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Kirjapaino Sisä-Suomi

To Paula and Vappu,
still my favorite mentors
in spreading nets for whitefish

ABSTRACT

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The present work is concerned with the paradigm theory of Thomas Kuhn, here given our own interpretation, in the course of which we consider the structure and nature of empirical theories and the development of science, taking also a stand on the question how growth and progress take place in empirical science.

To this end we first examine the traditional conception of science (e.g. the Received View, Popper, Lakatos), and proceed to an *exposition* of Kuhn's theory. Thereafter attention turns to the criticism Kuhn's theory has encountered. Against the background thus established we then form an *interpretation* of his theory. Among central elements in this construal are, on the one hand, an analysis of normal science and on the other the paradox prevailing between the theses of incompatibility and incommensurability. A vital factor in normal science as Kuhn conceives it is the actualization of paradigms, a circumstance almost without exception overlooked in the literature. Normal science turns out to be for a large part basic research and by nature something entirely other than what for example Popper and Feyerabend envisage. It emerges from our inquiries that in a certain sense the normal-scientific actualization process occupies a central position also with respect to scientific revolutions. At the same time the paradox between the theses of incompatibility and incommensurability is resolved on the one hand in association with the pragmatic criticality of the actualization process, on the other by merit of the values inherent in scientific activity. In the two latter chapters of the work the anti-Kuhnian criticism is shown to be in large measure erroneous.

Keywords: accumulation of knowledge, actualization of a paradigm, actualization theory, frozen theory, development of science, empirical theory, calculator, growth of knowledge, incommensurability, incompatibility, Kuhn, normal science, paradigm, paradigmatic theory, progress of science, reduction, scientific revolution.

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I have crossed lances with Associate Professor Esa Itkonen in many a colloquium during the past ten years; during the last two he has displayed unflagging interest in my researches. On the occasion of a symposium in spring 1985 he subjected my work to detailed comment and extensive criticism. Although there were many points on which our views could in no wise converge, the impulses I received from him in the course of our discussion made me realize how I might bring my undertaking on the right track from the standpoint of a dissertation. The fairness and the inestimable contribution which Associate Professor Itkonen's interest and penetrating criticism have constituted for the progress of my work have earned my profoundest gratitude and unqualified respect.

This study has evolved in a number of versions and contexts, for instance in numerous seminars, colloquiums and congresses over the years 1979-1985, since its central ideas were first presented on one and the same forum in my unpublished Licentiate thesis of 1979. On all these occasions I have received comments and helpful criticism from many persons, all of whom I thank, especially Professor Raili Kauppi, Professor Sven Krohn, Professor Ilkka Niiniluoto and every member of the Coordinative Research Group for the Science of Science of the Academy of Finland. I take this opportunity of expressing my gratitude to Professor G. H. von Wright for his comments, at an earlier stage of my undertaking, on my plans to go on with my study. I also warmly thank Professor Tapio Nummenmaa for lively - and sometimes heated - discussions we over the years have had on the subject matter of the nature of empirical research. While voicing my thanks to Dr. Tapio Varis, now Rector of the University for Peace (Costa Rica), I am looking forward in the hope that our intensive conversations which we used to have on the open body of Pälkäne Waters, can in the future have fresh iterations. My unreserved thanks are due to Ph. Lic. Tapani Turkka, of the tramping coffee society, for his sharp insights and lasting interest in my undertaking.

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Robert MacGilleon is responsible for the English version of the work. I revised the preliminary draft, and the semi-final version, thus accomplished, was carefully checked by him, producing the final text. It was my priviledge to cooperate with such a fine and determined man as Robert MacGilleon. I express my deepest thanks to him.

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In spite of the efforts of my mentors many things in my work did not reach the level the mentors had a right to mark. For this, and for any fault in my study, I am alone responsible.

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INTRODUCTION

1. The present work comprises a study of the theory of Thomas Kuhn, to which we give our own interpretation. Within the framework of our construal we examine the structure and nature of scientific theories and the development of science, also connsidering the question how growth and progress take place in empirical science.

To this end we shall proceed as follows. First, in Chapter I, we consider the conception of the structure and nature of scientific theories which we term traditional. We elucidate the conceptions of the development of science held in the various branches of the traditional view of science - for example the Received View on Scientific Theories - and trace how incompatible positions have emerged within this tradition. Chapter I represents the introductory part of our work, in which we give systematic form to the background against which we feel the theory of Kuhn can best be understood. (It follows from the function of Chapter I that its lines of demarcation lie where they do.)

A wide of views on Kuhn's theory are expressed in the very extensive literature it has catalysed. Following the account of the traditional view of science in Chapter I is an exposition of Kuhn's theory, a kind of standard version of it, in Chapter II. In Chapter III we turn our attention to some of the severe criticism which the Kuhnian paradigm view has called forth.

The above-mentioned elements - the traditional view of science, the exposition of Kuhn's theory and the criticism levelled at the paradigm view - constitute the sounding-board against which, in Chapter IV, we shall put forward our own interpretation of Kuhn's theory. One particularly vital component in this account of Kuhn's thought is an analysis of normal science. Normal science comprises the actualization of a paradigm, manifested for example in the emergence of new theories within the sphere of that paradigm and the more precise formulation of earlier theories. The concept of actualization is, or can be reconstructed as, the flagship of the paradigm view in what comes to the core of normal science, and surprisingly enough, of the incommensurability of theories. By implication, then, the process of actualization is constitutive not only of normal science, but of scientific revolutions, too. This being the case, it is somewhat odd to find that the literature on Kuhn entirely overlooks the matter (at least in Kuhn's original sense) or mentions it as it were only in passing.

Of importance in the development of science is the incompatibility and the incommensurability of successive paradigms, the result of which are breaks in the progress of science - irremediable ruptures, if Kuhn is right. However, the thesis of the incompatibility and *simultaneously* the incommensurability of successive paradigms brings about a paradoxical situation. If successive theories are incompatible, they must be comparable: how, then, can they be incommensurable? If, again, theories are incommensurable, how can they be so amenable to comparison as to be recognizable as incompatible! The search for a solution to this paradox forms part of this present undertaking.

For the purposes of examining the breakthrough points in the development of science and the incompatibility and the incommensurability of theories we introduce the concepts of the frozen theory and the calculator in contradistinction to what we mean by a genuine theory, the basic factor in scientific progress. From this basis we arrive at a conception whereby scientific breakthroughs and the incompatibility and incommensurability of theories, including the incompatibility and incommensurability of the ontologies of successive paradigms, are part and parcel of the progress of science.

In the latter parts of our study, in Chapters V and VI (and partly in Chapter IV), we analyse the criticism levelled at the paradigm view, thereby at the same time articulating our own construal of that view.

With regard to scientific revolutions one central claim in the anti-Kuhnian critique is to the effect that the representatives of two rival paradigms can attain to no degree of mutual understanding, a circumstance which would inevitably entail anarchy in the world of science. In this connection we examine, among other things, the matter of scientific criticality, which in the perspective of the concept of actualization acquires a content diverging for example from the Popperian principle of criticality. In its disputative mode the "critical approach" is apt to lead - and in some cases in the course of history has led - to phases of stagnation in which there is no room for that pragmatic appraisal inherent in the process of actualization and indispensable to scientific progress. We arrive at a conception whereby paradigms are comparable not only "critically" - when the progress of science may be jeopardized - but also critically and promotive of scientific advance, one prerequisite here being, however, that a certain scientific value system be preserved throughout the history of science. And yet, in spite of the above-said, the incommensurability of paradigms, which is also associated with the pragmatics of science, remains an ineliminable factor, as we seek to show in our work.

One of the claims at the core of criticism of the paradigm view is that normal science and routine are one and the same thing and that normal science is a danger to our whole scientific civilization. Here is included the assertion of Feyerabend that if we take seriously the battery of criteria with which normal science operates,the most peculiar modes of enterprise - for example the activities of an organized band of safebreakers - must fall within the sphere of scientific practise. This *Dillinger* dilemma we unravel in our work. Normal science is not to be identified with routine, nor is the normal scientist a pitiable slave of dogma as Kuhn's learned critics would construe him to imply. Normal science proves to be in large measure precisely basic research, and representatives of it in one particular normal-scientific tradition have included men like Euler, Lagrange, Laplace and Gauss, none of whom to our way of thinking succeeded in fulfilling Popper's expectations of the normal scientist - "badly taught" or "a victim of indoctrination" - and whose work in normal-scientific research, if we have understood the matter rightly, was not what Popper claims normal science always to be, "a danger to science and, indeed, to our civilization".

2. In the body of our work to follow we assume a good knowledge of the basic concepts of the philosophy of science*. With regard to the system of symbols adopted we direct the reader to the key appended.

Throughout our account we consider Kuhn's theory from the immanent point of view**.

We would further point out that in our bibliography the first year of publication of a given reference is in most cases indicated; for instance

Popper, K. R. (1969), *Conjectures and Refutations,* Routledge and Kegan Paul, London. (1963)

should be read to imply that our references are to the edition of year 1969, while the book first appeared in 1963.

CHAPTER I
PHILOSOPHICAL THEORIES OF THE DEVELOPMENT OF SCIENCE AND OF THE NATURE AND STRUCTURE OF EMPIRICAL THEORIES: BACKGROUND AND CONTEXT IN THE PHILOSOPHY OF SCIENCE FOR THE KUHNIAN APPROACH

How does growth take place in the content of empirical science or how does empirical science develop and progress? What is the nature and structure of empirical scientific theories? These two problem areas should not - and indeed at bottom cannot - be held separate, for theories are vehicles of scientific knowledge: whatever particular conception of theories and scientific knowledge we adopt, that very conception will influence our ideas of the mode by which science grows and develops, and conversely, results arrived at in studies of the growth of knowledge and the development of science may alter our views of the nature and structure of empirical theories.

This chapter is concerned with the possibility of a solution to the problem of characterizing empirical theories along what we speak of as *traditional* lines. An account will also be given of the way the different branches of this traditional view of science lead to specific views and conceptions of the growth of knowledge and of the development of science.

The considerations pursued in this chapter are not concerned so much with the *actual* history of the philosophy of science as with a *logical* reconstruction of it (or better: part of it). Our strategy here will be roughly this: the point of departure is a conception V_1 which intuitively seems natural and innocuous but which is subsequently found to be untenable. The sensible reaction to this discovery is to form alternative views V_2 and V_3, which, however, likewise prove dead ends; a way out may then be sought in terms of a philosophy of science V_4 and so on. In this way, by constructing or characterizing logical and rational alternatives - i.e. certain "possible" metatheories of the empirical sciences - for solving the basic problems in the philosophy of science, we arrive at a kind of network or "derivation tree" of the different positions of the traditional view: each node in the tree denotes a philosophical theory (e.g. the positivistic one) and a move, say from V_m to V_n, is a reaction against the shortcomings of V_m and at the same time an attempt to construct or accept a new theoretical trial V_n. In this network of trial and error (i.e. different approaches in the philosophy of science) of the traditional view two specific nodes, namely that of positivism on the one hand and that of Popperism on the other, play a role crucial from the viewpoint of the present work: we shall in subsequent chapters confront the Kuhnian approach especially with that of positivism and of Popperism.

Be it noted that since our account in this chapter is more of a characterization of certain rational and logical alternatives than a factual history of the philosophy of science, the term 'classical empiricism', for example, must not be attributed the strict connotations it carries in the history of philosophy. We would also emphasize that the title of this chapter does *not* signify an inquiry into which thinkers have foreshadowed the Kuhnian approach or ideas. This task, interesting as it is, is not the concern of this chapter or indeed of our undertaking as a whole.

§ 1. JUSTIFICATIONIST THEORIES

1. The distinction *justificationism* - *falsificationism*[1] is an important watershed in the philosophy of science as regards the appraisal of theories. The ideal of the various forms of *justificationism* is a permanent truth-distribution to theories (the truth-distributions may be (intended to be) absolute or related to the evidence given), the most powerful requirement being that of the provability of empirical theories, which requirement then is weakened in different ways in the more moderate forms of the approach. According to *falsificationism,* again, the situation is roughly this: scientific hypotheses are not justified, in the sense intended by the justificationists, on the basis of experience (let alone in any other way). - We may proceed now to a consideration of some of the main versions of justificationism.

2. When do we say that a given (empirical) hypothesis or theory is scientific? An empirical claim or hypothesis is frequently countered with the demand: *prove it!* A criterion for the scientific status of a theory or hypothesis and one answer to our basic problem could thus be - and for many has been and is - the following: an empirical theory is scientific precisely when its right to a place in the ranks of scientific knowledge can be settled by means of *proof.* If the conception of the nature of empirical science so formulated were correct, many of the problems of the philosophy of science and investigation of the mode by which knowledge grows would be easy to solve. If namely scientific empirical knowledge is accessible to proof, then at the same time scientific knowledge is among other things (J1) objective, intersubjective and non-relative (since by means of proof all factors subjective and offensive to objectivity can be eliminated and one single correct result reached); (J2) cumulative in its mode of growth (since a *proved* theory need not be relinquished on account of subsequent accretions of knowledge); and (J3) progressive (since no matter how loose a meaning is attributed to the term 'progress' in this context, surely no one would wish to deny this predicate to permanent and cumulative knowledge). The fact that in the course of the

development of science many theories have had to be abandoned would be explained simply by error: the rejected theories were not true knowledge but assumptions from the outset. Such a conception is from here on referred to as *naive justificationism*; other justificationist doctrines defined in the following are classical intellectualism, classical empiricism, dogmatic falsificationism and the positivistic view of science (cf. Lakatos (1970), pp. 91-103 and Brown (1977a), pp. 541-542).

The unqualified naive-justificationist conception of the provability of empirical knowledge is, however, apart from being precisely naive, also untenable: where are the ultimate starting-points of knowledge to be found and how are they to be "proved"? The answer to this question constitutes the dividing line between the two branches of justificationism - classical intellectualism and classical empiricism (see Lakatos (1970), pp. 94-95).

3. According to *classical intellectualism* the ultimate universal points of departure for knowledge lie in revelation, intellectual intuition, but also in the "proofs" afforded by experience (see Lakatos (1970), p. 94). Central for this approach in the philosophy of science are the *a priori* starting-points of science. When these "true" basic points of departure are subjected to the methods of proof (in the usual sense of the word), all knowledge can be justified in that for example the above stipulations (J1) - (J3) are fulfilled. The resulting account of the development of science is typically a theory of accumulating truths.

Classical intellectualism finds itself nevertheless in difficulties when it must meet the epistemological challenges offered for example by Einstein's physics: how is it possible that two mutually incompatible theories (those of Newton and Einstein) have in the course of the history of science both been regarded as true? One solution would be to say simply that Newton's theory was not true but based on an "error" (see above). If, however, nothing beyond this can be said, the solution remains from the standpoint of the philosophy of science too indiscriminate and leaves unanswered questions too important to overlook: how, for example, is the (actual) success of modern science, which *was* based on Newton's theory for hundreds of years, to be "explained"?

Particular difficulty is raised by the question of the nature of the *a priori*. Are not the *a priori* elements of our knowledge, after all, permanent and "common to all"? This would imply that the universal "axiomatic" points of departure aspired to in classical intellectualism might be mutually inconsistent, so that choice between them becomes problematic in a way that leads to a *cul de sac*. Classical intellectualism is not, then, a tenable approach.

4. According to *classical empiricism* the point of departure is a small number of factual propositions whose truth value can be definitely clinched on the basis of experience. This set of propositions - let the set be denoted by EB - we call the *empirical basis* (*in the sense of classical empiricism*), in terms of which all other statements of science are to be justified, producing as an outcome permanent knowledge. Here we have a typical theory of the accumulation of knowledge.

How does justification proceed? Can scientific laws (and theories) be shown to be true on the basis of the set EB? In the traditional view genuine scientific knowledge comprises for the most part universal laws and theories. Let us consider the law-statement

(1.1) $(\forall x)A(x,\ldots)$

and let us suppose $A(x,\ldots)$ not to contain theoretical terms. How is the claim (1.1) to be proved? *To begin with* it must be noted that the truth of (1.1) is not amenable to the approach "If assuming the truth of the statements in EB we can show of an arbitrarily fixed a_o that $A(a_o,\ldots)$ applies, we may conclude the validity of $(\forall x)A(x,\ldots)$", since the strategy of the law of universal generalization cannot - of course - be appealed to in an empirical context. *Secondly*, in no sequence of instances[2] supporting (1.1) can one proceed "far enough", that is, to inifinity (since (1.1) was a genuine universal statement), so that (1.1) is not proved by such a sequence even though the members of the sequence, as far as they could be checked, were to prove true on the assumption of the truth of EB. It follows that the law-statement (1.1) cannot be proved on the strength of the statements in EB. Be it further noted that without the assumption that A(x,...) contains no theoretical terms, proof of a law of the form (1.1) would be "even more difficult". All in all, then, we may state that genuine scientific laws are not to be proved (by means of deduction) even if EB is given in the required strict sense.

For a scientific law to be proved true on the basis of the set EB, a logic diverging from the standard logic would be necessary, a logic where singular statements could be pursued through to universal ones without reservations. One would thus need a "logic of induction", however this would be developed. Such a logic in *this* sense, however, is obviously impossible (see § 3, below). It must also be borne in mind that the assumed certainty of EB may with good reason be considered untenable (see § 3, below). Classical empiricism is thus an erroneous approach in the philosophy of science.

5. *Dogmatic falsificationism* is a kind of pessimistic reaction to the difficulties encountered in the above approaches: it holds that all that can be envisaged with regard to *theories* is their disproof[3] and that nothing beyond this can well be said of them. Although this approach is a justificationist doctrine, we shall consider it only later in the context of falsificationism proper. - We may proceed now to a consideration of positivism, the most influential branch of justificationism in our century.

§ 2. THE RECEIVED VIEW ON SCIENTIFIC THEORIES (JUSTIFICATIONISM CONTINUED)

2.1. The Structure and Nature of Theories According to the Received View

1. We now turn to a consideration of the conception of the nature and structure of scientific empirical theories originating largely in the logical empiricism of the Vienna Circle and Reichenbach's Berlin school and subsequently evolved within the framework of analytic philosophy (see e.g. Hempel (1970) and (1971) *passim)* to a considerable degree of subtlety. We shall refer to this famous and influential offshoot of the Vienna and Berlin groups loosely as *logical positivism* or simply *positivism* without regard to the terminological distinctions between its historical stages of development or the various doctrinal differentiations within it.[4] As we are not concerned here with the positivistic doctrine in general but mainly with the positivistic conception of theories, we shall not take up the "old-positivistic" background to this new positivism. With respect to the conception of theories itself we shall likewise confine discussion to certain aspects essential to the present inquiry.

Carl G. Hempel, a prominent figure in the development of the positivistic view of science, refers to the conception of science thus highly articulated in terms of analytic philosophy with the illustrative name *'the "standard conception" of scientific theories'* ((1970), see also (1971)). Another eminent representative of the standard conception, Herbert Feigl, holds that although a wide range of variations and modifications have been put forward within this tradition as to the nature of empirical theories, and although terminological divergences and various developmental trends are to be discerned, these analyses of the logical structure and empirical foundations of theories are at bottom *essentially similar* ((1970), p. 3). Feigl indeed employs of this view of science the appropriate term *'the "orthodox" view of theories'*. In most cases the standard conception is referred to as *the Received View (on Scientific Theories)*, which we shall hereafter abbreviate to RV (on this terminology see Suppe (1974), p. 3).

This view of science, RV, emerged along the lines of the general doctrines of positivism, but has on the other hand had its influence on those more general positivistic views. It is also appropriate to note that RV continued to be developed even after the doctrines with which it had at the outset been closely associated had already been rejected (see for example Suppe (1974) *passim*). As we shall see, RV is a logical and most rational solution to the *cul de sac* of the philosophy of science reached in our foregoing "derivation tree" (see § 1, above). The solutions of RV are pursued on the one hand on strict empiricist principles, on the other under the impact of perspectives afforded by the mathematical logic powerfully developing at the turn of the century.

In the following section we shall outline the RV conception of theories as formulated in (RV1) - (RV5) below, condensed and in the most generalized form possible. Thereafter, in Sections 3-7, we shall briefly consider the variations, implications and courses of development of these conditions and some difficulties into which they have led.

2. A scientific empirical theory T may according to RV, in principle and in the ideal case, be put forward by axiomatic means in a mathematical logic L observing the following conditions (RV1) - (RV5) (see e.g. Carnap (1936), (1937), (1949a), (1949b), (1956), (1966), (1975), Feigl (1956), (1970), Hempel (1970), (1971), Suppe (1972), (1974)):

(RV1) L has at least the power of a first-order predicate logic with equality.

(RV2) Let the vocabulary of the theory T be Voc(T) = DesVoc(T) $\cup$ LogVoc(T), where DesVoc(T) is the *descriptive vocabulary* formed of the descriptive terms in T, and LogVoc(T) is the *logical vocabulary* necessary to the formalism in question. There then exists on DesVoc(T) a partition {ObsVoc(T), TheoVoc(T)}, in which ObsVoc(T) is the *observational vocabulary* formed of the observational terms in T and TheoVoc(T) is the *theoretical vocabulary* formed of its theoretical terms. Where there is no danger of ambiguity we may also use the designations ObsVoc and TheoVoc. The members of ObsVoc(T) refer to directly observable physical objects or to directly observable properties and relations of physical objects.[5]

(RV3) The only descriptive terms in the theoretical postulates (axioms) $A_1,\dots,A_n$, involved, are from TheoVoc(T).

(RV4) The terms of TheoVoc(T) are defined on the basis of those in ObsVoc(T). (These definitions are known as *correspondence rules*.)

(RV5) Let the set of axioms $A_1,\dots,A_n$ be Γ and the set of correspondence rules $C_1,\dots C_m$ be C. Here the set

(2.1) $T = \Gamma \cup C$

is called an *(axiomatized) theory (in the RV sense)*.

Thus RV entails the following view of empirical theories: every empirical scientific theory is - in one way or another - always exhaustively identifiable with a set of statements whose internal logical relations and logical relations to adequate singular statements are (in principle and in the ideal case) amenable to characterization by deductive means.

3. According to (RV2) there is on the descriptive vocabulary DesVoc(T) of a theory T always a partition {ObsVoc(T), TheoVoc(T)}, where the observational vocabulary forms the neutral basis for interpretation of the theoretical terms in TheoVoc(T), each member of this basis having a completely definite and fully understood empirical meaning independent of all theories. The members of the interpretation basis ObsVoc(T) refer to physical objects or properties and relations of these such that each is directly observable[5] "in the sense that, under suitable conditions, a normal human observer is able to ascertain its presence or absence in a particular case by means of immediate observation, without reliance on instruments or inferences" (Hempel (1971), p. 371).

Let T be a theory. We call any language OBS(T), where

$$OBS(T) \subseteq W(ObsVoc(T) \cup LogVoc(T)),$$

an *observational* (*base*) *system* of the theory T, allowing OBS(T) to fulfil the desired specifications as to logical power. One restriction on the observational base system is now formed by observation statements, each asserting the presence or absence of a directly observable attribute in a particular instance. This restriction is the *observational language* or the *evidential basis* of T. The observational language is unproblematic in respect of the evidence in question, since on the basis of what was said above any two normal observers, regardless of their theoretical background, can always reach agreement about the statements of this restriction (*ibid.*, pp. 371-372, Suppe (1974), pp. 48-49).

Further, let T_1, T_2,... be theories and consider the observational system OBS = $\bigcup_i$ OBS(T_i) of these theories, called the *observational comparison basis* or the *observational* (*base*) *system of the theories* T_1, T_2,... . Now OBS is *neutral* with regard to theories, which can thus (neutrally) be compared on the basis of their consequences by various means in the basis OBS. Moreover, the restriction of OBS to observation statements (the observational language or evidential basis of the theories T_1, T_2,...) affords the theories an objective, common and neutral evidential basis which is static in the essential sense that statements need not be deleted from it in the course of time. This idea of neutrality embodied in (RV2), as elaborated above, we call the *(RV) doctrine of the neutrality of the observational language* or the *doctrine of the neutrality of the observational comparison basis*.

Condition (RV2) and the sharp observational-theoretical dichotomy it entails constitute the one thesis of RV which in its half-century of development has remained in principle unaltered[6], and which is considered the most reliable component in the whole approach.

4. The point of departure in the early phase of RV was the requirement that the correspondence rules (see condition (RV4), above) be formulable as explicit definitions as follows. Let F $\in$ TheoVoc(T) be arbitrary. It must now be possible to give for F an explicit definition (correspondence rule)

(2.2) $(\forall x)(F(x) \leftrightarrow O(x))$,

where the only descriptive terms in O(x) are from the vocabulary ObsVoc(T) (see Suppe (1974), pp. 16-17). Let the condition thus formulated in the original version of RV governing the correspondence rules be henceforward designated (RV4)'.

If the RV condition (RV4) can be realized in the form (RV4)', the consequence is that

(i) theoretical terms are merely abbreviations, so that

(ii) theoretical terms can in principle be completely eliminated from science, which in turn implies that

(iii) scientific (empirical) knowledge is in toto observational and concerns only the "surface structure" of reality.

