matter. A structure consists of a pair of principal base sets together with a pair of sets constructed from the base sets by iterating an operation defined on the power set of a cartesian product. It is convenient to refer to these as the structures proper, - the structures that are - so to speak - imprinted on the principal base sets. They are distinguished from all the other possible structures proper that could have been so distinguished but are not. If we now have not only a structure but also a theory about it then theories equivalent to the first one must not but may lead to other proper structures on the same base sets. Incommensurability may even be strengthened by changing the base sets, too. But I will not consider this possibility here. An equivalence class of theories as conceived here will then lead to a group of structures proper, generally widely varying in type but being structures all imprinted on one and the same system of base sets.

Take now two theories, each about one structure, the two structures having the same base sets. This is indeed a rather special assumption. Formalized it would amount to something like

$$\vdash \bigvee X, s, t.\ \Sigma(X, s) \wedge \Theta(X, t).$$

where Σ and Θ are the two theories. This assumption is satisfied by the Galilean and Minkowskian theories of spacetime (as Σ and Θ respectively). But it doesn't seem to hold for classical statistical mechanics and quantum mechanics. However, for the following 'illustration' we shall adopt the assumption which, obviously, is a commensurability assumption. What will then be the general situation with respect to the structures propers s and t? There are two extreme cases. (1) If the two theories are equivalent, then their associated bunches of structures proper are identical. (2) The other extreme would be that they have no element in common. In general there would be overlapping and at the same time structures belonging to the group created by one of the theories but not to the group created by the other and vice versa. The extension of these cases, then, would be a 'measure' of incommensurability. The mistake by Kuhn and Feyerabend can now be rephrased by saying that they tried to demonstrate incommensurabilitiy by referring to the overlapping cases instead of the non-overlapping ones. There is, of course, the refinement that brings in the theories. Given that there is an overlapping of the structures the theories may contradict each other or - alternatively - may be compatible. (They will be such with respect to any structure if they are with respect to any other.) And the case of contradiction then certainly will be a serious one. But vis à vis incommensurability not only the consequences in the overlapping part but more importantly in the non-overlapping one should be discussed. Although I cannot prove it it seems plausible that two theories which are contradictary in the overlapping part will have a non-overlapping part. Since the converse of this certainly is not true, inconsistency indeed defines a particularly strong case of non-overlapping.

In conclusion I should at least mention that the method of conceptual assimilation by equivalence transformations reintroduces the problem connected with inhomogeneous reduction and interpretational overdetermination. In this regard the most important distinction to be observed is whether to look at the matter from a purely logical point of view or from an ontological point of view. If one does only the former then the equivalence of theories can be taken at face value: From a logical point of view there is simply no essential difference between two equivalent theories even if they are about different structures. The inhomogeneities occurring here are inessential because they can be transformed away. Essential inhomogeneities from the logical point of view only arise in the non-overlapping cases. In such a case a structure belonging to one theory cannot be introduced by any means into the other theory. Such is the case, for instance, with absolute time in special relativity but also - conversely - with the Minkowski metric in Galilean spacetime. By contrast, from an ontological point of view genuine inhomogeneities may already occur <u>within</u> equivalence classes. If we look at one structure as having an <u>independent</u> existence, a second structure introduced by performing an equivalence transformation on the first has <u>so far</u> only a <u>derivative</u> existence. It <u>may</u>, of course, also have an independent existence, and these were the cases of interpretational overdeterminations introduced in Ch. IV.3. But independent existence cannot be established by logical means. Therefore the method of conceptual assimilation has to be supplemented by clearing up the ontological status of a logically possible common basis. If there should be no independent existence, it will be of a doubtful value in theory comparison, and incommensurability would receive a new dimension.

VI. 3. Incommensurability, Closed Theories, and Complementarity

In Ch. III it was pointed out that the notion of incommensurability was invented by the physicists. Its most explicit formulations are found in the relevant writings of Bohr and Heisenberg. Occasional remarks made by Feyerabend show that he is aware of this. Thus in one place[13] he tries to summarize (not very successfully) the essential features of classical and quantum mechanics that make these theories incommensurable. He then says of this form of incommensurability: "This feature of the pair has been discussed ever since Bohr introduced the principle of correspondence." In another place [14] he (indirectly) quotes Bohr as having said that CM and QM are "carricatures ... which allow us, so to speak, to asymptotically represent actual events in two extreme regions of phenomena." Nevertheless Feyerabend never gives a systematic account of the connection between his view and that one of Bohr and Heisenberg. This is the more surprising because he has written systematic papers on the work of Bohr, if not also of Heisenberg. In the following I will briefly sketch the connection in question and draw some consequences for Feyerabend's pluralism.[15]

My starting point is a comparison of Heisenberg's relevant work with Feyerabend's. First of all it should be noted that for Heisenberg the center of his position is a certain concept of theories, not a relation between theories. For Feyerabend it is just the reverse. In the first instance, therefore, we have the concept of a closed theory confronted with the concept of incommensurability _between_ theories. It should be noted however that both authors also talk about the other respective conception. Feyerabend asks himself what kind of theories will be incommensurable as soon as they are different. His answer is:[16] "Incommensurability will be met with most likely in theories that are _universal_ in the sense that they contain means for the description of every process possible within their scopes and that they allow us to define the measuring operations that are used to test them." So Heisenberg's closed theories seem to correspond to Feyerabend's universal theories. On the other hand, we have seen Heisenberg emphasize that the transition from one closed theory to another one in the history of physics always means a _radical conceptual rebuilding_. And this obviously corresponds to Feyerabend's notion of incommensurability. So we have a sufficiently broad basis of comparison: Both authors have a concept of theory as well as a concept of theory relation such that different theories subsumed under that concept typically stand in that relation.

The closeness of the relationship between universal theories in the sense of Feyerabend and closed theories in the sense of Heisenberg has to be assessed by a comparison of their respective definitions. Unfortunately, however, these definitions are as vague as they are interesting. One is therefore dependent on additional comments and illustrations, and from them the similarity of the two conceptions is even more evident. Both authors distinguish between slight modifications and fundamental changes of a theory, the former being of the type of correction terms in the basic dynamical equations, the latter being changes of the primitive concepts of the theory. In Feyerabend's words:[17]

> ... a change in the spatiotemporal ideas of Newton's celestial mechanics makes it necessary to redefine almost every term, and to reformulate every law of the theory, whereas a change of the law of gravitation leaves the concepts, and all the remaining laws, unaltered.

Here there is even agreement with Kuhn. In Kuhn's terms the problem of distinguishing between small and big theory changes is the "problem of distinguishing between normal and revolutionary change." And he says:[18]

> I have ... used the term 'constitutive' in discussing that problem ..., suggesting that what must be discarded during a revolutionary change is somehow a constitutive, rather than simply a contingent, part of the previous theory. The difficulty, then, is to find ways of unpacking the term 'constitutive'. My closest approach to a solution ... is the suggestion that constitutive elements are in some sense quasi-analytic, i.e. partially determined by the language in which nature is discussed rather than by nature <u>tous court</u>.

Terms like Kuhn's 'quasi-analytic', Feyerabend's 'determining their own measuring operations', and Heisenberg's 'fundamental concepts that already determine the domain of validity of the corresponding laws' all point in the same direction although none of these conceptions and their possible differences is yet sufficiently understood. Perhaps the following remarks will be of some help.

We have seen that incommensurability is a relation between theories. In the extreme version envisaged by Feyerabend it may even be only a relation between languages. This, of course, would include also the underlying theories in as far as they underwrite the meanings of the terms in the languages. At any rate, incommensurability is incompatibility not of truth values but of meanings, and in its extreme form it concerns all parts of the languages that are still connected with meanings. Now physicists have come across this sort of incommensurability in the recent history of their discipline: In quantum theory we have already met with a relation that bears the very same name. Moreover, the expression means what it says: incommensurability is a relation between two observables, and it means that they cannot be measured jointly. Since an observable can be characterized by a boolean algebra of propositions telling us the possible outcomes of measurement, incommensurability becomes a relation of incompatibility between these propositions, different from and even excluding logical incompatibility. In the corresponding calculus this can be easily shown: Logical incompatibility of A and B is

$$A < B^{\perp}.$$

And from this if follows for the disjunction

$$(A \wedge B) \vee (A \wedge B^{\perp}) \vee (A^{\perp} \wedge B) \vee (A^{\perp} \wedge B^{\perp})$$

that it is 1. In the quantum logical calculus, however, incommensurability is just defined by

$$(A \wedge B) \vee (A \wedge B^{\perp}) \vee (A^{\perp} \wedge B) \vee (A^{\perp} \wedge B^{\perp}) \neq 1.$$

We can even have the extreme case that

$$A \wedge B = A \wedge B^{\perp} = A^{\perp} \wedge B = A^{\perp} \wedge B^{\perp} = 0$$

which sometimes is called 'complementarity between A and B'.