The aim of logical positivism was to purge science of metaphysical entities. In the early phases of RV the condition (RV4)' guaranteed - so it was thought - that in *science* no such entities were necessary (cf. the stipulations (i)-(iii), above). Could not the same be applied to philosophy and all discourse? At that time (RV4) in the form (RV4)' was indeed broadened into a general theory of *cognitive significance*: the only meaningful discourse in natural language took place in a natural language analogous to the language of theories. Thus all statements which could not be dealt with in accordance with (RV2) and (RV4)' were to be eliminated as being metaphysical. These ideas were condensed into the *verification theory of meaning*, whereby all cognitively significant discourse concerning the world must be empirically verifiable, or, as the slogan ran, "The meaning of a term is its method of verification." (On the above see Suppe (1974), pp. 1-15).

5. Problems associated with verification were solved in RV as follows. *First*, the problem of verifying singular (molecular) statements was reduced by means of truth functions to the verification of observation statements in the evidential basis, and verification of those statements was regarded as unproblematic[5] (see (RV2) and Section 5, above). However, *secondly* it was realized that generalizations of the form

(2.3) $(\forall x)(P(x) \rightarrow Q(x))$,

of the observational base system, where $P,Q \in$ ObsVoc, cannot be verified (see § 1, Section 4, above). For this reason - if all scientific laws of the form (2.3) are not to be held meaningless - an inductive logic (confirmation theory) had to be sought, albeit only in the later stages of the development of RV, suitable for treating generalizations. Here, too, however, RV has encountered serious difficulties even where justification is entirely restricted to generalizations of the observational system. Such difficulties arise in the first place in qualitative confirmation theory in the form of the so-called paradoxes of confirmation. (The details of the debate on this subject lie, however, beyond the scope of our present interest). In the case of quantitative confirmation theory, again, the problem how to distribute more than the zero probability for genuine generalizations has given rise to much discussion. This lies likewise outside our present concern; suffice it to say that to the intuition of many - though by no means all - here is a serious hindrance to the RV approach. Apart from this the difficulties of confirmation theory and of RV only increase if we transfer our attention from the observational base system OBS to the actual theoretical problems of science. Carnap himself, one of the most prominent representatives of inductive logic[7], observes on this point:

> "... an application of inductive logic in these cases [for instance Einstein's general theory of relativity; the other steps in the revolutionary transformation of modern physics] is out of question." (Carnap (1971), p. 243)

- thus even in this direction, and even at the level of the OBS language (to say nothing of theoretical science proper) RV finds itself in difficulties.

6. Can correspondence rules be given as explicit definitions in keeping with the early RV condition (RV4)'? As Carnap's paper "Testability and Meaning", written as early as in the mid-thirties, implied, the requirement of explicit definition in the case of (theoretical) dispositional terms (for example 'fragile') leads to

absurd results; and yet such terms are clearly cognitively significant ((1936), pp. 439-441; see also Suppe (1974, pp. 17-18). The demand for explicit definition had thus to be abandoned[8]. After his above-mentioned discovery Carnap proposed that dispositional terms might be defined by means of so-called *reduction sentences* which only *partially* define theoretical terms ((1936), pp. 441-444). The shift to reduction sentences represented a first step in replacing (RV4)' with increasingly flexible conditions, so that the stand eventually adopted on the problem of (theoretical terms and) correspondence rules was roughly this (Suppe (1974), p. 27): the correspondence rules, of which there are a finite number, specify the experimental procedures permissible in applying a theory to observable phenomena, and further, these rules and the postulates of the theory together afford a partial interpretation for theoretical terms in specifying their observational content.

In the case of the correspondence rules, too, RV has failed to find a way out of its difficulties (see e.g. Hempel (1970), (1971), Suppe (1974), pp. 17-26, 66-94, 102-109 *passim*). A brief mention may suffice here of some of the problems involved. In the early days of RV the correspondence rules were regarded as analytic (specifying meanings), but when it was realized that they may have testable consequences, partition of correspondence rules into analytic and synthetic was proposed (see e.g. *ibid.*, p. 29). This, however, meant that some of the correspondence rules (the synthetic ones) could be understood as auxiliary hypotheses and not as part of the given theory (*ibid.*, p. 109). Also the assumption that the correspondence rules are finite in number gives rise to a dilemma in that

> "[T]he variety of legitimate possible ways of applying a theory to phenomena is potentially unlimited, and so the potential number of correspondence rules is unlimited. As the set C of correspondence rules is supposed to be finite, there is no guarantee that all of the legitimate ways of applying the theory to phenomena can ever be specified in any set of correspondence rules." (*Ibid.*, p. 104).

The RV construal also produces the anti-intuitive result that whenever a new experimental procedure is devised for the application of a theory $T = \Gamma \cup C$, one is in fact dealing with a *new* theory

$T' = \Gamma \cup C'$, where $C'(\supseteq C)$ contains the appropriate new correspondence rule (*ibid.*, p. 103; see also § 2.2, below).

7. Let the descriptive vocabulary DesVoc of a given theory be ObsVoc $\cup$ TheoVoc. *What do the terms of* DesVoc *refer to?* As far as the vocabulary ObsVoc is concerned the matter is clear (provided that RV is "true"). The question of interpreting the terms of TheoVoc, however, divides RV into two subsidiary branches: *realism* and *instrumentalism* (a dichotomy which is not of course restricted to RV).

Realism (*in the sense of the interpretation of theories*) involves the view that the theoretical terms of a given theory may in principle refer to actual - if not necessarily observable - entities of reality (of which one may thus in principle obtain true knowledge), so that for any theory $T = \Gamma \cup C'$ the following conditions are claimed to hold (see e.g. Suppe (1974), pp. 27-29):

(i) A given theoretical term (for example 'electron') has in principle a referent (electron) in the deep-structure of reality.

(ii) The objective of the postulates belonging to the set Γ of the theory T is to give a truthful account of the behaviour of entities in the deep-structure of reality; in other words the axioms of Γ should in principle be empirically true generalizations as to how the non-observable referents of the terms in TheoVoc behave in the deep-structure of reality.

(iii) Thus interpreted, part of the correspondence rules of C are (in principle) true or untrue statements characterizing the way in which the entities of the deep-structure are manifested in the surface structure of reality. Part, again, of the rules specify meanings; these rules are analytic (not, that is, factually true or untrue).

(iv) The elements in the sets Γ and C together specify the partial interpretation of the terms of TheoVoc. In accordance with the strict distinction in RV between analytic and synthetic, each statement of $\Gamma \cup C$ either specifies meanings (being therefore analytic and not factually true or untrue) or makes claims concerning reality which are either true or untrue.

(v) The consequences of T in the language OBS manifest precisely what T claims takes place in the surface structure as a reflection of the deep-structure. (The truth of these consequences is a necessary but not sufficient condition for the truth of T).

Instrumentalism (in the sense of the interpretation of theories) involves the view that theoretical terms do not refer to entities actually existing, so that for any theory $T = \Gamma \cup C$ the following conditions are claimed to hold (see e.g. Niiniluoto and Tuomela (1973), pp. 141-144, Suppe (1974), pp. 29-30):

(i) Those statements which contain theoretical terms are not factually true or untrue - and correspondingly the statements of T are not true or untrue.

(ii) The theory T is nothing but a set of rules (a grammar) for the systematization of observations and empirical regularities, and for the generation of statements in the observational language.

(iii) In keeping with (ii), above, the problem is now not the truth of the claims made in T but (a) whether T generates empirically true and only true statements in the observational base system *(the problem of adequacy)* and (b) how efficiently T generates these statements *(the problem of economy)*.

Instrumentalism gives rise to the following problem: If theoretical terms and the theory T are necessary only for the generation of certain statements in the observational system OBS, why is the set of these statements not defined directly without the vocabulary TheoVoc and the theory T? Without going into the problematics of the elimination of theoretical terms we may briefly note the following points. To begin with, the elimination of theoretical terms *by the method evolved by Craig* has often been considered inadequate to its purpose in that the theory T must in any case be assumed at the outset to be given definitely well formed, so that in fact one must refer to theoretical terms (see *ibid.*, p. 32). Secondly we may glance at the elimination of theoretical terms in accordance with Ramsey's idea (see *ibid*, pp. 32-33). Let $T = \Gamma \cup C$ be a theory with the theoretical vocabulary TheoVoc = $\{F_1,\dots,F_k\}$, and let $f_1,\dots,f_k$ be predicate variables diverging from one another. Here *the Ramsey-sentence corresponding to the theory T*

$$T^R = (\exists f_1)\ldots(\exists f_k)S^{F_1\ldots F_k}_{f_1\ldots f_k}\ \mathrm{CON}(\Gamma \cup C)|$$

is equivalent to T in the sense that the consequences of T and T^R in the observational base system OBS are identical (*ibid.*, p. 33); furthermore, T^R really does not contain theoretical terms. Ramsey assumed the nature of theories to be instrumental (see *ibid.*, p. 33, note 65), but as has frequently been pointed out, Ramsey's idea does not lend support to instrumentalism (see e.g. *ibid.*, p. 33, Tuomela (1973), p. 67). On the contrary, Ramsey's method is well in keeping precisely with realism, especially if we resort to *Quine's criterion*:

> "The bound variables of a theory range over *all the entities* of which the *theory treats*." (Quine (1947), p. 75; italics added).

Many of the (subtle) lines of thought, theoretical elaborations and constructions associated with the development of RV must be passed over here. Let us now turn to a consideration of the theory of the *development of science* which the standard conception implies.

2.2. The Received View Construal of the Development of Science

1. The *early phase* of logical positivism and RV involved (at least implicitly) the following view of *language-learning* and of the *development of science* (see Suppe (1974), p. 15):

(i) Language-learning takes place in two stages. First, observational terms are learned by means of ostensive definitions. Second, the higher levels of language are introduced by explicit definitions. The higher levels of language are thus in principle eliminable.

(ii) The development of science takes place likewise in two phases. First, the evolvement (and progress) of a theory concerning some particular sphere of phenomena is effected by an accumulation of facts and the positing of empirical generalizations. All this can be exhaustively formulated in the observational base system. Later, theoretical terms are introduced by means of explicit definitions, and theoretical laws are formulated by applying theoretical terms.

Science, then, proceeds from particular facts via empirical generalizations towards theoretical laws which, in keeping with (RV4)', are in principle eliminable and can be reduced to the observational system. Also in respect of the development of scientific theories using theoretical terms, then, the growth of knowledge is cumulative, because the observation statements and observational generalizations couched in the observational base system are (unless error has entered the process) permanent.

However, the changes which took place in RV made it necessary to abandon such oversimplified views of the growth of knowledge (and of language-learning). We now set out the highly articulated and subtle theory of the accumulation of knowledge which can be constructed on the basis of the subsequent development of the positivistic view. Thematically we shall proceed by first forming what we shall call the *naive accumulation theory* and then, to overcome the difficulties this encounters, what we shall call the *positivistic accumulation theory*. Be it noted that the naive version of the theory of the development of science by accumulation is *not* bound up with the early RV conception of theories or with the specific (Baconian) view of the accumulation of knowledge which early RV entailed. The naive accumulation theory, in addition to covering the early phase of RV, is also suited for the characterization of the implications of other above-mentioned justificationist theories (with the exception of dogmatic falsificationism) in respect of the development of science.

2. According to the widely accepted intuitive conception of the development of science, scientific knowledge is cumulative. This intuitive view we shall call the *theory of the development of science by accumulation* or *the accumulation theory* and now proceed to give of its versions the above-mentioned naive and positivistic.

The results of scientific research can be classified, of course, in different ways. Suppose we have a classification which tells us that the achievements are of n different kinds, in which case the scientific results (at a given time-point) constitute an n-tuple VOL = $[R_1,\ldots,R_n]$. (The sets $R_1,\ldots,R_n$ could be, for instance, in respective order the set of facts, the set of methods, the set of observational laws and the set of theories; this classification would then give us a quadruple.)

Let the ordered n-tuples constituted by the results of scientific research and suitably indexed over time form a sequence

(2.4) $\ldots, VOL_i, VOL_{i+1}, \ldots,$

where every scientific result attained and accepted at time-point j is taken to have its place in every ordered n-tuple VOL_{j+k}, when $k \geq 0$, regardless of changes in content possibly taking place in science over the period of time [j, j+k]. The n-tuples in (2.4) by definition fulfil trivially the accumulation condition

(2.5) $\ldots \sqsubseteq VOL_i \sqsubseteq VOL_{i+1} \sqsubseteq \ldots,$

which in the study of the quantitative growth of science has been brought to a high degree of precision (see e.g. Price (1978) and Rescher (1978)). According to the *naive version of the accumulation theory* of science the accumulation condition (2.5) provides the proper condition for the cumulative growth of science

(2.6) $\ldots \sqsubseteq \overline{VOL}_i \sqsubseteq \overline{VOL}_{i+1} \sqsubseteq \ldots$

by removing from the ordered n-tuples in (2.5) various flaws caused for example by simple error (even in mathematics, after all, erroneous proofs are put forward), by the repressive influence of factors external to science (for example the religious and political establishments) or, say, by the underdeveloped state of observation techniques. According to the naive version (2.6), then, the results of scientific research - provided scientific method is properly applied - are permanent, and progress in science can therefore be seen as an accumulation of truths.

This naive version, however, is - apart from being naive - also erroneous in that it fails to take account of the following phenomenon constantly observed in the course of the development of science: an originally highly confirmed theory T eventually begins to produce results which - while it cannot be shown that scientific method has been misused - are clearly untrue. Must the theory T then be abandoned outright? Is the accumulation theory then indefensible? By way of tackling this dilemma we shall in the following section put forward the positivistic theory of the accumulation of knowledge based on RV. (For our construction here consult Nagel (1949), (1968), pp. 336-365, Kemeny and Oppenheim (1956), Feyerabend (1962), Suppe (1974), pp. 53-56: cf. also Kuhn (1970), pp. 99-103).

3. As will have emerged from the foregoing, RV holds that the observational base system OBS firstly contains the stable evidential basis for the theory T at hand and, secondly, affords a neutral comparison basis for the various given theories T_1, T_2,... (pertaining to the same sphere of phenomena), by means of which these can with different methods, be compared as to their consequences.

Let a theory T now, on the basis of its consequences, be highly confirmed for an area Ω. As the evidential basis is stable (in the sense described), it is scarcely possible - so long as the method of science is appropriately and correctly applied, and for example the observation technique is sufficiently advanced - for the theory T to be subsequently disconfirmed for the area Ω (having once been highly confirmed for this very area). Manifestly, then, later disconfirmations of T can only mean that an attempt has been made to extend its scope to a new area Ω' where it is no longer applicable. The next step is then to devise a theory T' which can be confirmed for the area Ω' (and *not* to reject T for - as already stated - since the evidential basis is stable the confirmation of T for Ω is permanent).

How then does the expansion of knowledge take place outside areas already conquered? According to the positivistic accumulation theory the chief factors in the cumulative development of *theories* are these: (a) the proposal of new tentative theories, the testing of these and finally the introduction of those among them which stand the test as *initial* (confirmed) theories into the body of scientific knowledge; (b) the extension of theories; and (c) theory reduction. How do extension and reduction proceed?

Let T_1 and T_2 be confirmed theories. We then say that

(i) the theory T_2 is an *extension* of T_1 if $\mathrm{DesVoc}(T_1) \subseteq \mathrm{DesVoc}(T_2)$ and if T_2 is obtained from T_1 by adding to the latter new correspondence rules and/or new postulates;

(ii) the theory T_1 is *reduced* to T_2 if $\mathrm{TheoVoc}(T_1) - \mathrm{TheoVoc}(T_2) \neq \Phi$ and if a set of additional assumptions ξ exists whereby T_1 (or a good approximation to it) can be deductively derived from T_2 (in which case T_2 explains (or approximately explains) T_1);

(iii) the theory T_1 is *weakly reduced* to T_2 if $\mathrm{TheoVoc}(T_1) - \mathrm{TheoVoc}(T_2) \neq \Phi$ and T_2 (possibly under a few limiting conditions and perhaps approximately) explains at least all those observational elements which are explained by T_1.

In the process of reduction it must, however, be required that the following conditions hold:

(RED1) The terms of the vocabularies $\mathrm{TheoVoc}(T_1)$ and $\mathrm{TheoVoc}(T_2)$ must have unambiguous meanings.

(RED2) The set ξ must contain assumptions postulating a translation between the vocabularies $\mathrm{TheoVoc}(T_1)$ and $\mathrm{TheoVoc}(T_2)$. (These assumptions are biconditionals).

(RED3) The factual assumptions as to the translation of vocabularies must have adequate evidential support.

(RED4) Requisite for assumptions (RED1)-(RED3) is that the evidential basis be stable and neutral.

In weak reduction, translation in accordance with (RED2) is not required; the relationship between $\mathrm{TheoVoc}(T_1)$ and $\mathrm{TheoVoc}(T_2)$ is in this case (logically) more complicated. Here, too, however, especially assumption (RED4) must obtain.

On the basis of the above the positivistic accumulation theory of knowledge may with respect to the development of *theories* be constructed in the following manner.

Let

(2.7) $T_1^{(1)}, T_1^{(2)}, \ldots, T_1^{(n)}, \ldots$

be a series of (initial) theories in different fields, each highly confirmed for its original area. The cumulative development of scientific theories may now be condensed into the scheme

$$
\begin{array}{ll}
 & T_1^{(1)},\ T_2^{(1)},\ \ldots,\ T_m^{(1)},\ \ldots \\
(2.8) & T_1^{(2)},\ T_2^{(2)},\ \ldots,\ T_n^{(2)},\ \ldots \\
 & \vdots \\
 & T_1^{(r)},\ T_2^{(r)},\ \ldots,\ T_s^{(r)},\ \ldots
\end{array}
$$

where at each value of i and j the following conditions hold:

(a) For $T_{j+1}^{(i)}$ there is always a theory $T_k^{(i)}$, where $k \leq j$, such that $T_{j+1}^{(i)}$ is an extension of $T_k^{(i)}$ *or* $T_k^{(i)}$ is reduced or weakly reduced to $T_{j+1}^{(i)}$.

(b) The confirmation of $T_j^{(i)}$ for its original area remains (under the above assumptions) permanent.

Be it noted that in theory reduction the former theory T, which was reduced to the latter theory T', remains a special, sometimes, approximate, case of T' (possibly under a few auxiliary conditions contained in the set ξ restricting for example the range of parameters and variables) and does not lose its confirmation for the original area.

In the view of Kemeny and Oppenheim *progress* (and development) in science is this:

> "*Scientific progress* may broadly be divided into two types: (1) an increase in factual knowledge, by the addition to the total amount of scientific *observations;* (2) an improvement in the body of theories, which is designed to explain the known facts and to predict the outcome of future observations. An especially important case of the second type is the replacement of an accepted theory ... by a new theory ... which is in some sense superior to it. *Reduction* is an improvement in this sense." ((1956), pp. 6–7; italics added).

Condensing this division slightly, singling out from the content of science as a type of their own the observational laws and including the methods of science, we may identify the results of scientific activity at time-point t in the sense of (2.4) with the ordered quadruple

(2.9) $VOL_t = [FACT_t, METH_t, LAW_t, THEO_t]$,

where the members of the sets $FACT_t$, $METH_t$, LAW_t and $THEO_t$ are in corresponding order facts, methods, observational laws and theories. Removing possible errors (in the conventional sense; see above) from VOL_t we get the ordered quadruples $\overline{VOL}_t$. When, lastly, the doctrine of the neutrality of the observational comparison base is applied to the laws and theories of *these* ordered quadruples, together with the methods of (permanent) confirmation (RV represented justificationism!), extension and reduction, we may reconstruct the ordered quadruples

(2.10) $\overline{\overline{VOL}}_t = [\bigcup_{i\in\alpha_t} \{f_i\}, \bigcup_{i\in\beta_t} \{m_i\}, \bigcup_{i\in\gamma_t} \{L_i\}, \bigcup_{i\in\delta_t} \bigcup_{j\in\varepsilon_t} \{T_j^{(i)}\}]$

where f_i, m_i, L_i and $T_i^{(j)}$ refer in corresponding order to facts, methods, laws and theories, and where α_t, β_t, γ_t, δ_t, and ε_t are index sets and where theories $T_i^{(j)}$ fulfil the conditions of (2.8). The *positivistic accumulation theory* may thus be expressed in the form

(2.11) $\ldots \sqsubseteq \overline{\overline{VOL}}_{t_i} \sqsubseteq \overline{\overline{VOL}}_{t_{i+1}} \sqsubseteq \ldots$.

2.3. The Received View at the End of its Tether

1. As we have seen, the positivistic conception of science treats empirical theories as static, *completed* constructs whose historical process of development is abstracted away. Inasmuch as the object has been a study of the structure of all possible empirical theories, the assumption has been that matters pertaining to the content and evolution of particular theories may be passed over as irrelevant, and, further, that empirical theories may be appraised as "frozen" linguistic entities. This latter assumption, of course, is indispensable to the application of the type of logical machinery usually employed in RV. The result has been, on the one hand, a neglect of the dynamic aspect of the evolution of empirical theories (see later chapters of the present work) and, on the other, the adoption of a view whereby the history of science[9] is of no

theoretical significance for the philosophy of science. Thus, in the orthodox view of science, it has seldom, if ever, been felt necessary even to posit the problem of the development of science in any serious sense; rather:

> "Insofar as the development of science was considered [by the logical empiricist tradition] *at all*, it tended to be looked upon as a process of ever-increasing accumulation of knowledge, in which previous facts and theories would be *incorporated into* (or *reduced to*) later theories as special cases applicable in limited domains of experience." (Shapere (1966), p. 44; italics added)

A conception of science, moreover, in which it is assumed that empirical theories can be treated as static logico-linguistic entities amenable to analysis in the same manner as sets of sentences in logic (or mathematics), easily leads to formulating problems in the philosophy of science in such a way that many of their central aspects are either insufficiently weighted or even left entirely outside the analysis. As an example we might consider the problem of the eliminability of theoretical terms. The concept of elimination itself is usually thus defined: the elimination of theoretical terms means the replacement of a given theory T containing theoretical terms by a theory T' whose only descriptive terms are observational and whose consequences in the observational base system are exactly the same as those of the theory T. The eliminability problem correspondingly receives the form: Is it possible to eliminate theoretical terms from an arbitrarily given theory in the above sense? When the problem is framed thus, however, there are often a number of tacit assumptions involved, namely:

(i) Scientific theories can be examined, without obscuring aspects essential from the standpoint of the philosophy of science, as static and definitive (completed) entities in the manner implied by the logical methods employed.

(ii) Empirical theories pertaining to the same sphere of phenomena can, without in principle encountering any difficulties, be constructed in the same way as mathematical theories deductively equivalent with regard to a given set of sentences.

(iii) The problem itself is correctly formulated.

Each one of these assumptions, however, can be called in question:

(i)' Scientific theories are not static but dynamically evolving over time.

(ii)' The construction of alternative empirical theories on the analogy of mathematical theories, and in particular of their neutral comparison in respect of deductive capacity, is far from being a matter of course if (and when) the thesis of the neutrality of the observational comparison base is withdrawn.

(iii)' And finally, the problem itself is misleadingly posed *in those cases where* the solutions arrived at fail to answer the question: Is the rise and development of theories and their eventual replacement by new theories in principle possible without theoretical terms? – Results in the field of logic according to which elimination *is* possible (see § 2.1, section 7, above) say no more than this: *if a theory T is given* (NB: given!), it may be replaced by one not containing theoretical terms (see e.g. Suppe (1974), p. 32). *In itself,* however, such a conclusion tells us little (or nothing) about the possibility of eliminating theoretical terms or more generally about the nature of theoretical terms, when we pass from a static view of theories to the dynamic formulation called for by the actual evolution of science.

The more recent trend in the philosophy of science which Shapere refers to as a revolution (or at least a rebellion; see Shapere (1966), p. 41), involving among others Feyerabend, Hanson, Toulmin and Kuhn, is a reaction against the positivistic view of science (see also e.g. *ibid.*, pp. 41-48, Suppe (1974) *passim*).

Of the representatives of this so-called "new philosophy of science" especially Kuhn has attacked specifically the static RV conception of the nature of empirical theories. In order to anticipate Kuhn's views (to be represented later in the present work) we note the following. In RV it was assumed that the observational comparison basis OBS and the evidential basis, one restriction of OBS, of the successive theories $T_1,\ldots,T_n$ are neutral. This makes comparison of the theories and reductions between them possible, thus entailing the positivistic accumulation theory (2.11) of the development of science. Now Kuhn, on the one hand, maintains that by careful analysis of the history of the natural sciences what can be demonstrated is this: the history of science does not obey the scheme of the accumulation theory. On the other hand Kuhn denies the existence of the neutral basis OBS, a claim which eliminates - if Kuhn is right - the very basis of RV, thus entailing the impossibility of the comparability of "paradigmatic" theories in the RV sense. Without that comparability, however, we do not have reductions, as described above, and so are left without the positivistic accumulation theory (2.11).

2. The orthodox view of science has thus run into difficulties in the course of its own internal development. Moreover, its presuppositions have been shown to be, or have been regarded as, untenable. To this, however, it should be noted that K.R. Popper, a well-known opponent of the positivistic accumulation theory of scientific progress (2.11), has, with his "revolution in permanence" theory, been at odds with the positivistic approach from the outset. Popper's theory is often upheld as an alternative to RV, and for example as an anticipation of Kuhn's thought. We shall now turn to a consideration of Popper's theory and falsificationism, one rational attempt to solve the problems of the philosophy of science and a logical and rational alternative to justificationism in our "derivation tree".