So the relation between incommensurability and logical incompatibility, so difficult to grasp in the general case, is here a matter of pure calculation. This would be of little interest if there were no closer relationship with respect to the content of the two concepts of incommensurability. But there is. In quantum mechanics the usual way of talking about objects having well defined properties, e.g. this particle has such and such a momentum, is no longer feasible. The state descriptions are irreducibly probabilistic, and other than probability statements one can only make statements about actually performed measurements. Now, if two of those virtual quantum mechanical statements contradict each other they cannot both be obtained as the result of one measurement. However, both can be <u>decided</u> by the same measurement. It is only that not both of them can come out true. It is entirely different with incommensurable statements. Here the <u>decision</u> about one of them brought about by a measurement, excludes a corresponding <u>decision</u> about the other. If our virtual statements are said to be meaningful under the presupposition that they have been decided by a measurement then we would have to say that the principle allowing a statement, for instance, of the form 'particle E has position x' to be meaningful excludes the corresponding principle for the statement 'particle E has momentum p'. Another way of putting this would be to say that the two sentence forms in question cannot have a common interpretation.

Now recall the kind of incommensurability from which we started in the first section. There we tried to grasp the idea that two whole languages are such that the rules of forming meaningful statements in one of them excluded the corresponding possibility for the other and vice versa. This was Feyerabend's extreme case of incommensurability where not even the question of logical incompatibility could come up. If,in the present context, we ask for languages standing in this relation we would only have to produce two maximal boolean algebras of the quantum logical algebra such that of any two statements if one belongs to one algebra and the other to the other, then the languages are incommensurable or even complementary. It is not difficult to find such languages, and more generally any two different maximal boolean algebras will have at least one pair of incommensurable statements. So we also have a whole spectrum between total commensurability and total incommensurability as mentioned earlier. The most remarkable thing, however, is that all these languages are sublanguages of one universal language and that only this superlanguage allows a complete formulation of quantum mechanics. Thus whereas initially the idea of combining incommensurable theories appeared to be nothing less than horrendous the first completely lucid example turns out to be one where an infinity of incommensurable languages is united in one comprehensive theory.

Our digression into quantum mechanics, if a digression at all, was certainly fruitful. The mere existence of quantum mechanics as perhaps the most successful scientific theory ever produced is evidence of a quite positive aspect of theory incommensurability. It is time to turn the tables and cease to stare at incommensurability as a stumbling-block to scientific understanding. Rather we should develop the idea and embody it in our thinking. This approach as a general epistemological move was proposed by Bohr for the first time in 1927. Under the title of "complementarity", this proposal was repeated by him for almost four decades without any appreciable impact on philosophers of science.[19] Given the foregoing considerations, we can define two viewpoints or theories to be complementary in the sense of Bohr if they are incommensurable (possibly in the extreme sense) but nonetheless can be united in one inclusive theory. This definition does not exclude the possibility that in this supertheory not only the two theories first mentioned are united but a host of other complementary theories. This at least is the case in quantum mechanics. At the same time it opens up a new form of radical theoretical pluralism in which the appearance of many incommensurable theories exhibits productive coexistence because there is, in some sense, a unifying theory.

But one must not become euphoric. The only fairly well elaborated theory containing complementary sublanguages is quantum mechanics. And, of course, it is a question of interpretation to see it in this way. Moreover, the incommensurability of contingent statements on position and momentum of a particle cannot easily be subsumed under the analysis of incommensurability that we have given in the foregoing section. Although our example of extreme incommensurability - the wave-particle pair - actually has been united in quantum mechanics, this is a case different from though connected with the system of complementary boolean algebras in quantum mechanics. And both are certainly different from the more moderate incommensurability that relates relativistic and prerelativistic physics. Although the concepts of position and momentum in quantum mechanics lead to so many incommensurable propositions there is full symmetry between them: You cannot say that one of these concepts has superseded the other. Both are equally legitimate concepts of quantum mechanics. *Mutatis mutandis* the same can be said about the particle and the wave picture of classical physics as having become limiting cases of quantum mechanics. Which of them has to be used depends on the physical situation, and neither of two such situations is 'better' than the other. It is only that you would better use the particle picture in one and the wave picture in another of them. Now, nothing like this could be said, for instance, with respect to absolute time and relativistic time. There we are convinced that in the last resort relativistic time is the more appropriate concept. It is the one that can always be applied, while the use of absolute time presupposes special conditions. They are a truely asymmetric pair, and so it is with all otherwise incommensurable concepts taken from this pair of theories. Thus we still have much to learn. But, as I have tried to show, the problems are accessible by the usual forms of conceptual analysis; it is unwarranted to say that there is something irrational about them.

Notes to Ch. VI

1 Basic are Kuhn [1962] and Feyerabend [1962]

2 See Lakatos and Musgrave [1970]

3 Kuhn [1982]. The following quotations are from pp. 670 f.

4 Feyerabend [1981 b], vol. 1, p. 238

5 Feyerabend , p.

6 Feyerabend [1973], p. 98

7 Feyerabend [1981 b], vol. 1, pp. 114 f.

8 Feyerabend [1965], p. 227, no. 19

9 Scheibe [1973 c], pp. 29 ff. gives a review of the major senses in which Bohr speaks of 'complementarity'.

10 Even Feyerabend is guilty of this mistake, see his [1981 a], vol.2 p. 446

11 Kuhn [1983]

12 Ehlers [1985]

13 Feyerabend [1965 a], p. 271

14 Feyerabend [1970], p. 300

15 cf. Scheibe [1988]

16 Feyerabend [1973], p. 101

17 Feyerabend [1981 b], p. 114, no. 27

18 Kuhn [1976] p. 198, no. 9

VII. Ludwig's approach to theory and theory comparison

1. Introduction

This chapter presents a fairly explicit and in some respect powerful approach to physical theory and theory comparison. It is the work of the german theoretical physicist Günther Ludwig, well known for his work in the foundations of quantum mechanics. This work is contained in numerous papers either in German or English, and it has recently been set out in two comprehensive books written in English.[1] It was in connection with his work on quantum mechanics that Ludwig was driven to ask general questions about physical theories and their interrelations. A first account appeared as the introduction to an earlier book of 1970 on the foundations of measurement in quantum mechanics[2], showing the origin of that account to be located in Ludwig's proper field of research. But in 1978 the book 'The Basic Structures Of A Physical Theory' appeared in German[3], and this book concerns exclusively at a very general level the problems of physical theories, their extensions and interrelations. The book is the most important contribution to the general methodology of physics during the last two decades. In the following I shall present an overview of what was achieved in this book and also some more recent papers in the field.[4] The emphasis will, of course, be on questions of theory comparison.

Put in a nutshell the basic attitude underlying Ludwig's book is that of logical empiricism or, perhaps even more to the point, of the Carnapian branch of logical empiricism. Most conspicuous is the almost ruthless reconstructionism practised in the book. After declaring that physics is an application of mathematics to reality Ludwig couches even the most general concepts in the field in the language of mathematics. Since on the level of generality characteristic of basic conceptual questions mathematics is just set theory Ludwig's book is hard to read even for mathematically trained theoretical physicists. The same holds for philosophers of science but for the different reason that they too often are overcharged in their working knowledge of first order logic. The situation thus is similar to the one reported earlier with respect to structuralism. Yet from a reconstructionist point of view Ludwig's reckless reformulation of physics is of interest mainly because similar attempts of the logical empiricists themselves failed.

The logical empiricist character of his enterprise is also evident from Ludwig's treatment of the problem of interpretation. His reconstruction, being basically syntactic,requires interpretation rules, and this is done, more or less subconsciously, in the spirit of a verificationist, indeed even an operationalistic theory of meaning. When it comes to questions concerning confirmation and empirical content the scenario is changed a bit. Here Ludwig slips into Popper's coat or at least into that of a left wing member of logical empiricism. There is an empirical basis, pretheories are permitted in the testing of other theories, and vis à vis empirical content Ludwig is always content to point out falsifying instances. But once his attention was drawn to this point he readily admitted that physics has received more confirmation from means other than the failure of serious attempts to falsify its theories. The remarkable thing about these coincidences with logical empiricism is that Ludwig, although having a certain background knowledge of current philosophy of science, certainly did not seek to fulfill the logical empiricist's or <u>any</u> philosopher's program. He just did what he found appropriate to do once he turned his attention to these foundational problems.

His independence is not only evidenced by the idiosyncrasy of his terminology but also by the fact that he shows himself to be a physicist who approaches philosophy with great caution. Interestingly enough, such remarks can be found in his comments on the development of physics, and I want to present Ludwig's view in this kind of context also. Besides his more special book Ludwig has also written a four volume "Introduction Into The Foundation Of Theoretical Physics"[5]. At the end of this work he says:[6]

> <u>If</u> what we mean by physics is confined to what has been obtained by applying "the method of physics" and <u>if</u> we assume that a physical theory gives us a picture of a substructure of the universe, then

> physics appears as a growing tree ... whose trunk becomes ever more solid and stable. This view has been rejected by a great number of philosophers of science and at face value the historical development may in some cases appear quite differently. Even physicists themselves sometimes have the impression that as physics develops the old things break down and are replaced by entirely new ones. However, our whole presentation ... aimed at showing that no earlier physical theories have been rejected or dismissed as 'false'. On the contrary, they are completed by more comprehensive theories and retain their importance as approximate theories. Seen in this manner the picture that physics obtains of reality develops in the direction of an ever better, more comprehensive and more precise picture. In this sense a physical theory also is not the product of a caprice of the physicists nor of any social structures ... A physical theory as far as it is a draft of reality is in a sense forced upon us by reality even if one cannot just be read from it.

A review of the chapter on the physicist's view of progress would place Ludwig in the more conservative branch of the tradition described there. He is but another example showing over and over again physicist's emphasizing the 'eternal value' of their theories when properly restricted. In the following passage, taken from his book on the basic structures of physical theory, Ludwig even holds philosophers fully responsible for the exaggerated interpretations of the development of modern physics. He first says:[7]

> If we pass on from a theory T_1 to a 'better' theory T_2, the theory T_1 is by no means dismissed. On the contrary, T_1 takes effect in its domain frequently only after one has learned to estimate its limits with the help of the better theory T_2 ... Thus, for instance, inspite of the discovery of relativity theory and quantum theory, classical point mechanics is as valid as it ever was.

This is clearly a conservatively minded formulation of the limiting case situation. And the revolutionary interpretation of scientific progress is rejected outright when Ludwig continues:[8]

> He who speaks of a revolution in the world view of physics in the sense of a revolution <u>within</u> theoretical physics has not understood the <u>essence</u> of that science. If a revolution has happened at all then it was the destruction of philosophical world views that were supposed to be substantiated by physics.

This cautious attitude as to what the claims of the physicists actually had (or should have) been conditions Ludwig's rational reconstruction of physical theory in which he explicitly provides for the expression of the limited accuracy of measurement (thus <u>weakening</u> the statements that can justifiably be made) <u>and</u> develops a powerful notion of intertheoretic relations that allows for a rational assessment of progress in physics.

VII. 2. Physical Theories

Let me prepare the treatment of intertheoretical relations by introducing first the notion of theory itself. As mentioned earlier, Ludwig's approach is semantical, i.e. a physical theory consists essentially of two parts: a formal theory and its interpretation in some domain of physical reality. There is, however, an oddity about the approach because briefly stated its formal theory is mathematics, indeed it is formal set theory, e.g. the system of Zermelo and Fraenkel (ZF). The use of such a strong formal theory raises interpretational problems: Since ZF is itself a first order theory with set membership as a *descriptive* predicate you would be more than entitled to ask what interpretations of set membership and ZF are foreseen when the goal is only the reconstruction of physical theory. However, I will not delve further into this matter, than to invite you to draw the dividing line between formal theory and physics in such a way that membership, and so mathematics, lies on the side of formal theory, so that correspondingly, we will be content with partial interpretations of set theory concerning only certain extensions of ZF having as *their* interpretations what is called somewhat paradoxically "abstract structures".

These abstract structures abound from mathematics where, for instance, groups, manifolds, vector spaces, Hilbert spaces, fibre bundles etc. are typical examples of certain classes or species of structures. They are the models of mathematical theories, the theory of groups, of manifolds, etc. respectively, and one frequently assumed reconstruction framework for these mathematical theories is the formal set theory ZF. Since for Ludwig, as we have heard, physics is the application of mathematics to reality the idea comes to mind to reconstruct a physical theory as an application of such a mathematical theory as exemplified in modern mathematics. There is nothing strange about this idea as soon as we realize that the mathematical theories in question are the natural generalizations of formal theories in the quite common sense of mathematical logic if logic, including many sorted and higher order logic, is replaced by set theory. A reconstruction of physical theory in many-sorted, higher order logic would, therefore, have led us to a subclass of mathematical theories anyway, and the only new step is simply the introduction of set theory as a particularly strong logic. Whether this step is necessary for the reconstruction of physics is not known. Whether it is desirable is perhaps a bit a matter of taste[9]. It certainly is desirable at least in the sense that, according to the present state of the reconstruction business, it is comfortable. To-day, a set theoretical reconstruction of general relativity theory is readily at hand. But I for one would not be so keen at embarking on a purely logical reconstruction. And an instrumentalistically minded physicist (or philosopher) would probably be quite happy with set theoretical reconstructions in general. On the other hand, the question whether important physical theories whose set theoretical reconstruction is known can be reaxiomatized as typelogical theories is a fundamental question for everybody who is sensitive to incomplete interpretation. The price for set theoretical comfort is, after all, interpretational gaps.

This will be immediately clear from a brief description indicating how those <u>mathematical theories</u> in set theory (species of structures in the sense of Bourbaki) are defined[10]. First one selects a double series

(1) $$X_1, \ldots, X_m; s_1, \ldots, s_n$$

of set constants. They determine the language of our theory consisting of all set theoretical sentences containing (apart from defined terms) no other set constants than these X_i and s_i. We then select a series of n scale terms

(2) $$\sigma_1(X), \ldots, \sigma_n(X),$$

i.e. terms constructed from their arguments (which besides the X_i may include defined terms of ZF) by the iterated operation of forming power sets of cartesian products. As axioms of our physical theories we then admit any sentence (abbreviated)

(3) $$s \in \sigma(X) \wedge \alpha(X, s),$$

where apart from an invariance condition that need not be considered here $\alpha(X,s)$ is an arbitrary sentence. From the construction of (3) two things are immediately to be seen. On the one hand, the first member of (3), the so-called typification, exactly corresponds to the choice of a type-logical language with <u>m</u> sorts of individuals corresponding to the $X_1, \ldots, X_m$ and with $\underline{n}$ predicates corresponding to the $s_1, \ldots, s_n$ whose types correspond to the scale terms (2) by which, according to (3), the s_i are typed. So, in these respects, axiom (3) talks about m-sorted individuals in terms of certain typical and possibly higher order predicates. On the other hand, the axiom proper α is quite arbitrary within the language of set theory, and this is <u>prima facie</u> an enormous generalization vis à vis the formation rules of type-logic: Whereas any type-logical sentence can immediately be translated into set theory <u>salve</u> <u>implications</u> the converse is not the case and may lead to highly non-trivial problems of re-axiomatization.[11]

A typical example from geometry shows what happens in more complex physical theories. Euclidean geometry in terms of congruence and betweenness can be settheoretically formulated <u>in</u> <u>one</u> <u>compact</u> <u>axiom</u> by using the method of analytical geometry. The axiom would say that there exists a coordinate system for space that turns congruence and betweenness into their arithmetical counterparts well-known from analytical geometry. Thus this axiom would be of the form - roughly -

(4) $$\bigvee \varphi.\ \varphi \in C_0 \wedge \varphi(\beta) = \beta_0 \wedge \varphi(\gamma) = \gamma_0 .$$

where β and γ are betweenness and congruence respectively and β_0, γ_0 their arithmetical translations. From (4) one can see immediately the two things typical of set theoretical reformulations of physical theories: First, there exists quantification over variables not belonging to the natural universe of discourse of the theory. Second, there is the use of defined sets; in the case before us, the set of real numbers. A literal translation of (4) into type-logical language is, therefore, out of the question. What we still <u>can</u> do, of course, is to ask whether there can be a type-logical axiom system <u>equivalent</u> to (4). In the case before us the answer is affirmative: Tarski has given a reaxiomatization consisting of twelve first order and one second order axioms. But this is a non-trivial procedure, and it is an open question whether a type-logical reduction is possible in every physical case. In one of the two books mentioned at the beginning of this chapter Ludwig answered the question for quantum mechanics the immediate set theoretical reformulation of which is even more involved than (4).