The question whether Popper's thought really does constitute a significant and qualitatively superior alternative to RV will be also taken up in what follows.

§ 3. FALSIFICATIONISM: POPPER

> "... I have said many times that an excessive interest in formalization should be opposed and discouraged. He [Bar-Hillel] asks me whether this holds only for the scientist ... or for the methodologist ... . My answer is: for both. I am an enemy of uncalled-for complication and of all scholasticism. I know that this attitude leads me at times to formulate things not as carefully as they might be formulated. All right. I am sorry ..." Karl Popper ((1974b), p. 1044)

In this § 3 we shall examine certain aspects of Popper's philosophy of science which have a bearing on the present undertaking. In many contexts we shall concentrate on his outstanding work of 1934, *Logik der Forschung* (in the list of references the English version, Popper (1968), with regard to whose central themes Popper's views were not essentially *altered* between 1934 and 1974, in spite of certain "additions" and "clarifications" to his theory (on among other subjects the concept of truth, see for example Popper (1968), pp. 273-376, esp. note 1, p. 274, (1974b), pp. 1001-1003).

3.1. Theories and the Deductive Testing of Theories

1. There is a certain measure of vacillation in Popper's use of the *word* 'theory':

> "... *strictly universal statements,* i.e. *theories...*" (Popper (1968), p. 266; italics added)
>
> "Scientific theories *are* universal statements, like all linguistic representations they are *systems of signs or symbols.*" (*Ibid.*, p. 59; italics added)
>
> "I *always* [italics added] held the view that *theories* are *systems* of statements ..." (Popper (1974b), p. 1188, note 83)
>
> "This I believe, is the reason why the form of a rigorous system is aimed at. It is the *form* [italics added] of a so-called *'axiomatic system'*..." (Popper (1968), p. 71)

Popper does not define the term 'theory', but for example the above citations imply that his view is precisely this: every empirical scientific theory is always exhaustively identifiable with a set of statements whose internal logical relationships and logical relationships to adequate singular statements are (or should be) in principle and in the ideal case amenable to characterization by deductive means; of the statements in the theory the strict ones must be universal statements. (Popper's motivation to allow only universal statements to enter into a theory will be explicated in §§ 3.2 and 3.4, below.)

2. The manner in which the deductive *critical testing* of empirical theories proceeds in Popper's methodology may be described in the following concise terms. (We assume here that Popper's ideas of the structure of *scientific explanation* and of *prediction* are given.) Let T_1 be an old accepted theory and T_2 a new tentative theory pertaining to the same sphere of phenomena. The testing of T_2 under the fixed initial conditions I_o takes place as follows (henceforward the letters I - indexed or without indices or as quantified - refer without exception to sets of initial conditions). Consider the sets

$$\Gamma_1 = \{X \mid X \in [D(T_2 \cup I_o) \cap SING - D(I_o)]\} ,$$

$$\Gamma_2 = \Gamma_1 - \{X \mid (\exists I) \{X \in [D(T_1 \cup I) - D(I)]\}\}$$

and

$$\Gamma_3 = \Gamma_1 - \{X \mid (\exists Y)(\exists I)(Y \wedge \sim Y \in D(T_1 \cup I \cup \{X\}))\} ,$$

which trivially fulfil the condition $\Phi \subseteq \Gamma_3 \subseteq \Gamma_2 \subseteq \Gamma_1 \subseteq D(T_2 \cup I_o)$. The set Γ_1 is now compared with the results of experiments and observations. The test has two possible outcomes:

(i) The result is positive, in other words the elements of the set Γ_1 are *verified* (the expression is Popper's own, see (1968). p. 33). This implies that for the moment T_2 has passed its test and there are no grounds for rejecting it. If in addition $\Gamma_2 \neq \Phi$, and most especially if also $\Gamma_3 \neq \Phi$, and the theory T_2 stands up to a number of such stringent tests (with various sets of fixed initial conditions I), then T_2 is *corroborated* (see e.g. *ibid.*, p. 33, Popper (1969), pp. 241-242, (1972), pp. 192-193).

(ii) There is a non-empty subset of Γ_1, call it Γ_4, whose elements are falsified. This also falsifies the theory T_2:

> "... *if* the conclusions have been falsified, *then* their falsification also *falsifies the theory* from which they were logically deduced." (Popper (1968), p. 33; original italics removed, new italics added)

In the following we shall refer to the above procedure simply as the *Popper procedure*.

3.2. Critique of Justificationism

1. In Popper's criticism of justificationism we shall in the following distinguish four themes: the problems involved in being certain of the truth of (factual) singular statements, universal statements and existential statements respectively, and the critique of inductivism.

Before we proceed to a consideration of these themes it must be noted that the concept *'observable'* is an undefined notion in Popper's methodology, one to be

> "... introduced as an undefined term which becomes sufficiently precise in use: as a primitive concept whose use the epistemologist has to learn, much as he has to learn the use of the term 'symbol', or as the physicist has to learn the use of the term 'masspoint'." (Popper (1968), p. 103)

2. Can the truth of factual singular statements describing observable occurrences be verified? Popper points out in a number of contexts that a statement can only be justified by means of other statements (e.g. (1968), pp. 43, 93). He does, it is true, admit that it is to some extent correct to say that factual science is based on sense perceptions, but goes on to remind us that from the standpoint of a theory of knowledge this is not significant (*ibid.*, p. 93). It is true - so Popper thinks - that only observation can afford us knowledge of facts and that we become *aware* of facts by means of observation (*ibid.*, p. 98). "But this awareness, this knowledge of ours, does not justify or establish the truth of a statement" (*ibid.*, p. 98); and this is so no matter how intense our conviction - however sure we may be of the truth of a statement, this does not justify that statement (*ibid.*, p. 46).

But what then is the *connection* whereby the empirical sciences have their basis in perceptual experiences yet which does not allow of the justification of (even) singular statements on the basis of experience? According to Popper the *acceptance* or *rejection* of a statement describing a fixed observable phenomenon is *causally* connected with our perceptual experience, this being the *motivation* for our acceptance or rejection of the statement; it cannot, however, *justify* the statement under analysis (*ibid.*, p. 105). The motivation of this decision of Popper's is the following. In his view it is not the task of epistemology to answer the question: "... 'how can I, having had the *experience* S, justify my description of it, and defend it against doubt?'" (*ibid.*, p. 98). On the contrary, epistemology must ask: "[H]ow do we test scientific statements by their deductive consequences? And *what kind* of consequences can we select for this purpose if they in their turn are to be intersubjectively testable?" (*ibid.*, p. 98). This, again, reverts to a methodological decision whereby (also) singular statements must be objective and thus in principle falsifiable. (We shall shortly return to the concepts of objectivity, falsifiability and methodological rule.) With such a reading of Popper's theory we arrive at the (perhaps surprising) result: "observation statements" *must not* be justifiable and imbued with the status of absolute certainty, because such certainty would constitute a breach of the objectivity, the falsifiability and the scientificity required of the statements of science, and hence ultimately a breach of the requirement of fertility imposed upon methodological rules (see *ibid.*, pp. 38, 46–47; we shall shortly take up these questions in greater detail).

3. *Let us suppose* (for the sake of argument and only for a moment) that certain observation statements may be considered true in the binding sense. Can this truth be transferred in a strong deductive sense to strict statements, that is to universal or existential statements where the scope of quantification is not restricted? In the case of universal statements the matter is clear: this is not possible (see § 1, Section 4, above). For existential statements, on the other hand, their truth *is* (by our initial assumption) justifiable on the strength of observation statements provided the appropriate instance is found. However, Popper "defines" the concept of scientificity in such a way that statements carrying an unrestricted existential quantifier are metaphysical (non-scientific) and thus entirely beyond the scope of empirical science.

4. It is customary to attribute the success of modern (natural) science to the *inductive method*. *One* way of defining the *logic of induction* is to say that it is the logical analysis of inductive methods (whatever these may be). Correspondingly we say here that an *inference* is *inductive* if it proceeds from singular statements, describing observations and experimental findings, to universal statements. Since it is not possible by means of "pure logic" to proceed correctly from the finite material, to which empirical research is *always inevitably* restricted, to genuine generalizations (see § 1, Section 4, above), we are faced with the *problem of induction*: are inductive inferences justified, and if so, on what conditions? The matter may also be put thus: how can we demonstrate the truth of universal statements based on experience? By *inductivism* in this context we mean a view whereby inductive inferences are justified (here inductivism may be taken also to accept the establishment of probability in lieu of truth; see below). (On the foregoing see and cf. Popper (1968), pp. 27-28).

The core of Popper's critique of inductivism may be formulated as follows (cf. *ibid.*, pp. 27-30). Let us suppose that a *principle of induction*, let us call it P, were to be found whereby conclusions of the form

$$(3.1)\quad A'(a_1, \ldots) \ldots , A'(a_n, \ldots) \vdash_{ind} (\forall x)A(x, \ldots)$$

were possible[2]. The principle P cannot, however, be purely logical because, if it were, the whole problem of induction would disappear (*ibid.*, p. 28). *Therefore* the principle P is synthetic (so that its negation is not internally inconsistent). How is the validity of P now to be established? Since P is synthetic we must validate it on the basis of experience in more or less the following manner. Let

$$(A_1)\quad A'(a_1^{(1)}, \ldots), \ldots , A_1'(a_{n_1}^{(1)}, \ldots) \vdash_{ind} (\forall x)A_1(x, \ldots)$$
$$\vdots$$
$$(A_m)\quad A_m'(a_1^{(m)}, \ldots), \ldots, A_m'(a_{n_m}^{(m)}, \ldots) \vdash_{ind} (\forall x)A_m(x, \ldots)$$

be "successful" inductive inferences. On the basis of them we consider it possible to conclude

(3.2) $A_1, \ldots , A_m \vdash P$.

However, (3.2) is (of course) not deductively tenable; what is involved is an inductive argument

(3.3) $A_1, \ldots A_m \vdash_{ind} P$,

based in fact on a second order principle of induction $P^{(1)}$, which renders the inference (3.3) possible. But if we ask how $P^{(1)}$ is to be justified we find ourselves drawn into an infinite regress P, $P^{(1)}$, $P^{(2)}$, ... The principle of induction, then, cannot be founded either in logic or in experience.

If the truth requirement is replaced with the requirement of probability and we say that generalizations as to the world of experience arrived at by inductive procedures are not in the strict sense true but attain only a certain degree of probability, we are again faced with a dilemma, for

> "... if a certain degree of probability is to be assigned to statements based on inductive inference, then this will have to be justified by involving a *new* principle of induction, appropriately modified. And this new principle in its turn will have to be justified, and so on." (*Ibid.*, p. 30; italics added).

It must also be pointed out that

> "[n]othing is gained ... if the principle of induction, in its turn, is taken not as 'true' but only as 'probable'. In short, like every other form of inductive logic, the logic of probable inference, or 'probability logic', leads either to an infinite regress, or to the doctrine of *apriorism*." (*Ibid.*, p. 30).

Popper also examines the possibility of taking the principle of induction as a primitive (postulate). Let us consider two alternatives with respect to the falsifiability of P. Let us first assume that P is in principle falsifiable. But then the *first* scientific theory to be actually falsified would at the same time falsify (once for all) the principle P, because the theory in question was justified on the strength of P - this because we assumed for the sake of argument that singular statements used as premisses may be considered certain (*ibid.*, p. 254). Let us then secondly assume that P is *not* falsifiable. But

> "... this would amount to the *misconceived* [italics added] notion of a synthetic statement which is *a priori* valid, *i.e.* an *irrefutable* [italics added] statement about reality." (*Ibid.*, p. 254)

5. The ultimate conclusion is this: according to Popper *not one* statement of empirical science is fully justifiable on the basis of experience.

3.3. The Nature of Methodological Rules[10]

1. Although in Popper's view there is no *certain* empirical knowledge, he is nevertheless of the opinion that such methodological rules can be given within whose framework the progress of science (whatever he means by this) is possible. But what sphere of science do methodological rules belong to, and how can they be justified? According to the earlier positivistic view of science (RV) such rules must absolutely belong *either* to the sphere of logic *or* to empirical science. When Popper rejects both alternatives - as we shall presently see he does - this implies at the same time sharp criticism of the early phase of positivism.

Firstly, according to Popper methodology does not fall within the field of logic for the following reason. The object of methodological rules is to ensure that the statements of science are not protected from falsification, and although logic is needed in the analysis of such a set of rules, one cannot *confine* oneself to logic in this (Popper (1968), p. 54), as is easy to see.

Let us *secondly* consider the view that methodological rules are empirical. This is entailed in the approach known as *naturalism*. As a "paradigmatic" example of what naturalism can lead to we may examine the verifiability thesis of early logical positivism whereby a statement is meaningful if and only if it can in principle be verified. Talk here of "meaningfulness", however, becomes meaningless because the thesis itself is a senseless statement - it cannot be verified. On the basis of such "paradoxes" Popper rejects naturalism, at the same time aptly pointing out that when a representative of naturalism imagines he has discovered a fact he is in reality putting forward a convention (*ibid.*, p. 53). Conventionality is indeed precisely the nature of Popper's *own* methodological rules: rules are for him *proposals* (*conventions, norms, decisions*) (*ibid.*, pp. 53-56).

2. How are methodological rules argued for? Popper's answer is simple and lucid:

> "In establishing these rules we may proceed systematically. First a *supreme rule* is laid down which serves as a kind of *norm* for deciding upon the remaining rules, and which is thus a rule of a higher type. It is the rule which says that the other rules of scientific procedure must be designed in such a way that they do not protect any statement in science against falsification." (Popper (1968), p. 54; italics added)

Conceptions of the suitability of conventions (rules) may vary. Rational discussion of their acceptability is possible only among those who have common objectives - and on the other hand the choice of objectives lies outside rational argumentation (*ibid.*, p. 37). Only one mode of rational argumentation is possible and that, too, is based on value judgement:

> "There is only *one* way, as far as I can see, of arguing *rationally* in support of my proposals. This is to analyse their logical consequences: to point out their *fertility* - their power to elucidate the problems of the *theory of knowledge*.
>
> Thus I freely admit that in arriving at my proposals I have been guided, in the *last analysis*, by *value judgements* and *predilections*." (*Ibid.*, p. 38; apart from first line italics added)

3.4. The Empirical Basis and the Demarcation Criterion

1. The *empirical basis* fulfilling the requirements of Popper's methodology - let us call it EB (EB here not, of course, in the same sense as in § 1, Section 4) - comprises singular statements of a

certain logical form which are called *basic statements*. These describe the results of conceivable observations and experiments: a basic statement is "in brief, a statement of a singular fact" (Popper (1968) p. 43). As we have seen above, Popper holds that no absolutely certain and enduring empirical knowledge exists. Hence even "checked" basic statements ("observation statements") are not absolutely certain. Let us now examine more closely this theme and the problems it involves.

Popper identifies the *objectivity* of scientific statements with their intersubjective testability in the sense of the following methodological norm: only such statements may be introduced into science which are intersubjectively testable (*ibid.*, p. 56). Thereby, however, basic statements must also be objective and testable. This in turn implies that the truth of basic statements *cannot* be reduced to experience (*ibid.*, pp. 46-47) and that science can have no ultimate statements, not even observation statements, which cannot be refuted:

> "[T]here can be no statements [even: basic statements] in science which cannot be tested, and therefore *none* which cannot in principle be refuted by falsifying some of the conclusions which can be deduced from them." (*Ibid.*, p. 47; italics added)

This means that testing can be protracted *ad infinitum*; there is no "natural" end to it. But is not the result then an infinite regress such as Popper envisaged as a binding argument against the principle of induction? From the infinite regress connected with testing - and from the fact that his criticism of inductivism can be invalidated on these grounds - Popper seeks to escape by means of the following methodological rules. Firstly one avoids the difficulties arising from the endless chain of tests by *deciding* to accept a fixed basic statement B, at which point the chain is thus severed (*ibid.*, p. 104). Here, however, a new problem is encountered. A basic statement which one has *decided* to accept is of the nature of *dogma*, and this flies in the face of the objectivity requirement. This difficulty, Popper holds, can be overcome by adding to the last-mentioned methodological rule the following norms. The statements B comprising the closure point of a series of tests must in themselves be easily testable, and where need (disagreement) arises testing must be resumed (*ibid.*, pp. 104-105). Thus the dogmatic nature of basic statements is no more than apparent or at worst temporary.

But in this way one is again drawn into an infinite regress which, Popper proposes, is overcome on the grounds that

> "... this kind of *'infinite regress'* is also innocuous since in our theory there is no question of trying to *prove* [italics added] any statements by means of it." (*Ibid.*, p. 105)

A background to this is the following idea: anyone who is capable of applying a testing technique which is considered pertinent can decide whether a given basic statement can be accepted or not (*ibid.*, p. 99). But again a problem looms: what is a pertinent, *relevant* technique in a given situation? Methodological decisions must thus also include rules which determine the relevance of the technique.

2. The statements in EB thus describe conceivable results of observations and experiments, and in keeping with Popper's requirements they must be so defined as to afford criteria for both falsifiability and falsification and must therefore not be derivable from universal statements without initial conditions and must be such that they can conflict with universal statements (Popper (1968), pp. 100-101). To fulfil these stipulations Popper defines basic statements in the following terms: (i) basic statements must be of the form of singular existential statements (logical requirement; *ibid.*, p. 102); and (ii) the event which a basic statement claims to have taken place according to coordinates k must be observable[11] (material requirement; *ibid.*, pp. 102-103).

It follows from the definition that the set EB is inconsistent (see also *ibid.*, p. 84). In the course of testing or as a result of it, accepted basic statements come to form a consistent subset of EB which we designate Accept(EB). This subset is mutable over time in the essential sense that statements may also have to be removed from it. - Here attention may be drawn to the fact that Popper put forward these ideas in the context of the Vienna Circle, where they assumed a particularly radical aspect for the following reason. The logical positivists had interpreted Wittgenstein's *Tractatus* to mean that all statements are truth functions of elementary statements and that the truth of these latter for their part is beyond all doubt - in other words elementary statements are in principle absolutely verifiable[12]. Popper's views were thus in marked contradiction to the reading of Wittgenstein favoured in Vienna: this one perceives particularly clearly if one condenses the foregoing into Popper's own comment regarding basic statements:

> "[Basic statements] are, from the logical point of view, accepted by an act, by a *free decision*." (Popper (1968), p. 109; italics added)

3. As will have emerged from the above, methodological rules belong neither to the sphere of empirical nor to that of formal science; they are conventions which have a normative function. The *demarcation criterion* for science, the crucial normative convention by which Popper "defines" the line separating scientific empirical theories from metaphysical constructs is simply the following: the criterion for the scientific acceptability of a theory is its falsifiability (see Popper (1968), pp. 40-41). This means that any theory which is not in principle falsifiable is metaphysical[13] (lies outside the scope of empirical science). It follows immediately from this criterion that a statement carrying an unrestricted existential quantifier is metaphysical (non-scientific) even if it might be verifiable. Be it further emphasized that the demarcation criterion itself cannot be justified by either empirical or logical means[14] (see also *ibid.*, pp. 38, 55).

Let T now be a theory and EB the empirical basis. Here those statements in EB with which T is inconsistent are referred to as *potential falsifiers* of T (T *prohibits* these statements). We further say that T *permits* those statements in EB with which it is not in conflict. The demarcation criterion now involves the following methodological rules (*ibid.*, pp. 47, 86): (i) only such a statement can be scientific which can be tested; and (ii) a given theory T is *empirical*, that is, *falsifiable*, if T divides EB unambiguously into two non-empty subsets: the statements T prohibits and the statements T permits[15].

4. In Popper's theory a fixed basic statement may by decision (convention) be rendered (temporarily) unfalsifiable; on the other hand Popper's methodological norms are intended to guarantee that a *universal statement* (a theory) *cannot* be accepted by convention. This distinguishes Popper's philosophy of science from classical conventionalism (to eliminate whose "stratagems" Popper constructs a set of methodological rules of its own[16]).

3.5. The Development of Science According to Popper

> "As I have suggested before, scientific progress is revolutionary. Indeed, its motto could be...: 'Revolution in permanence.'" Karl Popper ((1975), p. 83)

1. According to Popper's demarcation criterion the crucial test of scientificity (empiricality) for a theory is its falsifiability. When a falsifying instance is found (the singular statement in question being accepted) the theory is also to be refuted (without any stratagems[16]):

> "If we accept as true *one* singular statement which, as it were, infringes the prohibition by asserting the existence of a thing (or the occurrence of an event) ruled out by the law, then the law is refuted." (Popper (1968), p. 69; italics added)

Guarantee of the permanence of a given empirical theory and of the cumulative growth of scientific theories on the "stable observation language - confirmation - reduction" principle is not then, according to Popper, at all possible (see also *ibid.*, p. 268). Indeed Popper is known as a critic of the accumulation theory of knowledge: in his view scientific knowledge does not accumulate; progress in science is a "permanent revolution" wherein falsified theories are eliminated and the *better* theory supplants the *worse*.

How is the superiority of one theory over another and thus the progress of science assessed in Popper's view?

Let T_1 and T_2 be rival theories. Here, *first of all*, Popper holds that comparison of the two may be carried out in the empirical basis by means of their deductive consequences or of statements standing in a certain logical relationship to these. Means of comparison are for example the *empirical content* (in other words "how much" the theory prohibits) and the degree to which the theory is *corroborated*[17]. It is however far beyond our present concern to enter into detail on the subject of the appraisal of theories; what essentially interests us here is to bring out the presuppositions Popper makes in this context (see § 3.6, below, where we revert to this point).

Secondly, the progress of science may be assessed on the following criteria. Let the set of problems solved within the framework of the theory T be v(T), let T_1 be an old accepted theory and T_2 a new tentative rival to it. For the transfer T_1/T_2 to constitute *scientific progress* the two theories must according to Popper's normative methodology fulfil the following conditions (Popper (1968), pp. 33, 253, (1969), pp. 241-242, (1975), pp. 82-83; see also § 3.1, Section 3, above):

(3.4) $v(T_1) \subseteq v(T_2)$

(3.5) T_1 and T_2 are mutually inconsistent

Thirdly, although in Popper's opinion scientific knowledge is not by nature cumulative, truth is nevertheless the *regulative principle* and can be "approached" by a series of successive rival theories ... T_i, T_{i+1},...: the theories T_k are more or less good approximations to the truth and "there are criteria of progress towards the truth" ((1969), p. 226). While again it is not our present task to discuss the problems relating to these criteria and measures of *verisimilitude,* we must take account of the presuppositions Popper holds as to the nature of theories and to the comparison of theories (we shall in § 3.6 return to this point).

2. The above-said links Popper's methodology to what might be called the *revolution theory of the growth of knowledge*: the development of science is not cumulative because the better theory *"overthrows"* its inferior predecessor; nevertheless what was said above and the comments to follow seriously problematize the

conception of a gulf between Popper's revolution theory and the (positivistic) accumulation theory.

Let T_1 and T_2, then, be such successive theories that the transition T_1/T_2 constitutes progress in science. Here Popper *firstly* requires that "[T_2] should *overthrow* [T_1]" ((1975), p. 83; italics added), but at the same time *secondly* presupposes that "[T_2] can be compared point by point with [T_1] and that [T_2] *preserves* [T_1] as an approximation" (*ibid.*, 92; italics added). But on this basis and on the basis of Section 1 - especially its clause (3.4) - the development of science *would be* cumulative for example in the sense of the growth of the set of explanations afforded by theories (where earlier explanations may be good approximations to the later) and of the empirical content of theories, so that Popper *does* in this respect proffer an accumulation theory of knowledge[18].

But now, in this very light the (positivistic) thesis of weak theory reductionism (see § 2.2 above) is entailed by Popper's theory, and further: cannot the *preserve* clause also be taken to indicate (in spite of the fact that according to (3.5) T_1 and T_2 conflict) that the (positivistic) idea of theory reductionism could be constructed on the basis of Popper's theory. Such a conception would seem to find support in the following[19]:

> "For a theory which has been well corroborated can only be superseded by one of a higher level of universality; that is, by a theory which is better testable and which, in addition, *contains* the old, well corroborated theory - or at least a good approximation to it.
>
> ...
>
> The methods of testing are invariably based on *deductive inferences from the higher to the lower level*" [italics added]; (Popper (1968), pp. 276-277)
>
> "*1 The 'deductive inferences from the higher to the lower level' are, of course *explanations*...; thus the hypotheses on the higher level are *explanatory* with respect to those on the lower level." (*Ibid.*, p. 277, note *1)

Popper's war-cry 'revolution in permanence' is thus problematic. In the first place he represents a cumulative conception of knowledge in the sense of (3.4) (see above). Secondly, while his falsificationism appears to connect him with the XVIIIth century intuitive revolution theory of knowledge[20] as the formulator of *one* adjustment of it, his *overthrow* - *preserve* clause gives rise not only to a certain tension within his own theory (cf. (3.5), above) but also to the manifestly justified question: how far, if at all, is Popper's 'revolution in permanence' theory ultimately removed from the positivistic accumulation theory of knowledge even as regards the development of *theories* in science? - Certain other aspects of Popper's thought and certain critical comments on his *presuppositions* will be brought up in later chapters of our discussion. In conclusion to this § 3, however, let us remain on the subject of the difficulties and on the subject of presuppositions concerning the comparison of theories inherent in Popper's theory.