Let us turn now from the expressive power of set theory to <u>questions of interpretation</u> in the proper sense[12]. As mentioned there is, of course, no question of physical models for set theory. The problem of interpretation is restricted to the terms (1). Ludwig likes to think of these terms, insofar as (3) makes an assertion about them, as being a <u>picture</u> that we take of some part of reality. Thus it is primarily not the direction of interpretation but the converse direction of copying or portraying reality by means of those abstract structures that is intuitively satisfactory for him. Correspondingly, the principles governing this representation were originally called 'mapping principles' (Abbildungsprinzipien). But I will follow the custom of calling them interpretative rules. They are empirical interpretative rules, to be sure, in the sense that knowing such rules in a particular case means that we know how to write down the results of our experiments, measurements or observations made in the domain of application of our theory in the terms of (1). These outputs, the observational reports, are thus partial atomic diagrams. If our language, i.e. the scale terms (2), is of first order then apart from equalities and inequalities an observational report would be of the form (abbreviated)

(5) $\ldots \wedge a \in X \wedge \ldots \wedge \langle \ldots a \ldots \rangle \in S$ resp. $\notin S$.

The word 'observational' is not meant to imply immediateness of the procedure by which we obtain the results (5). The statements (5) are the primitive statements of the respective theory under discussion, and from a logical empiricist view they are in general already 'theoretical' and not observational statements. But according to Ludwig's terminology the process of obtaining his observational statements by means of observational statements in a more direct sense is part of the interpretation rules. Theories other than the one to be investigated may enter the process of determining the validity or non-validity of the primitive statements of the theory at stake. From the view point of the latter its observational statements are its primitive statements.

However, Ludwig's most important innovation in this part of the theory conception is yet to be discussed. It is the introduction of inaccuracies of measurememt into the observational reports. The pictures we take of reality are, so to speak, not sharp. Our instruments don't have an infinite resolving power, and this helps to explain the viability of our theories. If the observational statements being used to test a theory were absolutely precise any theory ever suggested would have been falsified by the first relevant observational report produced. But *given* a physical language the observational reports actually produced simply are *not* of the form (5). Rather they explicitly mention margins of experimental error. Every physicist is well acquainted with this inevitable practice. And the only question is how the phenomenon of experimental error can be described in general vis à vis the concept of theory so far developed.

The essential idea behind the introduction of imprecision of measurement into (5) is to relate the objects $a \in X$ *not* by the relations s in terms of which not only (5) but also the theory axiom is formulated but rather by a relation resulting from s by *blurring* it in a topological sense. Giving, for instance, the measured distance of two places in space together with some margin of error means that we do not relate the two places and their measured distance by the distance function in terms of which we express, say, that our space is euclidean. We rather relate the three objects by a relation obtained from the 'exact' relation by a process of blurring. The blurred relation being wider than the exact relation we thus *weaken* the empirical statements according to what we reliably observed. The procedure is based on the existence of a so-called <u>uniform structure</u> in each scale set in which the s of (1) are subsets:

(2') $$s \subseteq \sigma'(X).$$

Uniform spaces are mathematical structures lying somewhere between metric spaces and topological spaces. A uniform structure is a set of neighbourhoods in the square of the space. We can use these neighbourhoods to speak of two points of the space as being <u>indistinguishable</u> up to a given neighbourhood. A subset F of the space can be blurred with respect to a neighbourhood u by putting all points in the blurred set that are indistinguishable from a point of F up to u. Blurring a curve in a plane, for instance, results in a strip of finite (and possibly varying!) width containing the curve.

If we modify the observational statements of a theory according to these ideas they take on the form

(5') $$\ldots \wedge a \in X \wedge \ldots \wedge \langle \ldots a \ldots \rangle \in Bl(s, u) \text{ resp. } \in Bl(\bar{s}, u)$$

where u is the blurring neighbourhood and $\bar{s}$ the complement of s in $\sigma'(X)$. The earlier concept of observational statements (5) is a special case of the new one: There is always a finest uniform structure with only one distinguished neighbourhood which makes points indistinguishable only if they are identical and therefore makes the blurred sets equal their originals. Two comments on this procedure concerning its uniqueness should be added. First, the uniform structures used in (5') must somehow be distinguished by the theory (3) to which the observational statements (5') belong. We cannot just take <u>any</u> uniform structure whatsoever. Technically this means that those uniform structures must be <u>definable</u> on the basis of the theory. Secondly, the neighbourhoods u appearing in (5') are unique in the much weaker sense that they are meant to represent the accuracy of measurement available at the time. They are therefore unique (for each s) within a report pertaining to a certain time. Nonetheless it is the goal of experimentalists to increase the accuracy of measurement leading to ever smaller neighbourhoods u in (5').

VII. 3. Theory comparison

The foundations in the preceding section are useful for various purposes, and much of what has been said in previous chapters can be unified on the basis of Ludwig's concept of a theory. In this concluding section I shall illustrate this claim with respect to the important concept of approximate deduction, at the same time generalizing it in accordance with an idea of Ludwig. I shall not follow Ludwig in all respects but will blend his ideas with my own. In the first place this may not only be advantageous, as evidenced in the foregoing section, with respect to the presentation of the matter, but may actually constitute an improvement on the matter itself.

Recall Ludwig's general attitude toward progress. It is in accord with physicists' general conception of progress as outlined in chapter III. However, Ludwig also elaborates the concept of a relation between physical theories that can be used for the description of successive theories, the latter of which constitutes progress with respect to the former. He is, therefore, much more explicit than even those of his fellow physicists who have ventured to express an opinion on this matter at least in an informal though cautions way. Moreover, Ludwig has emphasized that the intertheoretic relation in question is not only useful in describing progress but is also frequently applied to obtain approximations of a theory that are more easily manageable, indeed if manageable at all, although the step to such approximations always means a regress.

The relation in question is given by an approximate deduction or rather a quasi-deduction. To explain what this means we start with an ordinary deduction of the form

(1) $$\Sigma(X,s) \wedge Y = P(X,s) \wedge t = q(X,s) \vdash \Theta(Y,t)$$

according to which the theory Θ on $\langle Y,t \rangle$ is deduced from theory Σ on $\langle X,s \rangle$ by means of deduction terms P and q that transform the structure $\langle X,s \rangle$ into $\langle Y,t \rangle$. Since this deduction is to be used for reducing Θ to Σ one would expect that the identities on the left side be admitted (or even required) to be synthetic identities. However, in this respect Ludwig proceeds as physicists customarily do and views these identities as "redefinitions". He actually uses the terms P and q to <u>define</u> the interpretation rules for $\Theta(Y,t)$ given the interpretation rules for $\Sigma(X,s)$. This is a procedure that cannot be enforced for arbitrary terms P and q. By no means does an arbitrary definition in set theory lead from empirically meaningful structures to empirically meanigful structures. Interestingly enough, the deduction terms P and q which Ludwig is convinced do the job of theory reduction in physics are at the same time conservative with respect to empirical content. I will mention here only those for which

$$P_v(X,s) \subseteq X_{\mu_1} \times \ldots \times X_{\mu_k}$$

can be proven from Σ. They are clear cases of domain restrictions.