3.6. A Number of Comments on Popper's Theory

1. Popper has drawn up a special set of rules to eliminate the offensive "stratagems" of classical conventionalism which were intended to render a *theory* immune from falsification[16]. But how does Popper ensure that by his own statement,

> "I shall say even of some singular statements that they are hypothetical, seeing that conclusions may be derived from them (with the help of a theoretical system) such that the falsification of these conclusions may falsify the *singular statements* in question" ((1968), pp. 75-76; italics added),

he is not protecting theories from the arrow of *modus tollens* by precisely such "conventionalist stratagems"? To clarify this, let T be a theoretical system, I a set of singular statements, and Γ a set of conclusions which "may be derived from" T and I. Now, according to the above citation of Popper, "the falsification of these conclusions" in Γ "may falsify the singular statements in" I. But this procedure leaves T - the theory - untouched. Is the theory T protected by Popper with the aid of "conventionalist stratagems" which he himself wanted to eliminate? (Consult here also Brown (1977b), p. 74.)

2. When, according to Popper, is a theory falsified[21]? The above criterion for the falsification of theories, put forward in Popper (1968) (see § 3.1, Section 3), was *sufficient:*

> "... *if* the conclusions have been falsified, *then* their falsification also *falsifies the theory* from which they were logically deduced." (Popper (1968), p. 33; original italics removed, new added)

Later, however, Popper alters this criterion to *necessary, but not sufficient:*

> "We say that a theory is falsified only if we have accepted basic statements which contradict it ... . This condition is *necessary,* but *not sufficient* ..." (*Ibid.*, p. 86; original italics removed, new added)

This means that the Popper procedure mentioned in the foregoing must be repeatable before its negative outcome leads to theory falsification. Such a requirement is of course necessary and natural: a chance application of *modus tollens* cannot falsify a theory. From page 86 of (1968) onwards the characterization of theory falsification is supplemented thus:

> "We shall take it [the theory] as falsified *only if* [italics added] we discover a *reproducible effect* which refutes the theory. In other words, we only accept the falsification if a low-level empirical hypothesis which describes such an effect is proposed and *corroborated* [italics added]. This kind of hypothesis may be called a *falsifying hypothesis.*" (*Ibid.*, pp. 86-87)

According to this new definition of Popper's, then, the falsification of a theory depends upon the corroboration of a falsifying hypothesis in the following sense:

> "If accepted basic statements contradict a theory, then we take them as providing *sufficient grounds* for its falsification only if they *corroborate* a falsifying hypothesis at the same time." (*Ibid.*, p. 87; italics added)

The term 'corroborated' appears in this passage as if it were a familiar concept. Up to this point in Popper's work, however, corroboration is only mentioned a few times, only once with relevance to the definition. In fact we have at our disposal in this context *only* the following characterization of the notion (see § 3.1, Section 3, above):

> "So long as a theory withstands detailed and severe tests and is not superseded by another theory in the course of scientific progress, we may say that it has *'proved its mettle'* [italics added] or that it is *'corroborated'* by past experience." (*Ibid.*, p. 33)

But as to a falsifying hypothesis, which must itself be falsifiable and which itself, in spite of present corroboration, may eventually be falsified - when *is* such a hypothesis corroborated? Under what conditions does a falsifying hypothesis "overcome" (read: falsify) a theory? Why does the theory (which prior to this falsifying hypothesis was corroborated) not overcome and falsify the falsifying hypothesis (read: theory), which is - which must be - falsifiable? What kind of falsifying hypothesis (theory), which must itself be falsifiable, falsifies a falsifying hypothesis? When, then, is a theory corroborated? When is one theory *better* corroborated than another? Recently Popper's analysis of corroboration has in its entirety been called in question (see e.g. Stegmüller (1975), p. 250). If these attacks are justified we have additional support to our foregoing queries and criticism, the implication being of course that our above-mentioned questions can hardly be answered on the basis of Popper's theory. We prefer in this context, however, not to be drawn into a discussion of these problems of theory appraisal incurred in the matter of corroboration any more than of other problems of appraisals of theories for which as yet no solution has been forthcoming[22]; the aspects already adumbrated may suffice for the purposes of our present work. Let us simply record here Bar-Hillel's particularly desolating view of Popper's corroboration theory:

> "... when does a theory pass a test? When does it pass it with flying colors? When has it *proved its mettle?* When is it *highly*

> *corroborated?* I do not think that *any clear sense* can be assigned these expressions." (Bar-Hillel (1974), p. 341; italics added)

If now it is impossible to attribute any clear meaning to these expressions, the consequences for Popper's entire framework are fatal; if the concept of corroboration is problematic[23], this must also problematize the concept of falsification - as indeed we have seen. This renders Popper's theory problematic in its most essential elements. True, the concept of falsifiability would remain formally acceptable, but even so an odd situation would prevail: we would know when a theory is falsifiable but *not* when it is falsified. This for its part would mean (within Popper's framework) that we could characterize a certain *potential* circumstance (i.e. *whether a theory is falsifiable*) but in no case could we say whether a given actual circumstance *(a theory is rejected)* is a realization of that potential i.e. falsification, or not. This is absurd, because the concepts of falsifiability and falsification are central to Popper's theory: if of any empirical theory (actually) rejected in the course of history we cannot say whether it has been falsified or not, while of such theories we can *in every case* solve the problem of falsifiability, then Popper's methodology does not - in our view - fulfil the requirements of fertility.

We recall that according to Popper the only rational defense of methodological rules is "to point out their *fertility* - their power to *elucidate* the problems of the *theory of knowledge*" ((1968), p. 38; italics added). Since Popper's set of rules leads among other things to the situations described above, these rules would not appear adequate to "elucidate the problems of the theory of knowledge". Thus the only rational touchstone Popper himself provides us with for an evaluation of his methodological rules does not work to the advantage of his own theory.

3. Popper makes the following presuppositions in the context of theory comparison. Firstly, theories are deductive systems of statements. Let T_1 and T_2 be theories which are under comparison in order for it to be settled, which one is, say, more progressive, better corroborated or closer to the truth than the other. Now

Popper, secondly, resorts to the following idea. For T_1 and T_2 it is always possible to find or construct a "roomier" (Popper (1970), p. 56) system, in which T_1 and T_2 (and, of course, the corresponding basic statements) can be represented and neutrally compared with no "distortions" coming from T_1- or T_2-"color". If the comparison fails, the "roomier" system is not blamed but the particular appraisal function. (In this point Kuhn's view strongly diverges from that of Popper; see § 6, Section 5, and Chapter IV of the present work.)

4. We may pass over other aspects of Popper's philosophy[24] as lying beyond our immediate concern and turn now to three philosophies of science "defined" by Lakatos. We consider Lakatos' theory within this chapter in spite of the fact that Lakatos' ideas are influenced by Kuhn. This order of exposition is founded on the recognition that Lakatos' reshaping of falsificationism can be interpreted as a well motivated move in the traditional network. The relation between Kuhn and the falsificationism of Lakatos will be taken up in later parts of the present work.

§ 4. FALSIFICATIONISM: LAKATOS

We pass now to a discussion of dogmatic falsificationism, naive and sophisticated methodological falsificationism and the relationship between the two latter and Popper's theory.

4.1. Dogmatic Falsificationism

1. *Dogmatic falsificationism* (see Lakatos (1970), pp. 95-102) - referred to hereafter as DF - is a branch of *justificationism* which holds that the truth value of factual singular statements in the empirical basis can be conclusively established from experience. Moreover, the truth value of all other statements of science must be susceptible to proof via the empirical basis. It follows that universal statements are scientific because they can in principle be

proved *untrue* on the strength of the empirical basis (when the situation intended in *modus tollens* is construable[3]). The imposed requirement of provability leads to the DF view that the disproving of a theory is in fact the only mode of proof in the case of genuine theories; no truth value other than "untrue" can be applied to them (see also § 1, Section 4, above).

Dogmatic falsificationism thus involves the following stipulations.

(DF1) There is a strict distinction between basic statements and theoretical statements.

(DF2) Facts can prove a statement.

(DF3) The scientificity of theories is demarcated by the following criterion: a theory is scientific only if in principle it can be proved untrue - in other words, if in its terms certain observable states of affairs are not possible.

We shall not consider any further the problematic nature of (DF1) and (DF2) (see above and, for (DF1) in particular, later parts of the present work; see also Lakatos (1970), pp. 98-100); rather we would now point out that according to Lakatos (DF3) is also invalid *even if* (DF1) *and* (DF2) *are assumed to apply,* because "exactly the most admired scientific theories simply fail to forbid any observable state of affairs" (*ibid.*, p. 100). After preliminarily arguing for this claim with the aid of examples (*ibid.*, pp. 100-101), Lakatos concludes that "some scientific theories forbid an event occurring in some specified finite spatiotemporal region ... only on the condition that no other factor has any influence on it" (*ibid.*, p. 101; italics removed). But according to Lakatos this means that a theory combined with initial conditions cannot *as such* be in conflict with a basic statement[25]; the argument *also* involves a *ceteris paribus* clause whereby no factors other than those mentioned affect the test or observation situation (*ibid.*, pp. 101-102; see also, however, p. 101, note 3 and p. 186, note 2); but "in such cases it is always a specific theory *together* with this clause which may be refuted" (*ibid.*, p. 101) Further, however, by alternation of the *ceteris paribus* clause, such a theory can always be rescued from falsification. Thus - Lakatos concludes - some of the best theories (for example Newton's) cannot be falsified on DF criteria because the empirical basis, as DF envisages it, cannot include statements claiming that no factors other than those mentioned can exert an influence in a fixed test situation. Thus for example Newton's theory would, on the criteria of DF itself, be metaphysical (even though (DF1) end (DF2) were to be valid).

2. *Methodological falsificationism,* whose various branches comprise naive methodological falsificationism, sophisticated methodological falsificationism and Popper's theory discussed above, is (as was stressed in connection with Popper) an attempt to evade the dilemma of the philosophy of science into which justificationism (including dogmatic falsificationism) must lead. We shall now take up methodological falsificationism in the Lakatosian context.

4.2. Naive Methodological Falsificationism

1. *Naive methodological falsificationism* (for convenience NMF) results from Lakatos' interpretation of Popper. Its methodological rules belong to five classes (see Lakatos (1970), pp. 103–116; § 3, above, is assumed known):

(NMF1) It must first be determined by methodological decision which test and observation techniques are relevant in the assessment of singular statements. On the basis of this type of decision the limits of acceptability in principle are drawn for singular statements – in other words, the empirical basis is defined.

(NMF2) In the absence of "natural" terminal points in the testing process one is ultimately forced to resolve by methodological decision which given statement, in principle acceptable, is also (provisionally) accepted. In connection with (NMF1) and (NMF2) the falsifiability in principle of a theory is taken as the criterion of demarcation between scientific and metaphysical.

(NMF3) Since a probabilistic theory is not as such falsifiable, certain methodological rules are added whereby such a theory is rendered scientific[26] (open to falsification).

(NMF4) Testing rules are needed for the following reason. Let us suppose that a theory T is tested under initial conditions I and a *ceteris paribus* clause, and that in terms of the accepted basic statements the result of the test is negative. But is this outcome the "fault" of T, I or the *ceteris paribus* clause? In keeping with decision type (NMF4) we proceed as follows. *First,* I is put to serious test and accepted as unproblematic background data (provided the result of the test is not negative). *Second,* the *ceteris paribus* clause is tested in the following manner. We assume that there actually are other factors at work; these are specified and tested. If a number of such assumed factors can be eliminated, the *ceteris paribus* clause may be considered corroborated. (Lakatos does not, however, give any closer indication as to how the statement 'no other factor influences' can in fact be corroborated by assuming a few other factors and falsifying the assumption). If now, *thirdly,* the *ceteris paribus* clause is taken as unproblematic background data, the theory T is falsified[3]. (If it proves impossible to isolate the part of T responsible for the negative test result, one can only say that the conjunction of the statements in T is falsified.)

(NMF5) is Lakatos' *tentative* proposal for an extension of the concept of falsification to entail the falsification of a theory also in a case where it is in conflict with an earlier *theory* which is well corroborated.

We wish to draw attention here to the fact that the difficulties involved in Lakatos' interpretation of Popper (these difficulties will be analysed in a later part of this § 4) are *not* connected with decision types (NMF3) or (NMF5).

2. As Lakatos points out, a test is, according to NMF, simply a struggle between a theory and an experiment whose only interesting outcome is the falsification of the theory ((1970), p. 115). This, however, according to Lakatos - and here he is in fact partly repeating arguments put forward earlier by Kuhn - is not compatible with the testimony of the history of science; negative results have by no means invariably led to the rejection of theories. In practice testing is usually a struggle between a number of factors and involves rival theories and experiments; tests frequently lead to the confirmation of a theory rather than to its falsification; and so on (see *ibid.*, pp. 114-115). By suitable amendation of NMF Lakatos

believes he can bring the methodology and the history of science into harmony. The solution he constructs he calls *sophisticated methodological falsificationism* (hereafter SMF). In our view Lakatos' ideas of SMF and the allied *methodology of scientific research programmes* may be conveyed in the following schematic form (see *ibid.*, pp. 116-138, 154-159, 173-180; cf. § 3, above).

4.3. Sophisticated Methodological Falsificationism

1. Lakatos' idea is that the difficulties associated with NMF can be overcome by passing from the analysis of an individual theory to a series of theories - in fact, that is, to a scientific tradition. But what connects the separate theories of such a series in such a way that we may speak of a tradition? According to Lakatos, for a given (mature) scientific theory T there applies (or better: *must apply*)

(4.1) $T = C \cup B$,

where

(i) C is the *hard core* of the theory T, composed of certain laws;

(ii) B is the *protective belt* of the core C, consisting of auxiliary and observational hypotheses (and possibly a small set of initial conditions); and in this case

(iii) $D(C \cup B \cup I)$ comprises the predictions of T with initial conditions I when the protective belt is B ($B \cap I = \Phi$);

(iv) B "protects" (more correctly, since it is a question of norms: *must protect*) the core C from applications of *modus tollens* in the following sense. Even if T is in conflict with the results of observation, *modus tollens* must nevertheless not be applied to C, only to B. This methodological norm is referred to as *negative heuristics.*

Thus understood, the *continuity* of a given series of theories (for example the Newtonian tradition), which makes it a *scientific research programme,* rests in a core common to the members of the series, so that such a series can better be written in the form

(4.2) $T_1 = C \cup B_1,\ T_2 = C \cup B_2,\ \ldots,$

where according to negative heuristics *modus tollens* cannot be applied to the core, only to the protective belt, which latter changes with time (and can be replaced entirely). What is known as *positive heuristics* is in turn a set of rules in the methodology of scientific research programmes which at a certain stage in a programme contains instructions for the modification of the belt so as to bring theory and reality into better agreement.

2. Of the methodological decision types in NMF, SMF retains (NMF1), (NMF2) and (NMF3). In addition a new type is introduced, crucial from the standpoint of Popper's theory, whereby certain *theories*, so-called *interpretation theories*, upon which 'observations' are frequently based, may be made temporarily immune from falsification (cf. (NMF5), above). (In this respect SMF diverges in a very essential manner from Popper's theory, a "Popperian" sequel to which Lakatos nevertheless believes his SMF to constitute).

Further, Lakatos gives the falsification of a theory (and the acceptance of another) a *historical character* by redefining the concept of theory falsification (and acceptance of alternatives) to mean that in the falsification of an individual theory T in a series of theories (4.2) and its replacement by a rival T', the following principle (allowing the proliferation of theories) applies. The theory T is falsified if and only if there is another theory T' fulfilling the following conditions:

> "(1) T' has excess empirical content over T: that is, it predicts *novel* facts, that is, facts improbable in the light of, or even forbidden, by T; (2) T' explains the previous success of T, that is, all the unrefuted content of T is contained (within the limits of observational error) in the content of T'; and (3) some of the excess content of T' is corroborated." (Lakatos (1970), p. 116)

The result of this is that Lakatos' views on the *progress* of science comprise a certain type of accumulation theory involving at least weak theory reductionism (see § 2.2, above), because, *firstly*, a theory formed later in a research programme (a series of theories) explains everything which its predecessor explained, and *secondly*, according to Lakatos, the objective reason for rejecting an entire research programme is in broad outline this:

> "... such an objective reason is provided by a rival research programme which explains the previous success of its rival and supersedes it by a further display of *heuristic power*." (*Ibid.*, p. 155)

On the strength of the above SMF can dispense with (NMF4), because

> "To show this we only have to realize that if a scientific theory, consisting of some 'laws of nature', initial conditions, auxiliary theories (but without a *ceteris paribus* clause) conflicts with some factual propositions we do not have to decide which ... part to replace. We may try to replace *any* part and only when we have hit on an explanation of the anomaly with the help of some content-*increasing* [italics added] change (or auxialiary hypothesis), and nature corroborates it, do we move on to eliminate the 'refuted' complex." (*Ibid.*, p. 125)

4.4. Popper and Lakatos

1. What is the relationship of dogmatic falsificationism (DF), naive methodological falsificationalism (NMF) and sophisticated methodological falsificationalism (SMF) vis-à-vis Popper?

Lakatos defines three indexed Poppers (see e.g. (1970), p. 181). *Popper*$_0$ was a dogmatic falsificationist who published nothing. *Popper*$_1$ was a naive methodological falsificationist and *Popper*$_2$, a sophisticated methodological falsificationist. The *real Popper*, according to Lakatos, was Popper$_1$ and partly Popper$_2$ (see e.g. *ibid.*, pp. 106, note 2, 116–117, 181). The real Popper, however, thinks, as does Popper$_1$, that the falsification of a theory is the outcome of a "duel" between theory and test, regardless of whether or not alternative theories are available.

In his "definition" of the true Popper, however, Lakatos' stand wavers and varies in that at one point the true Popper is even identified with Popper$_2$ (see Lakatos (1974), p. 224, note), while the main line of thought is either that NMF is contained as *part* of

Popper's methodology (see e.g. (1970), p. 181), which then in addition contains elements of SMF, or else that NMF and Popper's methodology are *identified* ((1974), p. 244, note).

2. Lakatos' reading of Popper is inconsistent even if we set out from the assumption that NMF is only part of Popper's theory. This we may observe from the following. Decision type (NMF4) is according to Lakatos an *essential* factor in NMF ((1970), p. 125), so that on his assumption (NMF4) must belong to Popper's theory. By Lakatos' argumentation it is precisely (NMF4) which guarantees that, for example, Newton's theory is falsifiable on Popper's criteria. On the other hand, however, Lakatos, inconsistently with this, *doubts* whether Popper's demarcation criterion would work in the case, for example, of Newton's theory (see (1970), p. 101, note 2, (1974), p. 247). Background to this suspicion is Lakatos' argumentation for the "thought experiment" and his analysis in the *ceteris paribus* context (see (1970), pp. 100-102, 110-111; see also § 4.1, Section 1, above)

3. Apart from its inconsistency the interpretation Lakatos offers of Popper is also in error, because the *central* type of rule (NMF4) does not appear in Popper's methodology (see § 3, above). In replying to Lakatos Popper *expressis verbis* indeed refutes the above views of his theory[27]; in particular Popper holds that *ceteris paribus* clauses should not be introduced (see Popper (1974b), pp. 999-1000, 1005, 1186, note 75; see also Johansson (1975), pp. 169-197).

4. Is Popper a sophisticated methodological falsificationist as Lakatos at one stage claims (see Lakatos (1974B), p. 244, note) - or can Popper's methodology be reconstrued as SMF?

For SMF to be viable, Lakatos' idea of a theory core C within a protective belt B must be a prerequisite. This idea being an ineliminable part of SMF, SMF and Popper's theory are in sharp conflict, since the core, inaccessible to the arrow of *modus tollens* behind the protective shield of B, is the very notion which, in view of Popper's atomism characterized above (see § 3), is perhaps "most of all" in conflict with Popper's conception of science:

> "... there can be no statements in science which cannot be tested ..." (Popper (1968), p. 47)

> "... I have never operated in a context like this with such vague ideas as "the very heart of the system" (or of the theory), or with its "*most basic* assumptions" ... . On the contrary, I have indicated that it is a matter of risky conjecture to which part of a theory we attribute the responsibility for a refutation." (Popper (1974b), p. 1010)

5. Lakatos' studies of Popper's methodology are thus found wanting - inconsistent and erroneous[28] - and, in addition, SMF, which actually tends to be Lakatos' *own* theory[29], is in marked conflict with Popper's conception of science. This by no means implies that Lakatos' theory is so to say untrue - or that Popper's is true.

CHAPTER II
AN EXPOSITION OF KUHN

The term 'scientific revolution' has in the context of RV only a socio-psychological connotation, because according to RV the development of science comprises in an essential sense an accumulation of truths. Popper's theory is for its part likewise a cumulative conception: the theory of Popper - a philosopher of science who sees himself as the apostle of scientific revolution - can in the last resort offer no place for genuine scientific revolutions. The conceptions of RV, and in a certain sense also of Popper, regarding the development of science are thus markedly conventional, and correspondingly the doctrines of both RV and Popper on the nature of empirical theories are traditional (see later parts in the present work). Thus when Kuhn proposes that the essential components of scientific development are scientific revolutions not allowing reductions between the "parties" of a revolution, this must mean - apart from a sharp dissociation from the traditional ideas of the development of science - also a drastic reassessment of the generally accepted notions of empirical theories. Kuhn's theory indeed diverges - as we shall see - qualitatively from all the theories in our problem-solving scheme for the philosophy of science, and even if one would not endorse Glymour's evaluation of Kuhn's chief work

> "THOMAS KUHN's second book, *The Structure of Scientific Revolutions*, is very likely the single most influential work on the philosophy of science that has been or will be written in this century" ((1980), p. 94),

Kuhn's thought must nevertheless be seen as of great importance.

Our analysis of Kuhn's theory in this chapter rests mainly on this outstanding work, *The Structure of Scientific Revolutions*, published in 1962 (and to some extent on the source Masterman (1970)). Our references are to the 1970 edition of *The Structure of Scientific Revolutions*, which includes Kuhn's *Postscript* of 1969. In the coming discussion of the work (*The Structure of Scientific Revolutions*, Kuhn (1970a), pp. i-xii and 1-173) it will be referred to simply as STR, and the *Postscript* (Kuhn (1970a) pp. 174-210) will be abbreviated to POST. For the purposes of the approximate survey in § 5 we shall not trouble ourselves with source references. Be it noted that in the present chapter the terms 'theory' and 'paradigmatic theory' appearing in the Kuhnian context are not defined.

The *exposition* of Kuhn, set out in §§ 5 and 6 below will be followed by an *interpretation* in later parts of this study.

§ 5. THE MAIN OUTLINES OF KUHN'S THEORY

1. Kuhn's basic concept is the *paradigm concept*, which (as Margaret Masterman assumes she can demonstrate) he appears to employ with over twenty different meanings. However, these meanings are interrelated and would seem appropriately to convey the notion that every mature branch of science must possess a certain overall pattern without which it cannot operate. By 'paradigm', then, one means preliminarily the general formative principle P of a scientific field T. According to Masterman the various connotations of the term can be subdivided into three groups.

To the first of these belong the *metaphysical* formulations of the concept, or *metaparadigms,* that is, the principles which organize our perceptive activity and by which we thus understand reality. In this sense Kuhn employs such expressions as 'a set of beliefs', 'something governing a large proportion of reality'.

Secondly, *paradigm in the sociological sense* means a scientific achievement P, which possesses the following properties:

(i) P is sufficiently unprecendented to attract a group of permanent supporters from a rival scientific camp.

(ii) P is sufficiently open to leave a suitable area of problems for experts in the field to tackle.

Thirdly, we may speak of a *paradigm in the concrete sense*: among other things an actual text-book or classic, the actual mode of application of some equipment (for example the use of fire in chemistry), an analogy or a diagram.

By *normal science* Kuhn means research based on a given paradigm P accepted by a given scientific community at a given point in history as the foundation of their practice. Normal science PNS and its paradigm P are conceptually related to each other in that P defines the limits of PNS, while PNS for its part *actualizes*[30] and *articulates*[30] the paradigm P. (We shall be returning to these concepts: as an example of the articulation of a paradigm we may mention the determinations of the gravity constant in the period 1741-1901. In Newton's own day his paradigm left much to be desired; guesses for example as to the magnitude of the gravity constant were far from accurate.)

The functions of the paradigm P of normal science PNS include among others the following:

(i) It defines what is permitted in PNS (in other words, what laws, theories, applications and instrumentations are legitimate),

(ii) it acts as a model (e.g. the astronomies of Ptolemy and Copernicus),

(iii) it is an article of doctrine which the novice must learn and accept in order to acquire membership in the PNS community, and

(iv) it makes effective research possible because the common point of departure for the members of the PNS community is a basis which is not felt to be problematic.

On the one hand, then, normal science PNS guided by the paradigm P means effective research activity, but on the other normal science PNS may in the sociological sense be characterized as "dogmatic": to the representatives of normal science reality appears to exist only if it fits into a *box* prescribed by the paradigm. The consequence is oblivion for any "fact" which fails to fit, and further, the sociological effect that the dominant academic establishment seeks to prevent the emergence of rival paradigms.

Central in the work of normal science is what is called *puzzle-solving activity*. Dependence on a paradigm means that the representative of the normal science in question does not approach reality openly but selects as objects of his research only problems which he sees in advance to be amenable to solution. For this reason Kuhn sees normal science as puzzle-solving. As examples of this activity one might take the game of chess or crossword puzzle solving or especially jigsaw puzzles. Children putting together a jigsaw puzzle know beforehand that the puzzle can be solved - it is merely a question of observing certain rules (blank sides of pieces down, no gaps to be left, pieces must not be forced into place, no pieces must be left over, no new pieces must be made, etc.). If one of the players fails to comply the others will get angry and ostracize the renegade (even if perhaps none of them can explicitly define the rules they play by).

It follows that in the sociological sense a normal-science community is a monolithic society which considers a problem important only if a solution to it is to be anticipated within the accepted paradigm. In this way a dominating paradigm can isolate the community from all such socially important problems which are not intelligible in its terms, or the establishment upholding the paradigm may - in offering for a socially important problem an explanation which does not meet the requirements of the situation - come to constitute an obstacle to social development.

A scientific problem must fulfil two conditions in order to be recognized as relevant to normal science. Firstly, it must be possible to anticipate that the problem has in principle a solution within the paradigm; and secondly, in order to maintain interest in this problem, there must be a variety of possible solutions. Further, a *set of rules* R is required, specifying the quality of acceptable solutions and the number of steps necessary in attaining them.

Kuhn compares the rules associated with a paradigm with for example the rules of jigsaw-building described above. The rules contain information on the commitments of the scientific community, these being divisible into those pertaining to equipment (e.g. the use of fire in chemistry), conceptual commitments (i.e. accepted terminology, laws and theories) and philosophical commitments (ontology, methodology, methods).

The following example of Kuhn's will illustrate the manner in which these commitments are related to each other. Cartesian physics was bound up with an ontology which held that there exists only formed, moving matter. This inevitably meant that the corresponding methodological commitments contained the following rule-schemes. Firstly, scientific laws must specify the movement and mutual influence of particles of matter. Secondly, scientific explanations must reduce a given phenomenon to the movement of particles as governed by the above-mentioned laws.

With reference to the above we shall now - diverging from the earlier description - define the concept 'paradigm' as follows (cf. Kuhn (POST), pp. 176-191). We shall say that any *paradigm* P is (at least) an ordered quadruple

(5.1) $P = [(O, U, E, V]$

in which O is an ontology, U a set of symbolic generalizations in a natural or symbolic language in a suitable formalism (*for example* of the form $(\forall x)\ A(x,\ldots))$, E a set of exemplars (i.e. "successful" research performances) and V a set of values (if for example a scientific community gives priority to quantitative over qualitative predictions, this value thus attributed belongs to the set V). The members O, U and E of the paradigm are conceptually interdependent.

Since no radical scientific innovations are possible within the framework of normal science, we must ask how such things come about. Kuhn answers that what is involved is a change of paradigm, and here the central concepts are 'anomaly', 'the wearing out of a paradigm' (to use the expression of G.H. von Wright), and 'crisis'.

Anomaly means that some "facts" are at odds with what the normal science in question would anticipate. Central to Kuhn's idea is that the emergence of anomalies does not in itself lead, by application of *modus tollens*, to the elimination of the PNS theory involved, even where corrections to theory and corrections to marginal conditions fail to eliminate the anomaly (cf. the case of the planet Mercury). Kuhn's thought thus diverges essentially from that of Popper.

When an anomaly arising with respect to a given paradigm has been outlined, the normal science it concerns will face a *crisis* which can have three possible outcomes:

(i) Within the framework of this science the anomaly proves eliminable by means of appropriate theoretical alterations or changes to marginal conditions.

(ii) The anomaly cannot be eliminated, but on the other hand no new paradigm is available. In this case the anomaly is *"labelled"* and *"shelved"*. Essential here is that the paradigm is not rejected even though logical intuition would demand this.

(iii) Via the crisis one passes to a new paradigm. The replacement of the paradigm P by the paradigm Q is a so-called *scientific revolution,* which we designate P/Q.

Be it noted that the above implies for a paradigm in use in a given scientific community, the nature of logical truth, and therefore it cannot be called in question within the framework of the science based on it. Hence the target for *modus tollens* is - not the theory in question but - the researcher who produces the unsuccessful result.

New theories may arise in connection with three kinds of phenomena:

(i) Phenomena which fit into the prevailing theory. Here there is no call for change of paradigm. If, however, a new paradigmatic point of departure is tentatively proposed, it will for "sociological" reasons have little chance of survival. (Leibniz anticipated Einstein's theory, but because Newton's theory did not appear to be faced with insuperable difficulties and because Leibniz could not show how relativistic ideas diverged from Newton's in application to the observed world, his conceptions found no posterity.)

(ii) Phenomena whose nature the existing paradigm indicates but whose details are understood only when actualization has well progressed. Here, however, the objective is to improve the degree of actualization, not to effect paradigmatic change.

(iii) Phenomena whose assimilation into the existing paradigm proves impossible. Only this third type of phenomenon is likely to catalyze a scientific change proper. The recognition of such phenomena as anomalous means that the paradigm is *worn out* (cf. von Wright (1972), p. 59): the more precise, in other words the more fully articulated a paradigm is, the closer it is to its own end.

In sum, Kuhn holds all high-level scientific activity to be always of necessity bound to a certain paradigm. The "abstract" abandonment of a paradigm is therefore tantamount to abandonment of the science which rests on it. Rejection of a given paradigm P in a situation of crisis presupposes the existence of alternative paradigm candidates $P_1, \ldots, P_n$.

A central position in Kuhn's theory is further occupied by the notion of the *incompatibility* of two successive paradigms. Let us suppose that a normal-scientific theory T_2 associated with the paradigm P_2 solves the anomalies encountered by a theory T_1 associated with the paradigm P_1. Here the predictions of the respective theories will obviously diverge from one another in certain respects. But these divergences would not be possible if T_1 and T_2 were logically compatible. (We will later elaborate the idea of incompatibility.)

Further, Kuhn holds that two successive paradigms are (in part) *incommensurable*: one normal-scientific theory cannot ultimately be translated into another normal-scientific language because the differences between the successive paradigms P_1 and P_2 entail the following fundamental points:

(i) The world of P_1 contains different entities and is governed by different laws from that of P_2.

(ii) The science corresponding to P_1 is frequently defined in a different manner than the one corresponding to P_2.

(iii) Seen from the standpoint of P_2 the acceptable facts are not the same as those seen from the standpoint of P_1.

The incommensurability thesis may also - as we wish to interpret Kuhn - be considered from another point of view. Central to normal science is the actualization of the paradigm and the associated articulation of it (to this we shall return at a later point). Here the fact that the actualization *processes* of two different paradigms are incommensurable may be metaphorically regarded as equivalent to the fact that (in the standard case) a jigsaw puzzle cannot be built with pieces picked from two different boxes. (The incommensurability thesis and the complicated relationship between this thesis and the idea of incompatibility of paradigms will be elaborated in later parts of the present work.)

We may here, by way of example, point out that in linguistics the definitions of language of the generative school and the Bloomfield school, and their respective definitions of linguistic science and of linguistic fact diverge from one another in such a way as to render their theories in a strict view scarcely commensurable (On the idea of application of Kuhn's thought to linguistics see e.g. Derwing (1973), Percival (1976), Karlsson (1980) and Itkonen (1982)).

Since successive rival paradigms are mutually exclusive and incommensurable (one may speak of an obscuring of the limit between logical and empirical in both), it is understandable that a dispute between those representing a prevailing paradigm and scholars basing their researches on a new one is not to be solved "at the draughtboard" (cf. Leibniz) - at least not if this is to be done in the actualization phase of the rival theories.

A given paradigm P thus contains commitments at least with respect to equipment; commitments as to what are the world's basic entities, how these influence each other, what kind of laws and explanations are thus appropriate, and so on; and, connected with these, conceptual commitments (terms, laws, theories); and also value commitments (e.g. whether a quantitative prediction is to be given priority over a qualitative one).

By *disciplinary matrix*, or *matrix* for short, we mean hereafter more or less the same as paradigm (cf. Kuhn (POST), pp. 176-191). Since a paradigm expressly contains a certain conception as to what are the basic entities of nature in the field concerned and as to the way they affect one another, we may speak of a paradigm also as a *matrix for a disciplinary (scientific) world-picture*: the task of normal science is to actualize, to detail and to elaborate, briefly, to articulate the paradigm, thus the disciplinary world-picture it accepts.

The *actualization* of a paradigm takes place roughly speaking as follows:

(i) The discovery of such details of reality which are of interest from the standpoint of the paradigm (in astronomy, for example, calculations of the size and location of stars).

(ii) The making of such predictions on the one hand and determination of such observation and test results on the other that the elements in these two sets of factors may be brought into confrontation. Here the object is (a) to discover ever new "points of agreement" between a paradigmatic theory and nature, and (b) to improve the degree of agreement wherever some points of agreement have been established.

(iii) *Articulation,* which falls into three parts: (a) the determination of constants (e.g. determinations of the gravity constant); (b) the search for new quantitative laws (suited to the framework of the matrix); (c) the extension of the paradigm to cover new areas.

One of the core elements in Kuhn's thought is his doctrine of the development of science, which is based on the assumption that in a given (mature) field of research the development of theories involves - in actual fact - the following stages: (a) a non-paradigmatic stage; (b) a kind of multiparadigmatic stage (occasionally possible); (c) a normal-science stage (the actualization of a particular paradigm P): (d) a crisis arising when P encounters anomalies; (e) a scientific revolution in which P is replaced by a new paradigm Q; (f) a new normal-science stage (actualization of the paradigm Q); and so on[31].

In connection with the actualization of a given paradigm we may also speak of the *(specific) adaptation* of the paradigmatic theory in question. Further, taking account of the above stages (a) - (f), we may speak of *long-term adaptation* of scientific knowledge.

In Kuhn's theory, then, emphasis falls on the following points:

(i) The paradigm defines what is legitimate within the scope of a given normal science - with respect to both problems and solutions.

(ii) Research in normal science is always bound to a specific scientific world-picture (for example the cosmology of Ptolemy vs. that of Copernicus). This world-picture contains among other things a theory-specific ontology which indicates what entities there are in the chosen field of research and what entities there are not.

(iii) Of two successive paradigms it may be said that (a) their worlds contain different entities and are subject to different laws, so that (b) acceptable facts as seen from each respectively are not the same; and finally, (c) the corresponding sciences are in many cases differently defined.

(iv) Actualization is possible only in terms of the paradigm.

(v) *Progress* proper in science is possible *only* by way of the actualization of disciplinary matrices, their abandonment and the actualization of new matrices.

In the above conditions (i) - (v) the weight falls on the Kuhnian view that there are no neutral scientific facts, only *paradigm-coloured* (or *theory-laden*) data: this not only entails rejection of the thesis of the neutrality of the observation language (see Chapter I, above) but further casts doubt upon the notion that it is possible even to *construe* for two given successive paradigmatic theories a common observation language neutral in relation to these *theories* (see also Kuhn (1976), p. 198, note 11).

§ 6. KEY THEMES IN KUHN'S THEORY

1. An example of such successive paradigms as were referred to above are the following three from the realm of physical optics (Kuhn (STR), pp. 11-12, (1963a), pp. 345-347): (P_1; Newton) Light is composed of material corpuscules; (P_2; Young & Fresnel) Light is transverse wave motion; and (P_3; Planck, Einstein and successors) Light is formed of quantum-mechanical entities having certain of the properties of waves and certain of the properties of particles.

Prior to Newton history offers not a single generally accepted notion of the nature of light such as would have allowed of puzzle-solving activity and normal science; in the absence of common standards of research and common problems writers often reconstructed the doctrine from the start, so that the approach in writing on the subject was that of disputation rather than of a search for solutions relevant to nature proper and nature's problems. (Note that *geometrical* optics was well developed already in antiquity. This dicipline should not, of course, be confused with *physical* optics, which is our concern here.)

Before Newton the following conceptions among others had been put forward as to the nature of light: (a) Light is made up of particles emanating from material objects; (b) Light is a kind of mediator between the eye and the material object; and (c) An emanation *from the* eye is one factor in the phenomenon of light.

Not one of these conceptions allowed of problem-solving activity, and although "[a]ny definition of the scientist that excludes the more creative members of these various schools *will exclude their modern successors as well.* Those men were scientists" (Kuhn (STR), p. 13; italics added), yet the results achieved in schools (a) - (c) were something less than science (*ibid.*, p. 13): in the absence of common standards questions, observations, experiments, methods, target phenomena and the type of answers put forward could be freely chosen, so that writings comprised at least as much pure polemic and disputation as research into nature. - Orientation to concrete problem-solving and avoidance of mere dispute as an indicator of good scientific standards is most interestingly characterized by Raimo Lehti (see (1980)), using astronomy as his example: the backward state of the science in Sweden-Finland in the XVIIth century, a time when Central European states were practising normal science of a high standard, was essentially connected with the polemical spirit of astronomy in this part of the world to the detriment of concrete problem-solving.

In the framework of schools (a) - (c) observations were systematized in a manner which was later to contribute to the rise of *the first* (Newton's) *paradigm for physical optics.* In the general (and in this particular) case the emergence of the first paradigm in a particular field means the establishment of a *consensus* as to the foundations of the theory in question in the scientific community: disputes between various schools are dropped, argument ceases as to the bases of the discipline in question (in the circle, that is, where results *are* science), and in this way the actualization of an esoteric paradigm becomes possible. The establishment of a paradigm has among others the following effects:

(i) A set of *professionals* is formed, characterized for example by the emergence of journals and of scientific societies, and a firm status for the subject in question as an academic discipline.

(ii) Since the paradigm provides implicit or explicit answers to the questions

(a) What are the fundamental entities in the area under study?

(b) How do these affect each other and the senses?

(c) What questions touching these entities are legitimate and what techniques are permissible in the search for answers to these questions?

the ultimate foundations of the research involved cannot be called in question in the puzzle-solving undertakings of the normal-science community; the point of departure is - and can only be - the point reached hitherto, from which one sets out into esoteric areas.

(iii) The results of research are published chiefly in the form of articles - not in books such as Darwin's *Origin of the Species* which become classics - in journals intended for professionals (others are scarcely able to read them).

(iv) A text-book tradition is established, within which results achieved in science are stylized into easily assimilable form, thus for example making it unnecessary to read the classics in the field. (How many physicists today bother to read Einstein's original articles?)

(v) A gulf opens up between scientific and lay knowledge.

(In connection with this Section 1 see Kuhn (STR), pp. 1-22, (1963a), (1963b).)

2. Let T_i be a theory linked to a paradigm P (for example Newton's paradigm for the movement of bodies) in a given sphere of application (for example celestial mechanics) at a point in time i. Here the tasks of normal science include the development of the

sequence

(6.1) ... T_i, T_{i+1}, ... ,

where actualization of the paradigm takes place as follows (see above and Kuhn (STR), pp. 23-34 and (1961)):

(i) Determination of facts significant from the point of view of the paradigm.

(ii) Improvement of the degree of agreement between theoretical predictions and facts.

(iii) Promotion of the calculation of constants (for example determinations of the gravitation constant) and the search for new specific quantitative laws.

Be it noted that, strictly speaking, points (i) - (iii) cannot in fact be distinguished from one another and that in the sequence (6.1) *scientific knowledge accumulates.*

3. Let us next consider Kuhn's relationship to inventive induction.

It may well be thought - this is the purport particularly of text-book pictures of science - that the function of compiling (for example) numerical values obtained by experiment and observation is to discover new laws and theories (see Kuhn (1961), p. 164). This presumably means that when an experiment is repeated often enough (possibly under varying conditions), and the pertinent measurements are carried out carefully enough, one may from the figures obtained inductively arrive at generalizations which those figures manifest. One can see the untenability of inductivisim in *this* sense if one thinks of the following analogy. If the set of values of a function is given, one cannot on the strength of this decide what function is in question (or, if a function and one of its values are given, one cannot from this tell the value of the argument in question if the function has no inverse function). What is substantially involved here is that when an experiment is carried out one must already know *what* experiment is in question and *what questions* (if any) it answers.

Attention has nevertheless been drawn to the fact that in certain cases induction really appears to have taken place: is not for example Boyle's law relating gas pressure with gas volume directly derived from results of measurement (see *ibid.*, pp. 174-175)? As Kuhn points out, what this representative case involves is a situation in which the scientist knows in advance everything except the function in question. Such examples, then, are of no significance for matters of principle regarding the relationship between conceptual systems and experience (see also especially Chapter IV to come):

> "Boyle's experiments were *not conceivable* (and if conceived would have received another interpretation or none at all) *until* air was recognized as an elastic fluid to which all the elaborate concepts of hydrostatics could be applied." (Kuhn (STR), p. 28; italics added)

> "The road from scientific law to scientific measurement can rarely be traveled in the reverse direction. To discover quantitative regularity *one must normally know what regularity one is seeking and one's instruments must be designed accordingly...*" (Kuhn (1961), pp. 189-190; original italics removed, new added)

Kuhn's stand is thus anti-inductivistic (in the sense mentioned). What, then, is Kuhn's position on the two other central questions of the methodology and the philosophy of science: how does Kuhn's theory relate to the falsification and confirmation of theories? Let us consider first the matter of falsification.

4. As noted above (see § 5), a counter-instance to a theory has not in Kuhn's view the impact of *modus tollens* with respect to this theory. In other words Kuhn rejects the basic starting-point of falsificationism. Since Kuhn, however, does not offer sufficiently deep and sensitive qualifications as to the bases upon which the falsificationism he attacks rejects an empirical theory (see e.g. Kuhn (STR), pp. 77-91, 146-147), we may, to begin with, specify three concepts of falsification and then show that the Kuhnian criticism applies whichever of the three interpretations the approach sets out from. We shall further show this to be so even if rejection of a theory is deferred and theories are thus temporarily "dogmaticized" (cf. Popper (1970), p. 55). Fourthly, we shall note Kuhn's

extraordinary comment to the effect that falsifying instances *cannot* hit paradigmatic theories.

Falsification of a theory in the sense of *modus tollens* (application of the Popper procedure)

(6.2) $p \rightarrow q, \sim q \vdash \sim p$,

where the rule of substitution is invoked according to the requirements of the case at hand, may be understood in (at least) three ways (cf. § 3, above). *First*, one might say that a theory is falsified if a test produces a negative result. Put thus without qualification, however, such a definition of theory falsification permits an element of chance which - naturally - must be eliminated.

Thus, *secondly*, the concept of falsification may (and should) be modified so as to require the repeatability (objectivity) of the procedure for the situation under analysis. Here - on the one hand to avoid the difficulties entailed by the concept of corroboration and on the other to elaborate somewhat on Popper's stand - we might introduce the concept of repeatability as an undefined term as does Popper with the concept of the observable, applying - *mutatis mutandis* - to the term 'repeatable' his own words:

> "It should be introduced as an undefined term which becomes sufficiently precise in use: as a primitive concept whose use the epistemologist has to learn, much as he has to learn the use of the term 'symbol', or as the physicist has to learn the use of the term 'masspoint'." ((1968), p. 103)

The problems inherent in the notion of repeatability (how many times must an experiment be repeated? Under what varying conditions? etc.) would thus in fact be eliminated by a "methodological decision" taken on the grounds, for example, that the problems repeatability involves are not, from the standpoint of *practical scientific work*, particularly serious. (True, from the point of view of Popper's theory such a procedure would at bottom mean simply concealment of the problems connected with corroboration behind the concept of repeatability and would not ultimately eliminate the "inductive" corroboration relationship between accepted basic statements and a given falsifying hypothesis.)

Thirdly, the concept of falsification may be bound to that of corroboration - as Popper eventually decides to do (see § 3, above).

Where do these principles of falsification lead when applied in the Popperian sense (a theory once falsified must be rejected "without stratagems" even if no alternative theory is available)?

Be it noted at once that although only such problems are tackled by normal science which may be assumed in advance to be soluble within the framework of the prevailing paradigm - how indeed could a problem even be posed except against the background of some paradigmatic starting-point! - nevertheless any puzzle whatsoever which has not yet been solved can be interpreted as a counter-example to the theory in question (see Kuhn (STR), e.g. pp. 79-80, 146), a counter-example whose identification as an anomaly is possible only when the paradigm has been sufficiently actualized (see Section 2, above). As an instance of this we might take the disturbances in the behaviour of the planet Mercury in relation to the predictions of Newton's theory. The recognition of these disturbances, in the first place at all, then secondly as a serious problem for Newtonian physics to solve, and thirdly as an anomaly counter to Newton's theory, was possible *only* against a sufficiently matured theoretical *Newtonian* background - that developed by such successors in normal science as Euler, Lagrange, Laplace and Gauss (on the above see Kuhn (1961), p. 191). This counter-example to Newton's theory (originally a puzzle which was subsequently identified as an anomaly[32]) was later explained by Einstein's theory (see e.g. *ibid.*, p. 191).

Every theory is constantly faced with counter-examples (see above and Kuhn (STR), e.g. pp. 77-91, 146-147), some of which are successfully explained at a given point in actualization, while the analysis of others is by reason of their difficulty of solution postponed; of these, again, some are ultimately (with sufficiently progressed actualization) interpreted as anomalies which for the theory in question entail a crisis. (It is of course possible that an anomaly which has been regarded as established may in certain cases, by means of an ingenious adjustment to the theory itself, take its place among problems solved.)

However, the above implies that every theory invariably has its *established* ("*repeatable*" or "*corroborated*") counter-examples[33] and thus "even more" counter-examples upon which no additional qualifications are placed. But now, the principle of falsification - in whichever of the above three senses one takes it - would lead to the continual falsification of theories (see *ibid.*, p. 146), so that their potentiality would remain unactualized. This in turn would thwart the progress of science and relegate it to a pre-paradigmatic, disputative stage in which high-level and elaborate problem-solving and the construction of a *well-founded* scientific world-picture would be impossible.

Let us suppose that the above-mentioned principle of theory rejection (in each of the respective interpretations placed on falsification) is replaced by one which

(i) by merit of conditions construed in one way or another fulfils the methodological norm whereby a theory must be allowed a "suitable" period of development (see Popper (1970), p. 55), but which nevertheless stipulates that

(ii) a theory is to be rejected if it has been falsified in one of the above senses with condition (i) prevailing, *regardless of whether or not there is an alternative theory available.*

It follows from Kuhn's theory, however, that this solution likewise proves invalid, and in any case it is inappropriate to actual research practice - such "deferred" falsification (see *ibid.*, p. 55), too, would in the absence of alternatives lead to the abandonment of science itself or to a change of profession:

> "... if the scientist stays where he is, anomalous observation ... cannot tempt him to abandon his theory until another one is suggested to replace it. Just as a carpenter, while he retains his craft, cannot discard his toolbox because it contains no hammer fit to drive a particular nail, so the practitioner of science cannot discard established theory because of a felt inadequacy. At least he cannot do so until shown *some other way to do his job.*" (Kuhn (1961), p. 184; original italics removed, new added)

Finally we want to note the following point in the Kuhnian view: a paradigm enjoys - if Kuhn is right - the status of a tautological entity in the scientific community in question, so that no kind of evidence can refute it (see Kuhn (STR), e.g. pp. 68-69, 74, 77-80, 133, 144-147). As an example of this Kuhn mentions Newton's second law, which no evidence appears able to refute for those who are committed to Newton's theory.

In sum the Kuhnian view of falsification is this: the rejection of theories simply on the basis of *modus tollens* would on the one hand lead to chaos whichever of the above-mentioned construals of falsification were applied (nor would the matter be helped by the "deferment" of rejection). On the other hand paradigms seem to possess a tautologuous status, which implies that the arrow of *modus tollens* cannot - if Kuhn is right - ever reach them.

5. Can theories be confirmed or corroborated? We take up the following three points of the Kuhnian view against the confirmation and corroboration of theories.

Firstly, in the case of the positivistic view of science (RV) the confirmation of theories was based on a theory-free or neutral observational language. The existence of a theory-free and stable observational language was, however, denied outright by Kuhn - and with good reasons (as we have seen). But the elimination of the theory-free evidential basis eliminates the very basis of the confirmation theory in the RV sense.

Secondly, in the case of Popper, we must note that Popper also denies the positivistic or RV-type assumption which claims the existence of a theory-free observational language:

> "... there can be no *pure* observational language, since all languages are impregnated with theories and myths [?!]
>
> ... *there is no theory-free language* to describe the data, because [?!] myths (that is, primitive theories) arise together with language." (Popper (1968), p. 146; italics added)

Now, are the theses of Popper and Kuhn regarding theory-ladenness no more than different nuances of the same idea? That this is not the case is explicated as follows. Popper insists that for any two theories, say T_1 and T_2, under comparison it is always possible to find or construct a "roomier" (Popper (1968), p. 56) system, let us call it SUPER, in which T_1 and T_2 as well as the relevant basic statements can be represented and neutrally compared with no "distortions" coming from T_1-ladenness or T_2-ladenness. In contradistinction to this, Kuhn's incommensurability thesis claims that for two successive paradigmatic theories there is no common SUPER, in which the theories as well as the relevant data could be compared as required by Popper (see Kuhn (STR) *passim*, (1976), esp. p. 198, note 11, (1979a), esp. p. 416, second paragraph). Now, as far as Popper wants to speak of one theory as being better corroborated than another - and this Popper himself wants to do (see e.g. (1968), p. 268) - this is, according to Kuhn, rendered impossible, at least in the case of paradigmatic theories, in the absence of any SUPER.