Leaving out extraneous details consider two generalizations of (1). Ludwig allows for reductions - apart from all questions of approximation - in which only a substructure of a structure satisfying Θ can be deduced. (In fact he has a more general concept of "quasi-deduction". But the one just mentioned will do for us.) Its formal expression is

$$(2) \quad \left\{ \begin{array}{l} \Sigma(X,s) \wedge Y = P(X,s) \wedge t = q(X,s) \\ \qquad \vdash \bigvee \eta\, \tau.\ \Theta(\eta,\tau) \wedge Y \subseteq \eta \wedge t = \tau | Y. \end{array} \right.$$

where $\tau|Y$ means the restriction of the structure τ over η to the subset $Y \subseteq \eta$ - a process that can be defined quite generally. An example where (2) applies is the reduction of the theory of Galilean spacetime to the theory of Minkowskian spacetime. Expressed in terms of the corresponding groups the situation is that one cannot approximate the whole Galilean group by the Lorentz group. One can do that only in the vicinity of the identity. Only part of the Lorentz group - the restriction by P on the right side of (2) - can be identified approximately with part of the Galileo group - the restriction on the right side of (2). The same example demands a second generalization of (2) in a sense which, however, needs no separate expression in this sketchy presentation. The point here is that the foregoing consideration would properly concern not the groups but the totality of inertial systems. But to identify the latter with its corresponding group you must distinguish one inertial system, i. e. properly speaking the theory from which the deduction originates is not the theory of Minkowskian spacetime but an extension of it. And so it is in general. Formally, of course, this extension may thought to be already contained in Σ. It is only that this is not the theory which we normally would call the reducing theory.

Let us now come to approximation. We have already touched on it when discussing the inaccuracy of measurement in an observational report (see 2. (5')). There first-order atomic statements

$$\langle \ldots b \ldots \rangle \in t \qquad [t \subseteq \tau(X)]$$

were weakened by means of a uniform structure in $\tau(X)$ to become

$$\langle \ldots b \ldots \rangle \in Bl(t, u)$$

where u is a neighbourhood belonging to the uniform structure. The process can be generalized to arbitrary statements

$$\Gamma(Y, t)$$

by defining the blurring to be the statement

(3) $$\Gamma_u(Y, t) \equiv \vee t'.\ \Gamma(Y, t') \wedge t' \subseteq Bl(t, u) \wedge t \subseteq Bl(t', u).$$

Thus instead of stating $\Gamma(Y,t)$ we would - given u - make the same statement only for <u>some</u> t' that cannot be distinguished from t with an accuracy that would be greater than the one given by u. Γ_u depends transitively on u with respect to deduction, more precisely

$$(4)\quad \begin{cases} \bigwedge u u'.\ Unif[\tau(X), U] \wedge u, u' \in U \Rightarrow \\ \qquad u \subseteq u' \Rightarrow \Gamma_u^*(Y,t) \rightarrow \Gamma_{u'}(Y,t). \end{cases}$$

This concept of uniform weakening of a set theoretical statement about a structure can now be used for the definition of approximate deduction. The typical situation in which approximate deduction is possible it that 1) the deduction terms depend on an approximation parameter and 2) natural uniform structures can be introduced in the relevant scale sets of the deduced structure. They are defined by terms depending on the deducing structure. In the usual abbreviational notation approximate deduction can then be defined by the provability (in ZF) of the formula

$$(5)\quad \begin{cases} \bigwedge u \in U(X,s)\ \bigvee \varepsilon\ \bigwedge Y t. \\ \qquad \Sigma(X,s) \wedge Y = P_\varepsilon(X,s) \wedge t = q_\varepsilon(X,s) \Rightarrow \Gamma_u(Y,t), \end{cases}$$

where $\Gamma_u(Y,t)$ is the uniform weakening of the right side of (1) or (2).[14] It has to be noticed that approximate deduction is a purely syntactical concept. It relates the species of structures Σ and Θ with respect to a given family of deduction terms <u>and</u> the chosen uniform structures. If you think of $\mathcal{E}_0$ and $\mathcal{U}_0$ as being specially defined terms you can have implications of the form

$$(5') \qquad \Sigma(X,s) \wedge Y = P_{\Sigma_0}(X,s) \wedge t = q_{\mathcal{E}_0}(X,s) \vdash \Gamma_{\mathcal{U}_0}(Y,t)$$

that in a sense represent a certain finite stage in the infinite process given by (5). It is, of course, these <u>finite</u> stages that are important for the application of (5) to concrete examples. In this way the known inaccuracy of measurement also can be taken into account by an appropriate choice of $\mathcal{U}_0$. $\Gamma_{\mathcal{U}_0}$ will then be that weakening of the theory Γ that it is empirically meaningful to get explained. From the practical viewpoint the meaning of an <u>infinite</u> approximation can only be that it gives you further and further restrictions (by means of $\mathcal{E}$) such that, <u>if</u> the accuracy of measurement could be increased up to a certain point, under those restrictions it would still be meaningful to explain Γ by a proper deduction.

Almost all the known limiting cases in physics are covered by the above concept of theory approximation. One exception is the standard procedure of approximating <u>general</u> classical statistical mechanics by quantum mechanics. This procedure is incomplete because it does not take the dynamics into account. It only concerns the states and the observables. If one even restricts the procedure to the observables, (5) seems to be applicable. But there is a difficulty with the state space because in the procedure it is made dependent on the approximation parameter which is not allowed in (5). I think however that this problem will be solved in the near future. There are other difficulties with respect to general relativity theory and its Newtonian limit. In the main they do not concern the applicablity of (5) but are rather mathematical difficulties to be overcome in this special case. But there are also difficulties of translating the usual presentation of the matter in physics into the present framework. There is finally the fact that all efforts hitherto made have concerned a comparison of general relativity with Newtonian field theory of gravitation. The approximation of the particle version seems to be discussed nowhere and will remain behind the horizon until the n-body problem is solved in general relativity.

Notes to Ch. VII

1 Ludwig [1983] and [1985]

2 Ludwig [1970]

3 Ludwig [1978]

4 Ludwig [1981 a], [1981 b] and [1984]

5 Ludwig [1974 ff.]

6 ibid. vol. 4, pp. 477 f.

7 Ludwig [1978], pp. 81 f.

8 ibid.

9 These questions are dealt with recently from a nominalistic viewpoint in Field [1980]; see also Scheibe [1986]

10 See Bourbaki [1968], Ch. IV

11 See the references in no. 9

12 For the rest of this section I follow essentially Ludwig [1978] with some major simplification. See his 1981 a and the presentation in Scheibe [1982].

13 The reader has to compare the following presentation with Ludwig [1978], Ch. 8 and Ludwig [1981 b].

14 In getting straight formula (5) a discussion with Peter Woodruff was very helpful.

VIII. Coherence and Contingency. Two neglected aspects of theory succession

By calling coherence and contingency two neglected aspects of theory succession I do not mean to imply that _philosophy_ in general has neglected these concepts. Even if they are put into the context of the _development of human knowledge_ in general we can find them treated now and then in the history of philosophy. What is missing is their re-evaluation from the viewpoint of _scientific development as a complex network of theory successions_. From time to time we have to review the work of our predecessors and put it into the perspective to our own endeavours. In the case before us we must ask what the impact of the traditional views on coherence and contingency would be on present views of theory succession. My intention is not a presentation that would pass the judgement of an historian. My ambition is more of a systematic kind. I want to look at the development of science as characterized by an _increase of both coherence and contingency_.

This suggestion may not come as a surprise to a contemporary philosopher of science. The striving for unity in science may be expressed by saying that science strives to become more and more coherent. And is not the increase of contingency but a mirror image of getting at ever more universal laws? To repeat, I do not claim any originality in matters of principle. But as to the first point, the view of the unity of science in the movement known as logical empiricism has not resulted in <u>any</u> useful suggestion for an explication of coherence or - for that matter - unity. Nowadays one who wants to begin a study of induction is still not badly advised to read Carnap's "Foundations of Probability"; but one who wants to study the concept of coherence has to start from scratch. As to the second point the change from growing universality to increasing contingency is perhaps mainly a matter of philosophical emphasis. It sounds great to have the final, all inclusive law of nature. But what if it leaves us with a world in which almost everything happens by chance? Would this be the maximum amount of coherence to be obtained? Here comes what my major interest is in this paper: I will urge that we consider not the increase of coherence or contingency <u>separately</u>, but the simultaneous occurrence of <u>both</u>. In a sense the development of science seems to be characterized by the occurrence of both. However if you say this to a coherence theorist, he will not only be surprised, he will blow you in your face. So here we have a <u>problem</u>, one which I will not try to solve here but rather one to which I will give a more detailed and more precise description.