Thirdly, Kuhn has also another kind of argument against confirmation (and corroboration). This argument runs more or less as follows (Kuhn (1961), p. 171). Let us suppose for the sake of argument that testing were to have a confirming function in cases where the predictions of a theory and the results of experiment (observation) are found to agree. But is one not then all the more compelled to acknowledge that in cases of disagreement one ends up with falsification? But the normal-science research process does not lead to falsification. Evidently, then, with even less reason can one speak of theory-confirmation. - Kuhn's argument is very clearly based on a comparison of the invalid mode of inference

(6.3) $p \rightarrow q, q \vdash p$

and the valid *modus tollens* form of inference

(6.4) $p \rightarrow q, \sim q \vdash \sim p$,

and on - *modus tollens*!

6. If, then, theories cannot - as Kuhn claims - be either falsified or confirmed (in the usual sense), what is the function of the research process in normal science? According to Kuhn it is this. The task of normal science is to *actualize its paradigm*, for example by improving the degree of agreement between predictions and

observations and seeking new quantitative "experimental" laws. Although this may ostensibly be reminiscent of what is meant in the philosophy of science by testing, what is involved is not (as far as paradigmatic points of departure are concerned) testing at all, for the paradigm determines the quality of both question and answer. In any case the fact remains that normal science has as its objective

> "... to solve a puzzle for whose very existence the *validity of the paradigm* must be assumed. *Failure to achieve a solution discredits only the scientist and not the theory.*" (Kuhn (STR), p. 80; italics added)

Research processes thus act as tests, namely as experimental situations which test the researcher; in contrast, the paradigmatic points of departure are not under scrutiny in these processes.

The testing of paradigms (even if they comprise claims formulated as laws) is thus, in Kuhn's conception, not possible in the *modus tollens* sense, because in normal science problems are solved on terms imposed by the paradigm applied. Although a wide variety of possible solutions may be proposed for a problem, and erroneous alternatives (those which fail to achieve the objective) are eliminated, this does not constitute a testing of the *paradigm.* A metaphorical counterpart to this situation (see also § 5, above) is, for example, that in tackling a chess problem the player is not testing the *rules of the game*; moreover the rules are not "falsified" or "confirmed" according to the correctness or incorrectness of the moves of a player. (On the foregoing see Kuhn (1961), (1963a), (1963b), (STR), esp. pp. 144-145.)

One may only speak of the testing of a paradigm when (a) anomalies have led to a crisis in the scientific community; and (b) when alternative paradigms are available. In the testing of paradigmatic starting-points - should one wish to speak of such a procedure - a number of components are involved: nature and *at least* two rival theories constitute the basic factors.

7. But if there is no induction (neither in the context of discovery nor in the context of justification), if theories cannot be falsified, if logical and empirical truth cannot (in the context of

normal science) be distinguished from one another, and if even facts are changed to "facts", then is not the consequence the elimination of scientific objectivity?

Kuhn (1961) shows particularly clearly (see also e.g. (1963b) and (STR)) that this is not only a consequence of his theory, it is its almost explicit programme - in as far as objectivity is understood in the above sense or more narrowly, as for example in the manner of Popper (see also § 3):

> "The *objectivity* of scientific statements lies in the fact that they can be *inter-subjectively tested*." (Popper (1968), p. 44)

Let us turn next to a consideration of this theme.

8. One of the central components in Kuhn's formulation of this problem in (1961) might be condensed thus: Is the scientist - as one is wont to imagine - an impartial seeker after truth, who without prejudice or commitment collects and analyzes objective facts, carries out appropriate measurements and is the more satisfied with a theory the more its predictions are in agreement with the results of experiment (observation), but is prepared to reject this theory (or alter its basic constituents) if the results do not agree - and further: can he, on the basis of the objective data he has collected, arrive at genuine scientific innovations?

As we have seen, Kuhn answers all these questions in the negative. Let us here content ourselves, in the context of this problem of objectivity, with Kuhn's example of Dalton (on this subject see Kuhn (1961), pp. 171-177).

Chiefly on the basis of his observations in the sphere of meteorology and physics Dalton proposed that atoms are the smallest indivisible components of matter, and that they can combine only in a manner expressible in ratios of small whole numbers. Dalton had the idea of using chemical measurements: in searching through the literature for relevant information he observed that suitable light was shed on the subject best by reactions in which two elements formed at least two different compounds. Dalton believed the literature offered support for his tentative Law of Multiple Proportions, but what is essential is that the greater part of the existing factual material was in conflict with this law. For example where Proust, recognized as an outstanding *experimentalist* - perhaps the best in his time - arrived at a ratio of 1,47:1 for oxygen in his measurements concerning copper oxides, according to Dalton it should have been 2:1. It was not until some fifty years after the first proposal of the Law of Multiple Proportions that quantitative analytical methods suitable from the standpoint of Dalton's claim were created along the lines adumbrated by his theory: once chemists knew what kind of results they ought to expect of their chemical analyses, they were gradually able to devise techniques which produced these (desired) results; and thus

> "... chemistry texts can now state that quantitative analysis confirms Dalton's atomism and forget that, historically, the relevant analytic techniques are based upon the very theory they are said to confirm. Before Dalton's theory was announced, measurement did not give the same result. *There are self-fulfilling prophecies in the physical as well as in the social sciences.*" (*Ibid.*, p. 173; italics added)

As to Dalton himself, I.B. Cohen remarks:

> "... Dalton's magnificent construction of the atomic theory was made possible by his deliberate adjustment of certain *experimental* results to *fit* his *theory*..." ((1974), p. 341; italics added)

The objectivity of science is generally taken to mean that theories must be fitted to facts and not *vice versa*. Are we then to accept that the abandonment of objectivity and the adaptation of facts to theories having the nature of tautological truth by manipulation of these facts are not only permissible in the Kuhnian view of science (as indeed in Kuhn's allies' view), but in this view are in fact precisely the mark of mature scientific activity? For the moment we shall not go into this question but pass on to an examination of the nature of the rules of normal science.

9. Kuhn sees the treatment of scientific problems in normal science as the solving of puzzles (for example a jigsaw puzzle). Just as the latter does not involve a testing of the rules of the game,

the former is not a matter of testing the paradigmatic commitments: the process of actualization pursued in normal science is possible only so long as the paradigm is taken as given (see (STR), e.g. pp. 144-145).

The rules of normal science, specifying for example the quality of acceptable solutions, fall into more or less the same categories as the elements of the paradigm in (5.1), or else they are closely associated with these categories; among others the following types of rule may be distinguished (see *ibid.*, pp. 35-42):

(i) Ontological commitments and the methodological rules often closely linked to these (see the Cartesian example in § 5, above).

(ii) Generalizations, which include explicit formulations of scientific concepts, laws and theories.

(iii) Instrumental rules laying down the legitimate modes of application of equipment (for example, the use of fire in chemistry has changed with development).

(iv) Value commitments, which Kuhn here refers to as rules (for example, norms (or suitably expressed values) whereby the task of the scientific community is to study nature, and to study it on as broad a scale and as precisely as possible (see also *ibid.*, p. 168)).

10. Kuhn does, however, point out that the paradigm may guide research even though explicit rules abstracted from it are lacking ((STR), e.g. pp. 42-43). We may pass over the problematics involved here and note briefly that Kuhn argues for the *similarity* of problems and solutions in normal science (in corresponding order) on the one hand, and on the other, for the *openness* of normal science with respect to areas of application of paradigms by reference to Wittgenstein's concept of *family resemblance*. We may take up this matter now in rough outline (see *ibid.*, pp. 44-46, Wittgenstein (1963), §§. 66-72).

What must we know in order for example to employ the word 'game' correctly? The classical answer is that (in principle) we must be able to give certain *fixed* predicates $F_1,\ldots,F_n$ by which the set G of all possible games (and only of games) G is defined sharply so that in principle no failure to separate a game from a non-game can occur. According to Wittgenstein no such predicate constellation defining the sharp and fixed boundaries for the set G of all possible games and only of games need in principle exist. Rather, in encountering an activity hitherto unfamiliar to us we employ the word 'game' because what we observe falls in some respect into similarity relationships with activities we have previously accepted as games - in other words, the elements of G are bound by certain *family resemblances*.

According to Kuhn a similar situation prevails in normal science: in the actualization of a paradigm, as it is extended to new areas, there is *no* set of sufficient and necessary conditions available which would unambiguously define the limits of the set formed by the applications of the paradigm; on the contrary, it is precisely the results hitherto achieved within the framework of the paradigm which form the constellation with whose members new applications will show family resemblances.

The afore-said renders understandable the openness of the paradigm, that is, the fact that *it is impossible to see in advance* from the viewpoint of the paradigm what areas of application it might be extended to; for example

> "[f]or the heavens Newton had derived Kepler's Laws of planetary motion and also explained certain of the observed respects in which the moon failed to obey them. *For the earth* he had derived the results of some scattered observations on pendulums and the tides. With the aid of additional but ad hoc assumptions, he had also been able to derive *Boyle's Law* and an important formula for the speed of sound in *air*." (Kuhn (STR), pp. 30-31; italics added)

These few examples may serve to illustrate how Newton's laws of motion can in no way form a set of rules affording in advance sufficient and necessary conditions for the set of areas of application. In this context we may further note the following:

> "[Newton] saw that the straight-line character of light rays is explainable on the assumption that light consists of small *particles* ejected at great speed from the light source. *Following the laws of motion,* such particles will travel along straight paths." (Reichenbach (1951), pp. 168-169; italics added)

Subsequently, however, Newton's optics had to be abandoned (see above).

11. As has been stressed in the foregoing, normal science does not produce spectacular innovations inside it: nevertheless, the long-term process of adaptation in science is characterized above all by scientific breakthroughs and revolutionary discoveries. Though these are not possible within the framework of normal science, normal science, of all practices, does possess a built-in mechanism which tends to catalyze crises and scientific novelties. The core of this mechanism lies in the fact that the actualization taking place within the framework of normal science gradually renders a theory so accurate in its discriminating capacity that the scientist really is in a position to know exactly what, according to the theory he is operating with, should happen - and *thus* it is also possible, with ever greater certainty, to classify counter-examples to the theory as anomalies: *an anomaly is perceptible only against the background of a sufficiently discriminating scientific world-picture afforded by a paradigm.* Hence we can understand that the strict adherence to old conceptual categories which is frequently branded as dogmatism and as a consequence of human idiosyncracy, is, in the long run and from the standpoint of the scientific *community,* an essential and ineliminable condition not only for high-standard normal science but also for *genuine* scientific revolutions appropriate to the challenges of the situation. (On the foregoing, see Kuhn (STR), pp. 52-91, (1961), (1963a), (1963b), esp. pp. 349-350.)

12. As an example of the emergence and identification of an anomaly consider the following. In the discovery of X-rays the initial, unanalyzed observation of Röntgen in his laboratory that there must be something seriously wrong from the standpoint of our knowledge in the sphere of physics *at the time,* was no more than the beginning. Only after a certain time-lag, during which the anomalous event was analyzed, could one properly speak of a new phenomenon.

The evolution of such situations generally takes place in three phases: (a) awareness of the existence of an anomaly; (b) observational-conceptual location of the phenomenon and its identification as an anomaly; and (c) its ultimate elimination with the aid of the old conceptual system *or* its designation as an unsolved problem *or* the replacement of the categories in the old paradigm with new, hitherto unknown ones. We shall not embark on a closer analysis of the difference one may make between the emergence of new phenomena and the emergence of new theories. (On this section, see Kuhn (STR), pp. 52-65.)

13. Kuhn's example of the Ptolemaic and Copernican astronomies provides a good illustration of a scientific breakthrough. Let us take up those aspects of it which are significant for our present concern:

(i) What is known as Ptolemaic astronomy was originally developed roughly between 200 B.C. and 200 A.D. It was a normal-science theory within whose framework the solving of esoteric problems was possible. It was thus not a question of a pre-paradigmatic school of the kind whose results are for Kuhn something less than science. We might add that in fact the theory is still in use for certain calculations.

(ii) In the course of time an increasing number of counter-examples were found to Ptolemy's theory, their reconciliation with its tenets becoming an important element in Ptolemaic normal science.

(iii) The consequence, however, of eliminating discrepancies by means of various minute adjustments was that the complexity of the theory grew faster than its precision, and that usually a discrepancy which was corrected in one place would reappear in a new form somewhere else.

(iv) By the XIIIth century the difficulties of normal-science practice on Ptolemaic principles were already clearly seen, and before Copernicus' day there was a general awareness that something was irreversibly wrong with the theory.

(v) The sources of the scientific crisis thus arising were: (a) failure in solving the problems of normal science; (b) a social need (calendar reform); (c) a number of elements in the overall scientific atmosphere; in addition we may mention as an external *brake* on the crisis (d) the Church. Of these various sets of factors Kuhn's theory is addressed, however, mainly to type (a), since the other influences (for example the social and ideological) appear to affect more the *timing* of the crisis and not actually its content.

(vi) Via this crisis Ptolemy's astronomy gave way to that of Copernicus, which replaced the old approach with new, hitherto unknown categories.

(vii) Finally, it is especially worth noting that a heliocentric world-picture had already been proposed by Aristarchus in the third century B.C. Hence it has often been thought - thus Kuhn remarks - that if Greek science had been less deductive and less dogmatic, heliocentric astronomy might have been launched on its development 1800 years before this actually happened. Such a mode of thought, however, Kuhn holds unacceptable for the following reason. There were no such lacunae in the earth-centred system which Aristarchus' tentative idea might have filled, and observations afforded no possibility of making a choice between the two theories. One might say that there was no reason to take Aristarchus' view *seriously*. (Would it, at the time, have provided as effective navigational aids to Mediterranean shipping as did the old view? Hardly.) Only when Ptolemy's theory was actualized far enough could it be seen that an alternative must be found. (On the Section, see Kuhn (STR), pp. 66-67.)

14. In the general case we may formulate the above in the following manner.

Let T be a paradigmatic theory. When at some stage in its actualization the activity of the normal science constructed upon it meets serious failure, and there is general awareness of this, T will be faced with a crisis. During this phase a number of alternative candidate theories - let us refer to them as $T_1, \ldots, T_n$ - will often present themselves. *If* the crisis ends with the rejection of T, this in *only* possible if one of the theories $T_1, \ldots, T_n$ is accepted.

15. By the *presentiment problem* we mean the following. Let T_1 and T_2 be successive paradigmatic theories. Is it now possible for T_2 to be "foreseen" prior to the crisis of T_1, and if so, what are the consequences of such a presentiment?

Such foreshadowings are of course possible (we have the examples of Aristarchus/Ptolemy/Copernicus and Leibniz/Newton/ Einstein). It would nevertheless appear - this is most manifestly the content of Kuhn's theory - that there are clear-cut theoretical obstacles associated with the course of development of knowledge, which make it impossible for a "forerunner" such as Aristarchus or Leibniz to catalyze the initiation of *scientific* activity (normal science): he would have to be able to show how the observational consequences of the theories in question diverge from one another, and how the new proposal is superior to existing, established knowledge. The difficulty of this is surely reflected in the absence of any clear-cut historical example.

16. According to Kuhn a theory in crisis is not eliminated by comparing it with nature but by comparing it *and* at least one rival theory with nature.

Research in the crisis phase may be called *crisis science* or *extraordinary science*. It is characterized by the following features (see Kuhn (STR), pp. 77-91):

(i) Consensus is sought as to what constitutes the fundamental anomaly A.

(ii) An attempt is made to isolate A as precisely as possible.

(iii) A solution to A is sought within the framework of the original theory; and if this cannot be effected,

(iv) It is shown as clearly as possible where the rules of the old theory are inadequate.

(v) Various theoretical random walks are carried out.

(vi) Speculative theories are created.

(vii) It is sought by conceptual analysis (for example philosophical analysis) to solve the dilemma.

(viii) *Thought experiments* are carried out (this was the approach of for example Galileo and Einstein), one of the objectives being to show the rules of the old theory to be absurd (see esp. Kuhn (1964)).

If the crisis ends with the emergence of a new paradigm, a scientific revolution has taken place, in which science passes to a new set of scientific categories.

But how is *comparison* of the paradigm P and its alternative candidates P_1, ..., P_n as well as *choice* between them possible, if paradigms are incommensurable entities? The following sections will examine this theme.

17. Kuhn sees scientific revolutions to have the same structure and conduct as political revolutions.

A political upheaval develops in more or less the following manner (ref.: Kuhn (STR), pp. 93-94). The political crisis necessarily preceding a revolution arises when it becomes increasingly manifest that the established political institutions E are inadequate to the organizational and social challenges, some of which - this is a fundamental point - are produced by E *itself* or are such that they can be widely recognized only at the level of social organization E has reached. Growing awareness of E's ineffectuality brings out alternative models M_1, ..., M_n, from theoretically developed systems (arising during the crisis or handed down from earlier history) through scattered, for example populistic movements and ideas down to secular organizations of religious groupings and in the extreme case even the ideologies of fractional groups working for the total rejection of social cohesion. (Our present-day world offers examples enough of all of these developments.) As consciousness of the crisis deepens there may be increasing commitment to models M_1, ..., M_n; and when the crisis has progressed far enough, society will often be divided into competing camps, one of them defending the old order and the rest seeking new social patterns. Here the result may be such an opposition of forces that the society no longer remains amenable to control by its political institutions; it is at this point that a political revolution is possible. The objective in a political revolution is to change the society's institutions - but *in a way which is not derivable from the establishment's rules governing political behaviour*; on the contrary, the mode of promotion is usually in conflict with these rules. On the other hand, those institutions which the revolution will in the course of time give rise to are not yet in being. Thus: in the revolutionary situation the constellation for political control and the yardsticks of judgement are lacking. And again, there are - there can be - no suprainstitutional means of dealing with the situation, which means that resort will be taken to persuasion of the masses - violence being one possible method. A political revolution is by nature non-institutional, but it creates institutions: in the decisive phase there is no higher tribunal than the decision of the community in question.

18. Let P be a paradigm in crisis, let R be its set of rules governing normal-science activity and let $P_1, \ldots, P_n$ be rival paradigm candidates. The similarity Kuhn discerns between scientific and political revolutions is seen if we parallel (see Section 17, above) P and E, R and S and P_i and M_i ($i = 1, \ldots, n$). The comparison gives for the case of two successive paradigm P and Q the following:

(i) If the new paradigm Q is accepted, the old paradigm P must back down (P and Q are incompatible).

(ii) The differences between P and Q concern such fundamental matters that P and Q are incommensurable. For example scientific standards are not the same in P as in Q, so that

> "[the choice] between competing paradigms proves to be a choice between incompatible modes of community life.
>
> When paradigms enter, *as they must,* into a debate about paradigm choice, their role is necessarily *circular.* Each group uses its own paradigm to argue in that paradigm's defence.
>
> ... *in paradigm choice ... there is no standard higher than the assent of the relevant community.*" (Kuhn (STR), p. 94; italics added)

(iii) To assume that the discrepancy between P and Q might be overcome by a third paradigm R which is neutral vis-à-vis P and Q would be tantamount to imagining that a political discrepancy could be settled by some third, neutral power from without.

19. Let us now consider a fourth extraordinary point in the Kuhnian view of science (the first was normal science as puzzle-solving, the second the tautological character which a paradigm of a given *empirical* discipline enjoys in the scientific community in question, and the third, as noted above, the "political"

conduct of scientific revolutions).

Familiar from contexts involving our perception of gestalt figures are those forms in which one sees in turn two different objects, for example a cube from below and a cube from above. One frequent example in the literature is the duck/rabbit figure, after which the perceptual phenomenon is here named the *D/R effect*: in this figure one discerns alternately D or R, or one can teach oneself to perceive only lines, but one cannot see D-R (a "duck-rabbit" creature).

Kuhn sees the change of categories taking place in a scientific revolution P/Q as a D/R effect:

> "What were ducks in the scientist's world before the revolution *are* rabbits afterward." ((STR), p. 111; italics added)

In the process of the scientific revolution P/Q we in a way pass, according to Kuhn, from world W_P to world W_Q, in which situation the scientist must re-learn his mode of perception; once he has done so, W_P and W_Q will seem in many respects incommensurable.

The parallel between P/Q and D/R fails, however, in the sense that (1) the scientist cannot refer to anything outside his observations or instruments, for if he did, one must ask whence this higher authority originates; and (2) the scientist cannot, according to Kuhn, pass back and forth between P and Q[34]. (In contrast one may in D/R (a) pass at will from one to the other; (b) know that one is looking at the same paper on which one has previously distinguished D and R, or of which others have said that it is possible to do so; and (c) see only the lines as well as these lines in the form of D or R.)

A traditional mode of dealing with the scientific change P/Q might in Kuhn's view be the following:

(*) In the revolution P/Q only the *interpretation* changes with respect to certain observations. These observations, on the other hand, are fixed once for all by the object of the observations and by man's perceptual apparatus.

The following example of Kuhn's serves to characterize the content of (*). Consider a weight swinging on the end of a string and gradually attaining a state of rest[35]. According to the teachings of *Aristotle* heavy objects, when released, fall by natural movement from a higher to a lower place. Thus anyone of Aristotle's persuasion considering the swinging weight will regard it as a falling object (an object striving towards the centre of the universe), whose fall is prevented by the string so that it can achieve rest at the lowest point only by a "devious" route and after a considerable passage of time. To *Galileo*, in contrast, a weight swinging on the end of a string represented what is called a *pendulum* (of which we may properly speak only as a result of the work of certain Scholastics, see *ibid.*, p. 120). According to conception (*) both Aristotle and Galileo saw in this swinging weight the same thing, but they gave different *interpretations* to what they saw. In Kuhn's view this is not so; Aristotle *saw* a weight swinging on the end of a string as a case of prevented fall, while Galileo *saw* it as a pendulum. This means that, according to Kuhn, there is no simpler way of seeing the object in question than, for example, as a pendulum (in as far as one considers observation in the sphere of *science*). If it is seen as something else, it is nevertheless seen *as something*, that is through some paradigm.

But now, if scientific revolutions do not involve a change of interpretation such as envisaged, where are interpretations placed? They must be placed somewhere, because interpretation *does* take place in science. The articulation of this last-mentioned *fact* in Kuhn's philosophical paradigm is explained by noting that interpretation belongs to the articulation of the paradigm: interpretation presupposes some paradigm, the actualization of which may comprise evolving interpretations (these latter being, however, all coloured by or laden with the *same* paradigm). An interpretation can only articulate a paradigm - not correct it. Normal science - and interpretations of data - may lead to the recognition of anomalies and to crises, but - according to Kuhn - a crisis does not end in a re-interpretation but in an event which resembles the D/R effect and in consequence of which, for example in chemistry, "... chemists came to *live* in a world where reactions behaved *quite differently* from the way they had *before*" (*ibid.*, p. 134; italics added). Similarly in studies by certain Scholastics of the movement of solid bodies "[p]endulums were brought into *existence* by something very like a paradigm-induced gestalt switch" (*ibid.*, p. 120; italics added).

We concluded the foregoing section by asking how choice and comparison between two paradigms P and Q is made in the revolution P/Q. We now have the conclusion that the paradigms are not only incompatible and incommensurable but also appear to create their own different worlds - so that Kuhn's idea of the theory-ladenness of observations and data seems to go as far as one can imagine.

How, then, is comparison and choice made between P and the competing paradigm Q? If we say that P and Q are incommensurable in respect, for example, of their standards, concepts and even their worlds, thus allowing no SUPER for P and Q, there would seem to remain no other alternative but to say that a paradigm switch involves a *conversion*:

> "... before they can hope to communicate fully, one group or the other must experience the *conversion* that we have been calling a paradigm shift. Just because it is a transition between incommensurables, the transition between competing paradigms cannot be made a step at a time, forced by logic and neutral experience. Like the gestalt switch, it must occur *all at once* (though not necessarily in an instant) or *not at all.*" (*Ibid.*, p. 150; italics added)

Conversion, for its part, is sought - since logic and observation have no effect - by means of *persuasion* (that is, propaganda), appealing possibly to the problem-solving capacity of the theory associated with the paradigm in question, the revelation of new phenomena, aesthetic factors and so on, in respect of which the new paradigm may *promise* more than the old one could do.

In many cases, however, the work of conversion fails, in which case the rebels have no alternative but to bide their time and let biology work for them - or as Max Planck puts it:

> "[A] new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents *eventually die*, and a new generation grows up that is familiar with it."[36] (Italics added)

And when the revolution has taken place and one asks (or someone dares to ask) whether it was progress, Kuhn's theory seems to offer no alternative but to say without qualification that the *winner* is always right:

> "Revolutions close with a *total* victory for one of the two opposing camps. Will that group ever say that the result of its victory has been something less than progress? That would be rather like admitting that they had been wrong and their opponents right. *To them,* at least, the outcome of revolution must be progress, and *they are in an excellent position to make certain that future members of their community will* see *past history in the same way*." (Kuhn (STR), p. 166; italics added)

And indeed, it is not only that scientific training is made "a narrow and rigid education, probably more so than any other except perhaps in *orthodox theology*" (*ibid.*, p. 166; italics added); even the history of science is manipulated in Orwell's 1984 style - and thus legitimation of the victor's power (and indoctrination of the novice) is complete. (On this section, see *ibid.*, pp. 111-159 *passim.*)

20. For the time being we shall leave open the question whether a few strokes might be added to the above exposition of Kuhn which - D/R! - would bring about a gestalt switch.

21. In Chapter III to follow we shall take up the main part of what we feel to be the most pertinent and important elements in the criticism levelled at Kuhn's theory during the 1960's and to some extent in the early 1970's.