1. Coherence and Contingency: An Introduction

Let me begin this introduction to coherence and contingency by citing two representative philosophical positions. The first is the view of coherence in philosophical rationalism. It is characterized by the belief that the world can be understood or, more modestly, that understanding of the world is a supreme goal of human endeavour. The time-honoured tradition of rationalism is best represented in our time in the books "The Nature of Thought" and "Reason and Analysis" by Brand Blanshard.[1] In these he gives a formulation and a defense of a rationalistic epistemology that in some parts is of considerable heuristic value to my view of current philosophy of science.

The following passage[2] from Blanshard's "The Nature of Thought" gives an impression of his major concern and at the same time introduces the concept of coherence:

> ... reality is a system, completely ordered and fully intelligible, with which thought in its advance is more and more identifying itself. We may look at the growth of knowledge ... as an attempt by our mind to return to union with things as they are in their ordered wholeness ... And if we take this view, our notion of truth is marked out for us. Truth is the approximation of thought to reality. ... Its measure is the distance thought has travelled ... toward that intelligible system ... The degree of truth of a particular proposition is to be judged in the first instance by its coherence with experience as a whole, ultimately by its coherence with that further whole, all-comprehensive and fully articulated, in which thought can come to rest.

How can we deal with these breath-taking sentences? Can they help to understand the development of science? Our reaction should be measured by looking first at the other extreme, a position which since the time of Hume has had enormous influence and is today best known as logical atomism. Logical atomism is an ontology, copied from the language form of modern logic. Wittgenstein has couched it in the cryptic statements:[3]

1.2 The world devides into facts

1.21 Each item can be the case or not the case while everything else remains the same

6.3 The exploration of logic means the exploration of everything that is subject to law and outside logic everything is accidental.

Obviously, this is a position that the world is totally contingent. Ontologically this means that not only can we imagine the world to be other than it is, but to the full extent of logical possibility, it could be different from what it is. It is no surprise that Blanshard has called logical atomism "the most formidable attack ever made on reason as an independent source of knowledge".[4] Indeed even if the matter is considered solely from the viewpoint of modern science logical atomism is unacceptable. You cannot accommodate the struggle in physics for law and order by simply pointing out that before the throne of logic all are equal - from Newton's laws of mechanics down to the most trivial statements about one's present sense impressions.

Indeed it is hard to believe that science would be happy with either of the mentioned opposing philosophical positions. One would rather expect that for the description of science and its development both conceptions - coherence and contingency - will be useful when combined in an appropriate manner. Moreover, it is not difficult to find a starting point for such a combination; it is the basic structure of physical theory as the most systematized outcome of scientific theorizing. The starting point then is the following dualistic structure: On the one hand, a physical theory for which at least local validity is claimed for its laws, and on the other hand, a class of so called specified initial and boundary conditions. For the latter the theory leaves it open whether they are valid or not. The paradigm of this basic structure has been Newtonian celestial mechanics where the unconditional validity of the gravitational law is set against the complete arbitrariness of the positions and momenta of the bodies involved at a given time. But the new theories of this century are quite similar vis a vis the point in question: Einstein's field equations have many solutions that are restricted only by initial and boundary conditions, and in quantum mechanics the Schrödinger equation does not allow determination of single probabilities. It seems in general that a physical theory qua theory does not answer every question that it permits to be raised. In this respect an element of contingency is present in every theory. On the other hand, given certain initial and boundary conditions the laws do allow one to draw many contingent conclusions which, without these laws, would be completely disconnected from those same conditions. There is, therefore, also an element of coherence thereby introduced into the theory.

How does this come about? There are two steps. The first step is the decision to reconstruct a physical theory in the sense of Aristotelian axiomatics, i.e. as a system of primitive concepts and basic axioms grounded on a logic that allows one to give definitions and draw inferences. According to the present state of the art there are a great number of logics available. But they have one feature in common: They use atomic languages, i.e. languages built from certain elementary parts like a house is built from bricks. More precisely, every sentence of such a language is constructed from atomic sentences and sentence forms by means of logical connectives and quantifiers. There is, therefore, a sharp division of the language into logically simple and logically complex sentences. Therefore, secondly, if one wants to describe some piece of reality in an atomic language, two extreme possibilities suggest themselves. One could try to give the description by using atomic sentences and their negations only, <u>or</u>, secondly, one could try to avoid atomic sentences altogether and use instead pure sentences of unbounded logical complexity. <u>But</u> one could, of course, also do both, and this, infact, is what is actually done. But the two possibilities represent quite different functions. The logically complex propositions are used as possible axioms of a theory. In other words, they are used to express the laws of physics. The atomic statements, on the other hand, are used to formulate the initial and boundary conditions and, with them, possible observations.

I have associated the aspect of contingency with the initial and boundary conditions and the aspect of coherence with the laws of physics. We can now describe the situation in a more general fashion. An atomic diagram - a complete atomic description - taken by itself is totally contingent precisely in Wittgenstein's sense: Each item can be the case or not, be the case , yet everything else remain the same. Or - equivalently - if from any subset of an atomic diagram, an atomic proposition or its negation can be inferred, then this proposition is an element of that subset. In the presence of laws, however, the situation may change. The laws induce dependencies in the diagram enabling us to draw non-trivial inferences from one part to other parts. In other words, laws introduce an element of coherence into an otherwise entirely disconnected aggregate of atomic propositions. In the search for laws physics did, if only in this sense, decide in favour of coherence although it can not avoid introducing contingencies; there simply must be something to go on.

2. The increase of contingency

In the previous section we have seen that although philosophers tend to maximize the importance of either coherence or contingency to their mutual exclusion, in science they are found together. The important question is: What happens as science develops? In this section I will urge that, again and again, a typical step in the development of science is the recognition that what conventionally has been taken to be a lawlike affair really is a matter of contingency while the reverse never occurs. In the progress of science the increase of contingencies is vital.[5]

By way of introduction I shall provide two or three examples from the history of science. In these examples my paradigm of a contingent entity is some part of an object that may be different from what it in fact is because it may undergo just a change in time. In contrast a lawlike entity is a timeless structure, - timeless in the strong sense that a change in time is excluded as a matter of principle. Recall the passage in Plato's Phaedo (78 b3 ff) where such a pair of opposites is introduced and characterized in two ways. Trying to prove the immortality of the soul, Socrates starts his investigation in this part of the dialogue with the question: "For what kind of thing should we fear that it may be dispersed, and for what kind should we not?" An initial answer is suggested by the following characterization: "Isn't it most probable that the incomposite things are those that are always constant and unchanging ( ), while the composite ones are those that are different at different times and never constant ( )?" But there is also a second answer using a different characterization. Immediately after the passage quoted Socrates goes on to ask: "What of that very reality of whose existence we give an account when we question and answer each other? ... Can the equal itself, the beautiful itself, the being itself whatever it may be, ever admit any sort of change? Or does each of these real being ..., remain unchanging and constant, never admitting any sort of alteration whatever?" As opposed to these timeless entities Socrates then refers to "the many beautiful things, beautiful human beings etc. ... What about all the things that are called by the same name as those real beings? Are they constant, or in contrast to those is it too much to say that they are never identical with themselves ...?" Putting aside questions of interpretation concerning this text, Plato's distinction may very well serve as a first approximation of the kind of distinction to be used in formulating the following examples. Moreover, whereas Plato's first characterization seems to be more appropriate for application to the earlier stages of science, the second seems appropriate to more recent developments.

The first example is the development of our insight into the structure of matter. It has passed through the four levels of state transformations of substances, of chemical reactions, of radioactivity as the spontaneous decay of heavy atomic nuclei and of the decay of elementary particles first observed in cosmic rays. In each stage it was recognized that what at first had been conceived to be an unalterable structure - a state, a chemical compound, an atomic nucleus, an elementary particle - finally turned out to be changeable in time. In each case a deeper structure was discovered not to change during the respective transformations: the chemical constitution is not changed in a state transformation, the atomic nuclei are essentially stable during a chemical reaction. Processes within nuclei usually are accompanied by transformations of elementary particles. But at least some quantities characteristic of this level are conserved. It is at this point that it became clear that it is not a constituent of the object undergoing a process that remains constant during the process. In terms of Plato's two characterizations this would mean a shift from the first to the second, more abstract characterization as being appropriate to describe the situation. The behaviour of the elementary particles has confirmed this view.[6] Altogether, we have here a succession of theories - classical mechanics, thermodynamics, chemistry, nuclear physics and elementary particle physics - having had their _floruit_ in this order and each explaining a new kind of process that was veiled by assumptions made by the preceding theory.