CHAPTER III
CRITICS OF KUHN'S THEORY: SHAPERE, TOULMIN AND THE CONTRIBUTORS TO "CRITICISM AND THE GROWTH OF KNOWLEDGE"

We shall divide the critical literature on Kuhn's theory roughly into two classes: first the "older" critiques of the 1960's (and in part the early 1970's), and second the Kuhn controversy in the literature of the 1970's and the beginning of the present decade. The earlier Kuhn debate, that which in our opinion largely set the tone of the more recent criticism, will be the subject of the present chapter; the subsequent controversy will be taken up in later parts of this work.

In July 1965 the *British Society for the Philosophy of Science* and the *London School of Economics,* under the sponsorship of the *Division of Logic, Methodology and Philosophy of Science* of the *International Union of History and Philosophy of Science,* arranged in London an international colloquium on the philosophy of science, whose published articles have since proved influential. Volume 4 of them, *Criticism and the Growth of Knowledge* (edited by I. Lakatos and A. Musgrave, Cambridge University Press, Cambridge, 1970), contains the following papers: Kuhn's opening article (1970b), assessments of Kuhn's theory in Watkins (1970), Toulmin (1970), Popper (1970), Masterman (1970), Lakatos (1970) and Feyerabend

(1970a), and Kuhn's reply (1970c). Of these critics Masterman, whose attitude to Kuhn is favourable, makes her own contribution to the debate on the concept of paradigm; the other evaluations are bitterly opposed (Feyerabend albeit coming out in defence of certain of Kuhn's views), and the extensive article by Lakatos contains, apart from his criticism of Kuhn, his own much-debated attempt to answer the new challenges of the philosophy of science (see § 4, above).

The significance of *Criticism and the Growth of Knowledge* extends beyond its immediate influence on the Kuhn debate around 1970, not only to subsequent analyses of Kuhn's theory but also to developments in the philosophy of science to this day (and doubtless for some time to come). (In the following we consider also a couple of critical articles by Popper and Lakatos outside "Criticism".) The criticism of Toulmin, who has taken issue with Kuhn in a variety of connections, diverges from the attacks of the other writers in the collection to such an extent that we shall not class him among the "Criticism" writers. Likewise Shapere, an important evaluator of Kuhn's theory, represents an approach of his own in the debate of the 1960's and early 1970's.

For the present purpose, then, the outstanding earlier critics of Kuhn's theory are Shapere and Toulmin and the "Criticism" contributors Watkins, Popper, Lakatos and Feyerabend. The selection is based, it is true, on a value judgement, which, however, is doubly justifiable. In the first place, the pattern of priorities among the problems of the philosophy of science inevitably raises these scholars into prominence, and secondly, even a superficial survey of the bibliographies shows that few works on the philosophy of science, relevant here, have been able to omit "Criticism" from their lists of references, while, equally, Shapere and Toulmin enjoy unquestioned status.

We shall now examine the views of the older critics of Kuhn's theory in the following order: Watkins, Popper, Lakatos, Feyerabend, Shapere and Toulmin. Evaluation of their criticism will be the task of later parts of this work.

§ 7. CRITICS OF KUHN'S THEORY IN "CRITICISM AND THE GROWTH OF KNOWLEDGE"

7.1. Watkins

1. Watkins' attention centres markedly on the fact that, in Kuhnian normal science, testing is testing of the scientist, not of the theory, and that genuine theory-testing can only be conceived in the phase where one paradigm gives place to another, that is, in crisis science (see (1970), pp. 27-28; cf. §§ 5-6, above). Watkins' uncompromising view is that the absence of genuine testing in normal science (if there is such a thing) is not due to any theoretical considerations (connected for example with the growth of scientific knowledge); it is forcibly prevented by the scientific community (cf. §§ 5-6, above):

> "... within normal science ... the genuine testing of prevailing theories is rendered, in some rather mysterious *psychological-cum-sociological* way, impossible." (*Ibid.*, p. 28; italics added)

This general conception of Kuhn's theory is of prime importance from the standpoint of the present undertaking.

2. The consequences of this absence of genuine testing for Kuhn himself, as for Kuhn's relation to Popper, are - as we here dare to condense *Watkins'* view - the following (see Watkins (1970), pp. 28-31):

(i) For Kuhn, normal science is the *normal* state of science and crisis science an *abnormal* state.

(ii) *Popper* also allows, in the development of theories, a certain respite without falsification (cf. §§ 6 and 3, above), but the fact that Kuhnian normal science entails prevention of the testing and falsification of theories by community strategies means, on Popper's criteria (cf. § 3, above), a degeneration of science into metaphysics; what Kuhn regards as science (normal science) is for Popper not science at all.

(iii) Setting out from the *Kuhnian* mode of demarcation, whereby science is distinguished from other activities above all in its puzzle-solving activity, Kuhn's criterion may be thus formulated:

Normal science (in which there is no testing) is genuine science, and crisis science (where genuine testing may take place) is so abnormal, and diverges so markedly from true science that it can hardly be called science at all. Thus Kuhn's and Popper's *lines* of demarcation may coincide, but

> "... they divide the material in opposite ways. What is genuinely scientific for Kuhn is hardly science for Popper, and what is genuinely scientific for Popper is hardly science for Kuhn." (*Ibid.*, p. 29)

3. On the basis of his own formulation of Kuhn's demarcation criterion (see the preceding section, point (iii)), Watkins poses the following questions (see (1970), pp. 31-32): Why does Kuhn value normal science and set no store by science in crisis? Why does Kuhn, who is supposed to be studying the dynamic process of acquisition of scientific knowledge, identify science with its periods of theoretical stagnation? Why does he not like scientific revolutions? We shall not take up Watkins' answers to these questions for the simple reason that - as we shall see - the questions themselves are quite sufficient for the purposes of assessing Watkins' interpretation of Kuhn in this context.

4. We may next consider the line of thought which leads Watkins to claim that a new paradigm cannot emerge (even though a predecessor is in crisis) if Kuhn's *own* conception is adhered to, and to conclude that scientific communities are not (as Kuhn would have it) monolithic.

Watkins abstracts from Kuhn's theory the following claims (see Watkins (1970), pp. 34-35): (1) The scientist is always bound by some paradigm; (2) The scientist is always bound by *only one* paradigm[37]; (3) Successive paradigms are incompatible (the thesis of incompatibility); (4) The transfer of a scientist from one paradigm to another is a process extremely rapid and unequivocal, resembling a gestalt switch (or the D/R effect; cf. § 6, above). And because for the discoverer of a paradigm P the discovery of P and the adoption of the mode of thought it entails are one and the same thing, point (4) implies: (5) No paradigm has a prehistory; a paradigm is brought into being at once and *in toto*.

Watkins is of the opinion that (5) is not tenable on psychological if on no other grounds:

> "I do not know how much a single genius might achieve in the middle of the night, but I suspect that this thesis [5] expects *too* much of him." (*Ibid.*, p. 36)

Watkins goes on to draw a number of inferences (*ibid.*, pp. 36–37), which we may summarize as follows: (a) (1) $\wedge$ (2) $\wedge$ (3) $\Rightarrow$ (4); (b) (4) $\Rightarrow$ (5); (c) $\sim$ (5) (d) therefore [(1) $\wedge$ (2) $\wedge$ (3)]; (e) the incompatibility thesis (3) is valid; therefore (f) $\sim$ [(1) $\wedge$ (2)]; (g) theses (1) and (2) form a whole (6) which might be called the *monolith thesis,* i.e. (6) = (1) $\wedge$ (2); (h) thus $\sim$ (6); (i) thus "the scientific community is not, after all, a closed society whose chief characteristic is 'the abandonment of critical discourse'" (*ibid.*, p. 37).

5. Finally, attention should be drawn to one difficulty inherent in Kuhn's theory, of which Watkins gives the following formulation (Watkins (1970), p. 36): If two theories are incommensurable, they cannot compete and *are* compatible; and if they are incompatible, they are in fact comparable and *cannot* be incommensurable. (Be it noted that Kuhn has not adequately responded to this charge upon his theory.)

7.2. Popper

1. Watkins' critique of Kuhn (see 7.1., above) receives considerable weight in the literature, as Popper's comment

> "... this [Watkins (1970)] is a brilliant defence of my views of science against Kuhn's criticism. " ((1974b), p. 1195, note 202; italics added)

reflects. Since we are, on the basis of Chapter I, in a position to confront the views of Popper and Kuhn, and since the criticism of Watkins set out above represents a Popperian line of attack, we shall concentrate, in the case of Popper's critique of Kuhn's theory, on a number of quite specific lines of thought which - as we shall eventually see - are of prime importance and well representative of the chief attacks against Kuhn.

2. According to Popper, science is essentially critical and therefore revolutionary. Nevertheless, a certain measure of dogmatism is indispensable to it, for

> "[i]f we give in to criticism too easily we shall never find out where the real power of our theories lies." ((1970), p. 55)

Kuhn's brand of dogmatism, however, is for Popper something else entirely: Kuhn holds rational discussion to be possible only within the framework afforded by a paradigm, which gives rise to a thesis of *historical relativism,* whose nature is at bottom logical and according to which the Popperian method (bold conjectures and criticism) is not characteristic of science proper.

It will be clear from earlier contexts (see § 3) that Popper rejects such a conception. We may summarize the grounds on which he does so in Popper (1970) (see pp. 55-57):

(i) Truth is objective and absolute - relativism is thus an erroneous stand - and even if truth is never attained it can nonetheless be approached.

(ii) Although we are indeed always bound to some conceptual system, we can - with an effort - in principle break free of this system at any time; and although in so doing we may find ourselves equally bound by a new one, this is at least more "spacious" than its predecessor. What is more, there is no reason in principle why the old system of concepts could not be reconsidered from the standpoint of the new one.

(iii) This being so - to adopt the terminology Kuhn employs - successive paradigmatic theories are comparable; so that, finally;

(iv) The modern irrationalism, heralded by the (Kuhnian) notion of incommensurability, can be overcome.

3. In contrast to certain other scholars, Popper believes that normal science really does exist, and that it is important to take

account of its existence, because it constitutes a "*danger* to which I was blind before Kuhn opened my eyes" ((1974b), p. 1146; italics added).

We may condense Popper's views of normal science and of certain aspects of Kuhn's thought as follows ((1970), pp. 51-54, (1974b), pp. 1145-1147):

(i) There is such a thing as normal science, and its representative is a pitiable non-revolutionary and a not-too-critical professional, a student who accepts the prevailing dogma of the day, who does not wish to call this dogma in question, and who will only accept a new revolutionary theory when more or less everyone else is prepared to accept it - and especially if it becomes fashionable.

(ii) The normal scientist is poorly trained, or trained in a dogmatic spirit: "he is a victim of *indoctrination*" (Popper (1970), p. 53; italics added).

(iii) The normal scientist has learned techniques of application which may be employed without asking why they work: "As a consequence, he has become what may be called an *applied scientist*, in contradistinction to what I should call a *pure scientist*" (*ibid.*, p. 53). Success in normal science is *exclusively* a matter of showing that the prevailing theory can be appropriately applied in solving problems.

(iv) Normal science (the routine) is a relatively new phenomenon, whose significance was before the First World War but minimal, and which has become widely influential concomitant with the mass training of research workers. This in turn has arisen out of the present need for technologists.

(v) Kuhn (erroneously) projects this relative newcomer, normal science, which to himself is a real experience, onto the whole history of science.

(vi) But routine (normal science) is a danger:

> "We may soon move into a period where Kuhn's criterion of a science - a community of workers held together by a *routine* - becomes accepted in practice. If so, this will be the *end* [italics added] of science as I see it." (Popper (1974b), p. 1146)

And such a danger is not simply a danger to science, it is a danger "indeed, to our civilisation" (Popper (1970), p. 53).

(vii) Kuhn's approach to science is *psychological* and *sociological.* His definition of science is entirely lacking in the *rational* factor; for a scientific discipline to exist, it is sufficient that there be a community of enthusiasts in the field concerned who share a certain routine and whose task is puzzle-solving. A *lesser* misfortune to result from this (here we invoke an example other than Popper's) is that, let us say, Lysenko's biology would on these sociological criteria constitute science, and that on these criteria Lysenko's biology could quite legitimately be revived; however,

> "the *major* disaster would be the replacement of a rational criterion of science by a sociological one." (Popper (1974b), p. 1147; italics added)

7.3. Lakatos

1. How are we to decide whether an entity intended as a scientific theory is truly scientific? Is it possible to set up criteria for the scientificity of theories and, for example, for degrees of truth, and if it is, is it possible on the basis of these criteria to place theories (pertaining to the same sphere of phenomena) in orders of preference?

Let T_1 and T_2 be rival theories. According to Lakatos (see (1978)), we may distinguish three main types of answer to the foregoing questions:

(i) T_1 and T_2 are belief systems, neither of which is more correct or more erroneous than the other. Theories are simply beliefs, only it may be that one theory enjoys more favour (and influence) than another. This is the doctrine of *scepticism.*

(ii) There are in principle objective criteria for the scientificity of these theories, criteria which do not depend on the number of people who believe in them or are even familiar with them. Lakatos calls this mode of thought *demarcationism,* in whose framework it is generally held that in principle nothing prevents one from finding valuations or appraisal functions f, whereby T_1 and T_2 can be put into orders of preference. (Such valuations would, for example, comprise that indicating closeness to truth, those giving degrees of falsifiabilitity or corroboration, the probability of a theory and so on. Within demarcationism *contradictory* views may obtain as to the possibility of finding a *certain fixed* valuation.) The core of demarcationism lies, according to Lakatos, in the fact that theories, as an object of analysis, are representative of Popper's World III, not World II[38].

(iii) The scientificity of the theories T_1 and T_2 can be recognized, but there exist no generally valid objective criteria for it; the scientific elite are the deciding authority in this matter: T_2 is superior to T_1 if the scientific community in question prefers T_2. Such an approach Lakatos names *elitism.*

In elitism one passes from analyzing the entities in Popper's World III (propositions, theories as abstract entities independent of their creators etc.) to an analysis of those in Popper's World II (beliefs, mental states, crises of the scholar or of the scientific community and so on). Elitism further restricts the right to participate in the evaluation of scientific results expressly to accepted and authorized members of the scientific community. Since there are no universal objective criteria for such evaluations, the view of the community is all that matters. But how are possible differences of opinion *within* the community to be resolved? The answer is that such discrepancies cannot normally arise by reason of the consensus attained. If for some reason or other disagreement does arise, such disputes are also settled because the fittest will survive; when giants match their strength, outsiders can but await the outcome and after the battle acclaim the winner as a champion of scientific progress.

2. We may next consider the way Lakatos inserts Kuhn's theory into his characterization of the concept of elitism (see Lakatos (1970), (1978)).

According to Lakatos, Kuhn's theory represents an irrationalism whose basis is the *consensus theory* of truth: what is held to be true is true. The paradigm determines what is valid (for example, what is suited to explain something and what is not), and on the other hand the transfer from one paradigm to another is a mystical event resembling a conversion

> "... which is not and cannot be governed by rules of *reason* and which falls *totally* within the realm of the (social) psychology of discovery. Scientific change is a kind of *religious* change." (Lakatos (1970), p. 93; original italics removed, new added)

This being so - thus Lakatos argues - a scientific revolution in the Kuhnian sense is an abnormality belonging in fact outside the sphere of science, and the shift from criticism to committed scientific activity is the point at which the progress of science (normal science) commences (*ibid.*, pp. 92-93).

But if - thus we feel Lakatos' line of thought may be condensed -

(i) *criticism* is prohibited in normal science, which is bound to standards, and

(ii) *standards* are prohibited in scientific revolutions, which involve a quasi-religious conversion, this would imply that, in the terms of Kuhn's theory, there is no possibility whatever of carrying out a rational reconstruction of the growth of science: all one can do in the absence of objective, universally valid criteria is to give psychological, socio-psychological and sociological explanations for scientific change (as one does in the psychology and sociology of religion). This, however, implies that in the last resort the basis of truth is power:

> "If even in science there is no other way of judging a theory but by assessing the number, faith and vocal energy of its supporters, then this must be even more so in the social sciences: *truth lies in power*." (*Ibid.*, p. 93; italics added)

3. By adding an item on Kuhn to Watkins' comment on the views of three notable philosophers regarding the growth of science, Lakatos obtains the following "four-sector" scheme ((1970), pp. 177-178):

The growth of knowledge is

(a) according to Hume inductive and irrational;

(b) according to Carnap inductive and rational;

(c) according to Popper non-inductive and rational;

(d) according to Kuhn non-inductive and irrational.

Kuhn's irrationalism is not associated solely with revolutions and the unreasonable tolerance of falsification in normal science, for a crisis, too - as a result of which a theory may be rejected (not falsified) - is an irrational event:

> "There is no particular *rational* cause for the appearance of Kuhnian 'crisis'. 'Crisis' is a psychological concept; it is a *contagious panic*." (*Ibid.*, p. 178; italics added)

Hence, in Lakatos' view, study of scientific development and progress passes completely into the sphere of psychology (territory in Popper's World II), in such a way, however, that Kuhn's theory acquires a new feature in respect of the appraisal of theories:

> "[W]e have to study not the mind of the individual scientist but the mind of the Scientific Community. Individual psychology is now replaced by social psychology; imitation of the great scientists by submission to the collective wisdom of the community." (*Ibid.*, p. 179)

In keeping with the elitist conception the maxim of the scientific community must ultimately be

> *"Do your Master's thing"*
> (Lakatos (1978), p. 117)

4. It is of prime importance for our present concern to note that Lakatos also regards the emergence of a scientific crisis as an irrational event, and, if we add to this the other factors in his reading of Kuhn, it is not to be wondered at that he devises the following thought experiment to characterize Kuhn's theory (Lakatos (1978), p. 116). Let us by way of example consider astronomy and suppose that the following takes place:

(i) Astronomy progresses over a certain period of time on both objective and Kuhnian criteria.

(ii) At a certain point in time, however, astronomers are seized with the sense of a Kuhnian crisis.

(iii) As a result of this crisis all astronomers experience a conversion, abandon astronomy and, by an irresistible D/R effect, become astrologers.

According to Lakatos, an analysis of (i) - (iii) on Kuhn's terms gives the following: (1) a crisis; (2) mass conversion; therefore (3) an ordinary revolution. Note further Lakatos' emphasis in this would-be Kuhnian analysis of a revolution: "Nothing is left as problematic and unexplained" (*ibid.*, p. 116).

7.4. Feyerabend

> "... I am fortified in my behalf by the fact that almost every reader of Kuhn's *Structure of Scientific Revolutions* interprets him as I do..." Paul Feyerabend ((1970a), p. 198).

1. Feyerabend considers Kuhn's theory a danger to culture in that, according to Kuhn, the scientific community can adhere to only one paradigm (within a given field of research): this is not only likely to work to the detriment of the growth of knowledge, it would also seem to make Kuhn's theory an ideology legitimizing the narrowest and most complacent specialization "bound to increase the anti-humanitarian tendencies which are such a disquieting feature of post-Newtonian science" (Feyerabend (1970a), pp. 197-198).

Because in certain connections Kuhn's theory has been taken to imply that a less developed discipline could be made into a mature science by restricting criticism, reducing the number of extensive theories in it to one, preventing the emergence of rival theories, creating a monolithic normal science with the pertinent sole theory as its paradigm, preventing students from experimenting with different approaches, calling restless colleagues to order and so on, Feyerabend asks: Is Kuhn's theory intended to be normative, directive? In answer to his own question he suggests that Kuhn's approach is evidently *deliberately* ambivalent, so that his theory may be read as either *descriptive* (giving an account of facts) or *normative* (offering methodological recommendations); thus Kuhn - craftily - *on the one hand* seeks to imbue value-judgements, which are usually considered subjective, with a firm and objective historical foundation, while *on the other* he offers to those who do not like the idea of deriving values from facts the possibility of reading his theory with the full assurance that no such value derivation has taken place and that his theory is pure description (which in no way implies that the matters described are at all worthy of following up). (On the foregoing see *ibid.*, pp. 198–199.)

2. If we suppose that Kuhn's theory is no more than a descriptive account of influential historical events and institutions, then the puzzle-solving tradition - so Feyerabend thinks - comes to constitute what *de facto* distinguishes science from other activities. But here, he points out, one encounters difficulties which he characterizes as follows. Consider, he says, organized crime specializing in safe-breaking. How would one describe the activities of a group engaged in this mode of crime? The procedures they adopt would doubtless include the following:

(i) The amount of basic research (e.g. theory of explosives, electricity) is kept at a minimum.

(ii) Failure tests the members of the gang, not the procedure used (not, for example, the theory of electricity): failure is blamed not on the equipment but on the practitioner.

(iii) Faced with sufficiently strong pressure (new, unfamiliar security devices), efforts are made to overcome anomalies (repeatedly unsuccessful attempts at burglary) until the situation clears up with the discovery of a revolutionary method of breaking in.

If, then, Kuhn's theory is mere description, the result according to Feyerabend is that it permits classification of the most peculiar activities as science, and this is absurd. (On the foregoing, see Feyerabend (1970a), pp. 198–201.)

3. In the following we shall refer to the Kuhnian pattern of the development of science (normal science/crisis/revolution/new normal science) as the *Kuhn procedure*. Let us next consider how Feyerabend analyzes the consequences of the view whereby normal science is in the Kuhn procedure an indispensable condition for revolution (the *functionality argument)*.

Feyerabend divides this argument into two claims:

(i) The paradigm is an indispensable conceptual requisite in high-level research.

(ii) Research must be based on one single paradigm.

According to Feyerabend (i) holds true in the sense that advanced research is impossible without a directive conceptual framework. But this idea - Feyerabend stresses - is nothing new. He maintains that Kuhn justifies (ii) by appeal on the one hand to *actual* research practice and on the other to the fact that it is precisely the strict adherence to a single paradigm which leads (in time) to the rejection of this paradigm, in other words to scientific revolution. The former argument is in keeping with a descriptive reading of Kuhn's theory, while the latter derives from a normative reading: in the Kuhn procedure strict adherence to a single paradigm is a *sensible and recommended* approach.

The normative defense of the monoparadigmatic procedure is for Feyerabend acceptable if revolutions are desirable and if the way in which Kuhn's normal science leads to them is desirable. Apart from these two points, Feyerabend suggests, one must thirdly ask, when applying a descriptive reading of Kuhn, whether normal science really exists (in the Kuhnian sense). (On this section see Feyerabend (1970a), pp 201-202.)

4. Are revolutions desirable? In Feyerabend's view their desirability can hardly be demonstrated in terms of Kuhn's *own* theory, for we can hardly say that a post-revolutionary paradigm is *better* than its predecessor, because by reason of their incommensurability no such comparison is possible.

Is then the *way* that adherence to a single paradigm ultimately produces revolution something to be desired?

The *principle of tenacity* means the following: Let $\{T_1, \ldots, T_n\}$ be a set of theories. Choose among them the one that looks most promising, and develop it, in spite of counter-examples, as far as it can be taken.

This principle is for Feyerabend justifiable, since theories can be developed and modified over time.

Once the principle of tenacity is accepted, a given theory T cannot - usually - be eliminated except by exploiting other theories T', T'', ... , which give promise of a solution to anomalies encountered by T. At this point the tenacity principle in fact demands rejection of T. Thus: if change of paradigm is the objective, it is best attained by articulating, concomitant with th development of T, its alternatives T', T'', ... This latter principle Feyerabend calls the *proliferation prinçiple,* and it constitutes one means of hastening revolutions; it is also a ration method.

In Kuhn's theory proliferation is connected with science in crisis, but as Feyerabend sees it, Kuhn is in fact saying that revolutions occur along the lines of the tenacity-proliferation sche described above. According to Feyerabend, the development of scie in such a scheme is possible; it is, moreover, to be desired, bec it accelerates revolutions and does not lead to dangerous monolithicity. Thus the way the Kuhn procedure promotes revolutio is not desirable, and it should be replaced by the tenacity-proliferation scheme.

Finally, Feyerabend goes on to claim that the development o theory T simultaneously with its rivals is also a historical fact, whereas Kuhnian normal science can scarcely exist (in reality). T dialectics of tenacity-proliferation is thus both a fact and a desideratum - so Feyerabend thinks - and this dialectics ensures the one hand the critical aspect of the growth of knowledge (a po Popper stresses), and on the other sufficiently prolonged developm of a fixed theory (Kuhn's point of emphasis). (On the foregoing, Feyrabend (1970a) pp. 202-213.)

§ 8. SHAPERE

1. Dudley Shapere is of the opinion that Kuhn (POST), Kuhn (197(and Kuhn (1970c) represent a decisive withdrawal from the stand their writer took in STR towards the conventional position (see Shapere (1971)). We shall therefore first take up Shapere's critic of STR and thereafter his conceptions of Kuhn's alleged retraction

2. We may condense the main points in Shapere's critique o STR in the following (see Shapere (1964) and (1971)):

(i) It is indeed true that one finds in the history of science various clusters of commitments or guiding principles to which scholars in certain branches of science have adhered in certain periods. In this respect Kuhn is right.

(ii) It does not, however, follow from (i) that the paradigm theory must be introduced in order to explain this. (There exists a completely traditional and conventional view which admits the directive function of a basic set of concepts in research.)

(iii) Kuhn's mode of exposition makes it difficult to believe that his stock of concepts can be derived from what has really happened.