The second example is from the history of cosmology. Apart from rare movements to the contrary, the predominant world view of antiquity, and its christian version during the Middle Ages, were essentially static. The earth was thought to be at rest in the center of the universe, the celestial sphere revolving in uniform circular motion with the stars fixed to it. There were the planets exhibiting their rather irregular motions.However, saving the phenomena, these motions were explained by reducing them to constant circular motion. The development begun by Copernicus gradually destroyed these simple structures in favour of more and more changes in time and other contingencies. The earth moves, the stars move. Theories about the genesis of the solar system arose, the stars were declared to be alterable products with birth and death. Finally, gravitation, the new static quantity, and euclidean space, the time-honoured structure, were merged into one time-dependent metric in general relativity. And this theory tells a story about the universe according to which its original state was toto coelo different from what it is now.

Certainly, stories like these could be multiplied, not the least of which is the evolution of organisms where again the seemingly timeless structures of living species were recognized to have a history. For the present the examples may suffice to show that there is a uni-directional shift of the borderline between what is still assumed to be a timeless structure and what is currently recognized as being capable of change. Hypotheses and discoveries resolving timeless structures into processes are of incisive importance because they often lead to the assumption of new and more basic structures. More generally, the frequent concomitant of the replacement of one theory by another one is the emergence of a new contingency in the sense that some part of the old theory can be seen to correspond to some part of the new theory which according to the latter is recognized and explicitly admitted for the first time to have genuine alternatives, not only in the sense of possible change, but also in the more general sense of logical alternatives. A general description of the process of increasing contingency suggests itself by making use of the idea that theory succession is accompanied by explanations of the earlier theories by the later ones.

That science develops no one will deny. But there has been much opposition against the view that science develops in such a way that its earlier stages are always *explained* by later ones. A more specific formulation of this view has been given, for instance, by Popper when he says that it is the very *aim* of science "to explain what so far has been taken to be an explicans, such as a law of nature." "Thus", - continues Popper - "the task of empirical science constantly renews itself. We may go on forever, proceeding to explanations of a higher and higher level of universality ...".[7] I accept this view of the development of science, especially physics, conceding that as of yet no satisfactory general concept of explanation suitable for the description of the development in question has been elaborated.[8] But for the current purpose I need only to employ some rather general features of this concept. The most important is that whatever part A of an earlier theory T is recovered from its successor theory T' will be recovered my means of (absolutely) contingent propositions *c* specifying the particular conditions under which according to T' that part A of the earlier theory T holds as widely as it does if it holds at all. This may be symbolized as follows:

The *precise* definition of this relation does not matter much as long as it implies that within the new theory T' alternatives to A become known for the first time. In this way the earlier theory, formerly the Last Word in the field, becomes contingent relative to its successor. It is in this broad sense that there is an increase of contingency, displayed by the conditions c, as physics develops.

In addition to the examples already mentioned there are many other cases that are subsumable under this conception of theory succession. The step from Kepler to Newton is a shopworn but still instructive case in point. For Kepler who, although a Copernican, still believed in the old cosmology of the celestial sphere, the sun and the planets known had a quite unique and exceptional position in the universe. Kepler found the beautiful regularities expressed in the laws named after him and, although he already entertained the notion of a force exercised by the sun on the planets, he still tried to understand the relative distances of the planets from the sun. In his view the solar system was an essential constituent of the structure of the universe to be understood precisely as given to us. So, it would not have made much sense for Kepler to have entertained any alternative or to have asked for the particular conditions under which the planets showed the regularities that were discovered by him. This was left to Newton and his followers in the 18th century. They came to realize that neither the solar system itself nor Kepler's laws about it are the kinds of thing that are necessarily immutable. In their view the former became a brute fact that could be understood only by asking for its genesis, and the latter were explained within Newton's theory of gravitation by pointing out those particular conditions under which Kepler's laws are approximately true.

The history of science provides an abundance of cases where - as in the Kepler-Newton case - basic lawlike assumptions loose their privileged status of being the Last Word in the field and thereby become contingent on the new view of the Last Word. There were cases of minor importance such as those where only correction terms are attached to some law, there were cases of fundamental importance as was the replacement of classical mechanics by quantum mechanics and the transition from pre-relativistic to relativistic physics. Sometimes the development of a radical change led to a series of steps following each other in rapid succession. Such was the case with the treatment of the electron by the Schrödinger equation, Pauli equation,Dirac equation and quantum electrodynamics. Though, our understanding of the relationship between succeeding theories admittedly is far from satisfactory, I am pretty sure that, whatever the details may be, the relationship can be reconstructed in such a way that the notion of an increase of contingency will be among its key features.

3. The increase of coherence

Turning to coherence, the development of physics shows an increase in coherence, too. It would, of course, be good to know beforehand what coherence is. However, as emphasized in The Introduction this basic question is difficult to answer. After having mentioned the numerical reduction of languages and theories to one of each, in their article on the "unity of science as a working hypothesis", Oppenheim and Putnam[9] go on to say that "unity of science in the strongest sense is realized if the laws of science are not only reduced to laws of some one discipline, but the laws of that discipline are in some intuitive sense 'unified' or 'connected'". Obviously, the authors could equally well have said "coherent". Then they say: "It is difficult to see how this last requirement can be made precise; and it will not be imposed here". I am afraid that the situation has not essentially changed from this paper in 1958. The ideas about coherence to be put forth in what follows are not meant to be the Last Word on the matter but only serve to fix the mind in order to facilitate the basic idea of an increase of coherence.

Coherence, in the sense of the Introduction, is a relative property of a (possible) axiom system. It is a measure of the amount of dependency induced by the axiom in an atomic diagram. (Instead of atomic sentences one could also use some other basis of absolutely contingent statements, e.g. Hintikka's constituents.

But atomic statements are certainly the most simple choice.) To have a concrete idea of the degree of coherence that may be obtained in this case think of the theory of linear order that can be defined by three very simple axioms. Given the length N of a sequence, a complete atomic description of it consists of N^2 statements. Using the axioms this number is reduced to N-1. Thus if we had describe a macromolecule consisting of 1000 molecules ordered in a sequence we could do this by means of about 1000 statements from which, together with the laws, the 999,000 other atomic statements would follow. If we define the degree of coherence to be the quotient of the number of statements saved by the laws and the number of a complete atomic description, then this quotient converges to unity.

The reducing effect of the differential equations of dynamical theories is even much stronger than this simple minded example shows. Differential equations can reduce an infinity of contingent statements to a finite subset. Accordingly, the step from a description of a phyiscal system only using atomic statements to one only applying laws governing the behaviour of the system as a whole is accompanied by a considerable gain in coherence in the sense under discussion. Related concepts of coherence come to the mind when we ask ourselves the difficult question: What direct properties of a theory bring about those reducing effects in contingent descriptions? It can hardly be answered in general.

But there can be no doubt that physics avoids decomposable or factorizable theories. It was, by the way, an idealist, the british Hegelian Bosanquet, who once asked: "Is there any man of science who in his daily work, and apart from philosophic controversy, will accept a bare given conjunction as conceivably ultimate truth?"[10] But what can this rhetorical question imply that we must avoid?It is something like this: Given an axiom system, a reaxiomatization is possible which splits into two parts using disjoint languages:

(1)

This can be rewritten as

(2)

with constants a and b such that not all of the must actually occur in A or B. So (1) is contained in (2) but, obviously, (2) is more general than (1). To obtain coherence we could exclude (1) or even (2) in the sense that given any two disjoint and or different a and b there can be no reaxiomatization (1) or (2) respectively.

Coherence conceptions like these can be illustrated most impressively by the way in which the interaction between physical systems is treated in classical physics. Some of what classical physics says about a system consisting of two subsystems even fits form (1). But it is a trivial part when compared with the interaction. That there are non-trivial aspects, I shall have occasion to discuss when we come to quantum physics. At any rate in classical physics the non-trivial part is the interaction introduced by a dynamical law of the theory. And it is this law that makes the theory coherent in the sense of avoiding (1) or even (2). A famous example is Newton's theory of universal gravity and the step from Kepler's theory of the solar system to Newton's. According to Kepler's theory any planet moves independently of any other. The statement how all planets move is the bare conjunction of the statements concerning the movements of each individual planet. By contrast, the theory of universal gravity, introducing an interaction also between any two planets, is a non decomposable theory representing a considerable gain in coherence when compared with Kepler's theory. An outstanding example of its fruitfulness was the discovery of the planet Neptune, grounded on a prediction from data pertaining exclusively to two other planets. Such a prediction is impossible in Kepler's theory. In general, the coherence of Newton's theory verifies (and even makes intelligible) many assertions of philosophical coherence theorists. What that theory has to say about one body as being a gravitating body cannot be said other than by relating it to every other body in the universe. Moreover, if one were to find a system of bodies moving exactly according to Newton's theory, the very same theory would imply that system to be all-inclusive. In other words, the part can only be understood by referring to the whole, and a completely coherent system must be the whole.

The development from decomposable to irreducible theories can have the peculiar feature that the entities connected with the decomposable theory loose their independent existence and somehow are absorbed into a larger whole. The step from quantum mechanics to quantum field theory displays such in the various transformations of elementary particles. The unification of static electric and magnetic fields in electrodynamics is an even earlier example. Its foundation is probably the most remarkable instance: the development from Newton's view on space and time to Einstein's special relativity. Newton's theory of absolute space and absolute time is the paradigm of an incoherent theory, - a bare conjunction of two theories referring to two quite different subjects. In modern terms: Newton's spacetime is just the direct cartesian product of space and time. Then galilean spacetime was developed. In it the concept of space no longer occurs as an independent entity. Consequently, the corresponding theory is no longer decomposable into two independent subtheories. However, the new theory still contains a theory of absolute time as a subtheory built on a proper sublanguage. From the special relativistic spacetime also time has been extirpated. In 1908 Minkowski described the situation not unjustly in his famous saying: "Henceforth space by itself and time by itself shall become degraded to mere shadows and only some kind of union of them shall remain independent."[11]

4. Coherence and contingency: A point of view

The result of our considerations has been that as physics develops the network of its theories becomes less coarse in virtue of an increase of coherence while, at the same time, in some sense of the word "increase", the contingency woven into that network also increases. In this concluding section we must try to understand how this is possible. The best way to approach the entire matter is to try to understand why it is even necessary.

Let me first make it quite clear that an increase of coherence leads of necessity to a decrease of contingency in the sense in which the two concepts were envisaged in the Introduction. "Coherence" there meant the amount of connection introduced into an atomic diagram by axiom systems consisting of "lawlike" sentences, e.g. pure sentences containing no constants of the type of variables quantified over in the very same sentences. And "contingency" meant just disconnectedness within a set of statements as most impressively illustrated by those atomic descriptions. Consequently, and trivially, an increase in either of them means a decrease in the other. And this is the case not only at the lowest level, defined by atomic statements. To be sure, in physics to have some theory at all presumably requires at least this: there are lawlike connections between atomic statements. But once this stage is reached we can ascend to higher levels. In them, too, disconnectedness will occur although it becomes more difficult to grasp. And it will be reduced by even higher level theories. The reduction of many experimental laws by general electrodynamics is a well known case in point.

So there is, no doubt, the complementary pair of coherence and contingency. But there also is contingency in another sense.[12] Although not directly related to coherence itself, its change is related to a change of coherence and indeed in such a way that an increase of the latter necessarily is accompanied by an increase of the former. "Contingent" in this new sense means "known to have alternatives". This sense of contingency, therefore, will increase whenever something that up to a certain point in time has been considered categorical comes to be viewed as having alternatives. Now precisely this happens as part of an increase of coherence. If a couple of hitherto uncorrelated physical statements of whatever level becomes correlated by a new coherent theory then they will be explained by this theory in the sense indicated in the previous section, i.e. absolutely contingent conditions will become known under which those statements hold. The corresponding increase of contingency in the sense under discussion is here reinforced by the appearance of those conditions called absolutely contingent. I will try to make clear what I here mean by "absolutely contingent propositions" in two ways.

First, as a matter of logical fact, contingency in the sense inversely proportional to coherence, cannot be reduced to zero. Even for categorical theories a model, although uniquely determined up to an isomorphism, cannot be derived from the theory alone. However advanced our theories may be there will remain a residual of statements that together with the theory have to be assumed in order to construct a model. This situation will not be changed no matter how high increases of coherence are involved. And it is for this reason that an increase of coherence can lead to explanations of the kind described above. Another way to see the matter is to imagine a list of all explanations in question that have ever been given in the history of physics. Then in the _premises_ of these explanations we can distinguish the fundamental assumptions of the respective theories from the contingent assumptions added to them for the sake of explanation. Call the former to appear in a _L-position_ and the latter to be in a _C-position_. Then, although many of the premises of our explanations will also occur as explananda of other explanations in the list, it will _never_ happen that a proposition occurring in a L-position in one explanation will occur in a C-position in another explanation and _vice versa_.

Despite this emphasis on absolutely contingent propositions, the relative weight of the two extreme philosophical positions from which we started out can be expressed by saying that, in physics, _of course_ there is increase of coherence, there is unification and perhaps even the fruit of an eventual unity of physics. But there is also this apparently inexhaustible reservoir of contingency more and more of which becomes known as such. And it is only at the price of its actual increase that coherence can grow. Knowledge will therefore not progress in the sense of absolute understanding although there will always be local progress.

In concluding, let me touch briefly on an aspect of coherence and contingency that, although also being of first importance, seems to be completely different from all that has been said so far. What has been said so far exclusively concerned the coherence and contingency status of the fundamental assumptions of a physical theory and their change. Consider, however, perhaps the greatest advance made by physics in our century, the step from classical to quantum mechanics. In this case there is an increase of coherence of an entirely different kind which concerns not the fundamental assumptions but rather the contingent descriptions provided by a theory. And perhaps the most striking fact is that here, too, there is a corresponding increase of contingency.

In classical physics a system consisting of subsystems is described as the direct cartesian product of the subsystems. This means that a complete contingent description of the total system simply is the conjunction of the complete contingent descriptions of its subsystems. Consequently, there are no inferences from data pertaining to one subsystem at time t to any properties of another subsystem at t. Inferences from one subsystem to another one can only be grounded on an interaction between the systems and will then involve at least two different time points. The situation is entirely changed in quantum physics. The quantum theoretical mode of description of a system consisting of subsystems has the remarkable feature that a complete contingent description of the total system at time t does not generally imply complete descriptions also of the subsystems at t. In fact the overwhelming majority of total states lead to incomplete descriptions, the missing information having drifted away into contingent so called EPR-correlations between the subsystems. Thus instead of having fairly definite information of what the result of a measurement of observable A of subsystem I will be we are definitely informed only about what this result would be if we were to measure a certain observable B of the other subsystem II. Thus we here do have some coherence between subsystems at a given time, and although this coherence may be brought about in controlled way by means of an interaction, its nature seems to have nothing to do with the latter and can be described independently.

This sort of coherence is characteristic for quantum physics and completely foreign to classical thinking. It can be used to illustrate traditional philosophical ideas on coherence even more impressively than the other type of coherence. For the quantum theoretical coherence allows the thesis that the domain of validity of quantum theory - and that is according to some authors the entire universe - strictly speaking admits of no isolated objects but is rather an undivided whole. However, the step from classical incoherence to quantum mechanical coherence of subsystems also seems to necessitate a simultaneous increase of contingency. As seen from classical mechanics, the process of its quantization consists in first destroying the independence of position and momentum of a particle and making them complementary observables. This new relation, however, cannot exist without the introduction of an infinity of quantum mechanical observables that have no classical counterpart whatsoever. They also come in complementary pairs, and as a consequence have entirely independent empirical interpretations. So here again, coherence brings with it a wealth of new contingencies. Seemingly they make as simple an object as an electron as complicated as any many-particle system can be.

Notes

1 Blanshard [1939] and [1961]

2 Blanshard [1939], vo. II, p. 264

3 Wittgenstein [1922]

4 Blanshard [1961], p.

5 Scheibe [1987]

6 In his later writings Heisenberg liked to give this situation in elementary particle physics an interpretation in Platonic terms; see Heisenberg [1984-5]

7 Popper [1958], p. 26

8 More details in Scheibe [1984] and [1986]

9 Oppenheim and Putnam [1958], pp.

10 Quoted from Blanshard [1939], p. 511

11 Minkowski [1909], p. 54

12 Karel Lambert has suggested not to use the term "contingency" in this (major) sense because it could easily lead to misunderstandings. I feel that he is right. But in spite of honest efforts discussion did not lead to a suitable substitute.

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