(iv) According to Kuhn a paradigm cannot, on the one hand, be exactly described in words, but on the other he takes it for granted that the identification of a paradigm is easy. It is precisely here that the serious deficiencies of his theory are discerned: (a) identification of the paradigm becomes *too* easy, for Kuhn appears to find them almost anywhere; yet (b) in the cases he does find he cannot say what exactly *is* the paradigm.

(v) Most likely the paradigm concept covers anything and everything which allows the scientist to do something. Here Kuhn's basic thesis, that every scientific tradition is guided by a paradigm, seems to become void, in which case his theory loses much of its significance. (For one thing, the distinction normal vs. extraordinary science would lose the clear-cut demarcation which Kuhn's theory requires of it.)

(vi) There would not appear to be any objective grounds for choice of paradigm, since there are no pure facts, and paradigms are incommensurable; but in this case

> "... there can be no good reason for accepting a new paradigm, for the very notion of a "good reason" has been made paradigm-dependent." (Shapere (1971), p. 707)

The result is a chaos of scientific relativism, in which the objectivity and progress of science as traditionally conceived are entirely abandoned. Nor does Kuhn's relativism stop at this:

> "... not only is there no means of *rationally* assessing two competing paradigms; there is no way of comparing them *at all,* so different is the world as seen through them (or ... so

different are the *worlds* they define)." (*Ibid.* p. 707; italics added)

(vii) Kuhn's theory is tantamount to idealism, for the paradigm seems even to give existence to its target area (cf. § 6, above; see point (vi), above).

3. At an earlier point (see § 5) we inserted into the paradigm the set of values V, whose problematics play a central role in the withdrawal Shapere claims Kuhn has made from his STR stand. We may briefly consider here the nature of the constellation of values which give direction to research.

In POST, Kuhn analyses the criteria which function as *values* directing research activity and choice of paradigm; these include the following: (a) accuracy of prediction by the theory; (b) balance between level of abstraction and everyday experience; (c) the number and nature of problems the theory solves; (d) the simplicity of the theory; (e) the scope of the theory, which should extend beyond its original scope of application; (f) the compatibility of the theory with other areas of knowledge (Kuhn (POST), pp. 185-186, 205-206). On the basis of a constellation of such values as these the scientific community makes its choice of paradigm. Conflict of values may of course arise in concrete situations (for example, values (a) and (e) may easily give a different preference-order for two theories), and various individuals may apply the constellation in different ways: there is no universal algorithm (see also Kuhn (1973)). - We pass now to a consideration of Shapere's claim that Kuhn's position has altered markedly in writings subsequent to STR, that is, in Kuhn (POST), (1970b) and (1970c). As noted, the above-mentioned value constellation occupies a central position in the retraction Shapere alleges Kuhn to have made.

4. We may summarize these claims of Shapere's regarding a far-reaching change in Kuhn's thought (see Shapere (1971)):

(i) A change takes place in the paradigm concept when Kuhn introduces his notion of a disciplinary matrix. In STR the paradigm was a holistic entity. Does Kuhn now wish to claim that each component in the matrix has its own separate and discrete function?

If so, Kuhn would seem to be approaching the *completely traditional* idea that presuppositions affect both scientific evaluation and activity.

(ii) In replying to the charge of relativism, Kuhn backs out most conspicuously from the extreme position he took up in STR, for now (a) problems are posed by nature, not by the paradigm; (b) there is apparently an objective world (which presents these problems); (c) in the matter of choice of paradigm one is not, after all, entirely bound by the criteria dictated by the paradigm, because in the choice situation one may appeal to a constellation of criteria having the function of values (see preceding section); thus what is involved would not appear to be

> "... a relativist's position; but it is a far cry from Kuhn's first-edition attack on the view of scientific change as a linear process of ever-increasing knowledge ... . It is, in fact ... a *long step toward a more conventional position in the philosophy of science* - one that makes a distinction between the "given" and the "interpretation" (or "theory") and holds that the latter are adequate to the extent that they account for the former." (*Ibid.*, p. 708; italics added)

(iii) In his new phase Kuhn seems to acknowledge the existence of objective factors (cf. for example, matters connected with the value constellation; "nature cannot be forced into any matrix whatsoever"), but this is not in keeping with certain views he has previously expressed and to which he still adheres. One cannot, namely, reconcile with the demand for objectivity the fact that in the matter of values this application of rational grounds is not subject to any limitations: if, in the choice between two alternative theories, two scientists arrive at different decisions because they emphasize different elements in the value constellation, *neither can be held in error*. Of such a stand on the philosophy of science one can but say:

> "It is a viewpoint as relativistic, as antirationalistic, as ever." (*Ibid.*, p. 708)

(iv) In his new position, Kuhn - in contradiction to his earlier stance - allows communication between paradigms. But it is then inconsistent with this that he still insists on his thesis of incommensurability.

(v) Kuhn thus appears to withdraw from his extreme position on certain essential points and to move towards a completely traditional and conventional philosophy of science. Yet he preserves so many of his earlier views that the end result is incoherent.

(vi) In spite of his inconsistent presentation, and in spite of his efforts to revert to the ordinary mode of approach, Kuhn nevertheless seems to take the *latter* of the two following possible stands:

> "Do scientists ... proceed as they do because there are objective reasons for doing so, or do we call those procedures "reasonable" merely because a certain group sanctions them?" (*Ibid.*, p. 709)

This implies that Kuhn takes the sociological view, which in turn means not only abandonment of objectivity and rationality in science, and acceptance of relativism, but also this: the distortions of objectivity and rationality are core elements of science in the Kuhnian view.

§ 9. TOULMIN

1. Stephen Toulmin distinguishes five stages in those aspects of Kuhn's production which concern the present inquiry. Of these stages, STR comprises the third.

The core of Toulmin's analysis and criticism of STR is this (see Toulmin (1970) and (1972), pp. 98-121):

(i) In the scientific revolution P/Q, such a thorough conceptual change takes place that the borderline between P and Q cannot be crossed by rational means, so that their respective representatives cannot hope to understand each other.

(ii) Every individual sees the world according as his own *Gestalt* organizes it: for example, what a person sees in, say, carrying out an experiment depends not only on the structure of his eyes and his instruments but also on the particular paradigm he operates with;

hence

> "... tacit reliance on an *idealist* theory of knowledge encourages Kuhn to accept an idealist theory of perception also." (*Ibid.*, p. 101; italics added)

(iii) Idealism entails the Kuhnian chaos of scientific relativism: "Paradigms are sovereign; they make their own laws" (*ibid.*, p. 102).

(iv) According to Kuhn, there can be no communication whatsoever between two successive competing traditions, because a common language is lacking by means of which theoretical discussion could be conducted: no procedure exists for the comparison of results achieved in different traditions. The example of history, however, proves otherwise: the more remarkable the scientific change has been, the more thorough and protracted has been the debate on it (in other words, the radical discontinuity Kuhn alleges simply does not obtain). For example, the Copernican revolution was the outcome of a prolonged critical exchange of opinions:

> "If the men of the sixteenth and seventeenth centuries changed their minds about the structure of the planetary system, they were not forced, motivated, or cajoled into doing so; they were given reasons to do so. In a word, they did not have to be *converted* to Copernican astronomy; the *arguments were there to convince them.*" (*Ibid.*, p. 105; italics added)

(v) The nucleus of Kuhn's analysis is the theory of scientific revolution, but since STR this theory has undergone far-reaching changes.

2. According to Toulmin, Kuhn's (pertinent) work can (as noted above) be divided into five stages reflecting the shift of position in his thinking. We may condense Toulmin's view as follows (see Toulmin (1970), (1972), pp. 107-118):

Stage 1 comprises Kuhn's work *The Copernican Revolution*, in which he speaks of scientific revolutions only in a descriptive sense.

Stage 2 comprises his treatise of 1961 (referred to in the source list of the present work as Kuhn (1963b)). In this phase the paradigm is compared to a system of dogmas governing a religious order, and correspondingly a change of paradigm, or scientific revolution, involves a conversion that may be compared to a change in the articles of a religion.

But the comparison of a paradigm to a set of dogmas - thus Toulmin - proves groundless when we distinguish the inherent intellectual authority of a well-founded theory from the external authority of an individual scientist representing this theory. For example, the main tenets of Newton's *Principia* functioned as the ultimate theoretical basis of evaluation in the tradition they created (in matters such as what problems were to be considered meaningful); in this sense the *Principia* was paradigmatic, but in no way did it assume the nature of a dogma: on the contrary, it was the weight of its intellectual content which induced scholars to adopt it. There are, on the other hand, cases where the personal aura of an outstanding scientist has imbued his ideas with the power of a dogma even though the intellectual content of his theory has long since lost its significance (as happened, for example, to Newton's *Opticks)*. Here one may well speak of a dogma, but only on the sociological level. This being so - says Toulmin - the fact that research is based on a paradigm does not *in itself* involve anything dogmatic. For Toulmin, the point is that

> "we must take care to respect the distinction between the two corresponding kinds of authority - the *intrinsic intellectual authority attaching to a well-established conceptual scheme* and the *magisterial or institutional authority exercised by a dominant individual or school.*" ((1972), p. 111; italics added)

Thus Kuhn's claim that scientists adhere to paradigms in a dogmatic spirit is a consequence - so Toulmin appears to think - of a conceptual confusion (or at best his claim is a rhetorical exaggeration). But if one accepts this point, one must ask whether there are any grounds whatever for seeing a change of paradigm as a revolution in the Kuhnian sense.

Stage 3 brought STR, which makes no reference to a comparison between paradigm and dogma. In other respects, however, STR is conceived along the same lines as the paper in Stage 2, and suffers from the same obscurities. In STR Kuhn stubbornly adheres to his sharp contrast "normal" vs. "revolutionary".

Stage 4 consists of the papers Kuhn wrote between 1965 and 1969 in reply to criticism of his theory.

In the years 1962-1965 Kuhn's basic conceptions were most intensively called in question: Has scientific change ever been as revolutionary as Kuhn's basic categories assume?

As a result of the debate on this and allied questions Kuhn's stance had by 1965 undergone a considerable change: he began now to concentrate on cases in which the conceptual shift was not so drastic as the previous theory had envisaged. This Kuhn was obliged to do, because closer study had shown so-called scientific revolutions to have been in reality less revolutionary than the Kuhn procedure would entail.

Faced with these difficulties - Toulmin suggests - Kuhn could have withdrawn in a more conventional direction, only instead, by 1965,

> "he was conceding that his first distinction between 'normal' and 'revolutionary' change in science might have been too sharply drawn; but he was arguing, in reply, that scientific revolutions were in fact, not less frequent, but more frequent than he had previously recognized." ((1972), p. 113)

Thus Kuhn proceeded to describe theoretical change in science in terms of an endless series of lesser upheavals which might be called *microrevolutions*.

In so doing, however, Kuhn gradually comes to abandon the very basis of his theory, the contrast between normal science and scientific revolution, or between scientific change taking place *within* normal science and scientific change involved in a *change of paradigm*, which contrast is step by step transformed into the following logical distinction by dividing scientific claims into two parts:

(i) Those scientific arguments which do not involve conceptual or theoretical changes, and which thus can be set out in terms taken from formal logic.

(ii) Those scientific arguments which introduce conceptual or theoretical innovations, and which cannot therefore be set out in accordance with (i).

Ultimately Kuhn's theory leads, via the changes entailed in Stage 4, to a situation which Toulmin describes as follows:

> "And, since every genuinely theoretical change in science involves a conceptual innovation of some kind - *however minor* - all genuinely theoretical changes clearly involve, in some measure, arguments of the second 'revolutionary' kind." (*Ibid.*, italics added)

Stage 5 is constituted by two articles appearing in 1970 (in the References, Kuhn (POST) and (1970c)), in which finally the difference between "normal" and "revolutionary" is altered so as to become the following distinction:

(i) *Propositional* changes, which do not involve conceptual innovations, and which are justifiable by means of some mode of deduction or quasi-deduction.

(ii) *Conceptual* changes, which go beyond the bounds of mere deductive procedures.

Seen in this light, however, there is in every scientific change something "normal" and something "revolutionary". If nothing more is said in Kuhn's theory, then this theory indicates in a completely trivial manner the logical aspects involved in all theoretical change:

> "On his latest reinterpretation, Kuhn's account of 'scientific revolutions' rests on a logical truism and - as such - is *no longer a theory of conceptual change at all*." (Toulmin (1972), p. 117)

EPILOGUE TO CHAPTER THREE

As has emerged in the foregoing chapter, the criticism levelled at Kuhn's theory is unusually violent even for philosophical debate. When such noted scholars are thus roused - and we shall see that the campaign continues extensive and unabated to this day - the reasons are to our mind to be sought on one of the following two quarters: either

(i) Kuhn's theory is erroneous, only for some reason it has attained unusual vogue. The detrimental influence of this trend the scientific community is concerned to eliminate and to restore affairs to their rightful order; or

(ii) Kuhn's theory adumbrates a breakthrough in the philosophy of science on a scale sufficient to shake the very foundations upon which its critics stand. At the whiff of danger they are thus up in arms.

The present writer is inclined to seek the explanation rather in the latter direction: by way of *articulating* our interpretation of Kuhn (given in Chapter IV, below) we shall in Chapter V show that the criticism of his theory which we systematically set out in this Chapter III is less than fully warranted.

CHAPTER IV
AN INTERPRETATION OF KUHN'S THEORY

According to Kuhn's theory, the task of normal science is the *actualization* of paradigms, a process to which STR devotes an entire chapter, and which receives a clear and concise formulation already in Kuhn (1961). The concept of actualization is, or can be reconstructed as, the flagship of the paradigm view as to the core of normal science, and surprisingly enough, of the incommensurability of theories. By implication, then, the process of actualization is constitutive not only of normal science, but of scientific revolutions, too. This entails that the paradigm view cannot but be unintelligible from such a point of view which does not possess an adequate insight into the structure and nature of normal scientific actualization work. This being the case, it is somewhat odd to find that the literature on Kuhn, as far as we know, either entirely overlooks the matter (at least in Kuhn's original sense) or mentions it as it were only passing. Even the few exceptions of which we are aware and which mention actualization in Kuhn's sense do not reach the core of Kuhn's conception of it[54].

In the present chapter our own interpretation of Kuhn's theory is given and his view is thus confronted with the traditional view of science. In order to effect this we clarify the paradigm concept by means of examples and then scrutinize the concept of actualization. Some of the anti-Kuhnian themes of Ch. III are evaluated at the end of this chapter (followed later in this work by a systematic analysis of Kuhn's critics based on our interpretation of the paradigm view).

§ 10. THE DEVELOPMENT OF SCIENCE: PROGRESS BY REDUCTION OR REVOLUTION?

10.1. Textbooks and the Accumulation Theory of Knowledge

1. Kuhn considers the conception of science conveyed by textbooks in the various disciplines (in other words their implicit metatheories) to be erroneous in that, among other things, their content rests upon the assumption of the cumulative nature of knowledge. *If* we thus accept as valid the claim - and Kuhn, while perhaps the most prominent, is by no means the only proponent of it - that development and progress in science take place in the form of revolutionary breakthroughs whereby, contrary to what the textbooks used imply, part of the body of knowledge hitherto acquired must be abandoned without allowing to construct the abandoned theories as special cases of present-day knowledge[39], *then* the source of error in the conception of science the textbook tradition propagates must also be acknowledged to constitute an interesting culturalphilosophical and epistemo-sociological problem associated with the philosophy of science. Though we cannot within our present scope go into this problem, a number of comments will nevertheless be appropriate if only because this image of science implicit in the (relatively recently established) textbook tradition is after all a reflection of the theory of science which scholars in the empirical sciences have (often tacitly) accepted - for we can scarcely imagine textbooks to have been deliberately devised to inculcate erroneous knowledge.[40]

By way of illustrating this mode of presenting developments in scientific knowledge and concept formation as a cumulative process, we may consider an excerpt from a Finnish university textbook in the field of chemistry:

> "The Greek ARISTOTLE ... had already adopted the concept of element ..., [which,] it is true[, does not] correspond to the concept as we know it, but the basic idea of the composition of matter was in the presentation [of his concept] already correct. Our *current* concept of element was propounded by R. BOYLE ... in a form *that* [since] has only been more precisely formulated ..." (Antikainen (1962), p. 19; italics added, translation by the present author).

The conception of science as touching empirical theories (the metatheory of science) is mediated chiefly by textbooks in the various fields of research, for example physics (implicit propagation) and the various philosophies of science (explicit propagation). One mode of approach to gain considerable prestige in the philosophy of science in the present century has been to consider empirical theories as logico-linguistic entities (see earlier parts of this work). In keeping with this, the textbooks respectively envisage a static situation in which they may review what is accepted science by *present-day* standards. This latter circumstance - one which cannot be called in question - means that in textbook tradition (a) the revolutionary bases of current conceptions are forgotten; (b) the textbooks in any given field must be partly rewritten after any notable scientific breakthrough in that field; and (c) the truncated version of the history of science passed on to researchers must be "appropriately" amended from time to time (Kuhn (STR), pp. 136-137). Setting out in this manner from a perusal of accomplished theories, the achievements of earlier scientists are subsumed in current conceptions so that "students and professionals come to feel like participants in a long-standing historical tradition" (*ibid.*, p. 138). The motive for the repeated rewriting of history is surely at least in part to be found in the following:

> "The objective of a textbook is to provide the reader, in the most economical and easily assimilable form, with a statement of what the contemporary scientific community believes it knows and of the principal *uses* to which that knowledge can be put. Information about the *ways* in which that knowledge was acquired ... and ... enforced on the profession ... would *at least* be *excess* baggage. Though including that information

> would almost certainly increase the *"humanistic"* values of the text and might conceivably breed more flexible and creative scientists, it would inevitably detract from the *ease* of learning the contemporary scientific language." (Kuhn (1961), p. 167; italics added)

We shall not pursue the question whether the tendency to write textbooks in the accumulative spirit is essentially connected with the emphasis on technical considerations (or the depreciation of humanistic values[41]) in our culture, and without going into those and certain other of the problems the textbooks in various fields entail for the historiography and philosophy of science and the philosophy of culture[42], we may, in respect of the afore-mentioned textbook assessment of Boyle's status,conclude this section with a brief reference to Kuhn's stand (cf. that of Antikainen, above):

> "... Boyle offered it [his "definition" of an element] *only in order to argue that no such thing as a chemical element exists*; as a *history,* the textbook version of Boyle's contribution is quite mistaken. That mistake, of course, is trivial ... . What is not trivial, however, is the *impression of science fostered when this sort of mistake is first compounded and then built into the technical structure of the text.* Boyle's definition, in particular, can be traced back at least to *Aristotle* and forward through Lavoisier into modern texts. Yet that is *not* to say that science has possessed the modern concept of an element since antiquity." ((STR), p. 142; italics added)

2. The intuitive accumulation theory of knowledge, inherent in textbooks, may be variously explicated, as we have seen. In those technical reconstructions of the development of science the basic idea has a parallel in textbooks because in these reconstructions, too, the earlier components of scientific knowledge are seen from the point of view of present-day achievements: the old theories we must be able to reconstruct as special cases of the present-day ones, if the old theories are to be scientific at all. This implies that in the view of many accumulation theorists, for example some theories of the movement of solid bodies, now rejected, are not only unscientific, but even childish.

In what now follows we will consider in the light of an example how Kuhn's theory diverges from different accumulation theories.

10.2. An Example of Scientific Change in the Development of Science

1. It is a common conception that "*correct*" knowledge and study of nature (in contrast for example to the Aristotelian tradition) was - with a few notable but isolated exceptions - established by studies of the motion of solid bodies initiated chiefly in the XVIth and XVIIth centuries, in particular by the heroic endeavours of Galileo and Newton. Further, the core of this "true" study of nature is seen to lie in empiricism, in the experimental method and inductive approach (knowledge obtained by which means need but be corrected and adjusted in the wake of new scientific discoveries).

"We must consult experience in diverse cases and circumstances until we can *derive* from them a general rule. Although nature sets out from cause and ends with experiment, we must proceed in the opposite direction, in other words, we commence with experiment and thereby investigate causes" - the words of Leonardo succintly crystallize the common approach to modern natural science in general and for example to Galileo in particular in post-VIIth century conceptions of science, textbook tradition and the historiography of science. We referred above to the multiple osmosis obtaining between the metatheoretical ideas thus (usually implicitly) mediated and certain explicit conceptions maintained in the philosophy of science. Now we must ask whether really

(i) in contrast to the true Galilean construal, for example the conception of nature of the Aristotelian tradition was speculative, myth-ridden and evasive of empirical experience; and

(ii) the Galilean breakthrough was possible precisely because Galileo - in contrast for example to Aristotle - had the sense to consult *experience* (in other words to experiment) and *derive general rules (laws) from his observations;* and therefore whether

(iii) the thought of Galileo represents true science whereas that of his predecessors - with a few remarkable exceptions - was myth-making.

2. A. Koyré states that

> "[M]odern physics, which, in my opinion, is born with, and in, the works of Galileo Galilei, looks upon *the law of inertial motion* as its basic and fundamental law." ((1968), p. 2; italics added)

This so-called *law of inertia* (Newton's first law) says simply that every body perseveres in its state of rest, or of moving uniformly in a straight line, except in so far as it is compelled by impressed forces to change its state. Koyré argues that this law *seems* to us moderns completely self-evident by referring to the following trivial train of thought (*ibid.*, p. 3). If an object is at rest it will remain in its place and not move elsewhere "of its own accord". If, again, an object is in steady linear motion it will continue thus, in the same direction and at the same speed, ad infinitum (provided no intervening forces are involved), for we can see no reason why it should alter either, its direction or its speed.

But is the law of inertia as natural and completely self-evident (trivial, even) as anyone who endorses the conceptions of modern (classical) physics is likely to assume?

An answer at once presents itself if we turn, for example, to the following two points.

In the first place the historical facts would indicate (a) that the law of inertia is of very recent origin if measured against the yardstick of the history of science; (b) of known history by far the greater part has elapsed believing in laws quite different from that of Newton, which those preceding ages would have considered erroneous; (c) in the initial stages of modern (classical) physics those who in some sense apprehended this law encountered considerable difficulties in formulating it; and (d) the prominent figures in the history of science prior to the days of Newton were unable to formulate his law (see *ibid.*, pp. 2-15, Cohen (1974), pp. 315-328).

Secondly, the relation of the law of inertia to the factors generally upheld as the basis of the Galilean mode of thought, i.e. the relation of this law to observations, experiments and inductive thinking, is particularly problematic, because the motion the law postulates is absolutely impossible, as it is easy to see.

'Trivial', then, is in the light of the above by no means an appropriate epithet for Newton's first law.

The reason why this law strikes those accustomed to the conceptions of modern (classical) physics as self-evident is - as Koyré points out - that it constitutes part of an entire internalized conceptual system (see *ibid.*, p. 3); taken as a singleton it is far from being self-evident (or derivable from experience), and if we place this law in the context of Aristotelian physics we find it to be not only not self-evident but, as a view, a downright absurdity. Here we have the important precept of the philosophy of science whereby fundamental relationships must not - indeed cannot - be introduced in science as isolated laws; this must proceed by way of entire conceptual systems. Thus the development and progress of science in a given phenomenal sphere may be seen as the joint result of two distinct processes: in the normal science phase new discoveries of a factual or law-like nature are made within the framework of the existing system, while in scientific revolutions the conceptual apparatus (paradigm) is formulated and introduced whereupon post-revolutionary accumulation of knowledge may rest until a subsequent breakthrough reiterates the process - or again with Koyré[43]:

> "This ... enables us to understand why discovery of such simple and easy things as, for instance, the fundamental laws of motion ... has needed such a tremendous effort - and an effort which often remained unsuccessful - by some of the deepest and mightiest minds ever produced by mankind: they had not to "discover" or to "establish" these simple and evident laws, but to work out and to build up the very framework which made these discoveries possible. They had ... [1] to *reshape and to re-form our intellect itself*; to give to it a series of new concepts [2] to evolve a new approach to being, a new concept of nature, [3] a new concept of science ..." (*Ibid.*, p. 3; italics and numbering added)

Kuhn's relation to Koyré's views becomes clear if for example we observe that the above might be expressed in Kuhnian terms, thus [1]' In the scientific revolution which eliminates the paradigm P_1 we pass on to a new paradigm P_2; [2]' the world of this new paradigm contains different entities and is governed by different laws from those obtaining in P_1, and the facts accepted under P_2 are not those accepted under P_1; and [3]' the corresponding science is now on the basis of P_2 defined differently from its predecessor.

3. To what manner of conceptual entity does the law of inertia belong? Following Koyré we shall in this section consider certain aspects of the system in question (see Koyré (1968), esp. Ch. I).

Among the writings of Galileo we find the following statement: "Two things [are] required for motion to be perpetual; unbounded space and ..."[44]. So, *first,* to the new way of thinking space was infinite, and it is precisely this assumption which makes it possible to postulate the law of inertia[45]. *Secondly,* the new (classical) physics is mathematical in approach: the real world is appraised from a mathematical standpoint; in this conception real space is identified with the homogeneous infinite space of Euclidean geometry. *Thirdly,* motion is a pure geometric transfer from one point to another: motion *per se*[46] has no effect on the object moving, this being indifferent with regard to rest or motion in the sense that, taken as a singleton, it cannot have the predicate of either: a body is in motion or at rest only in relation to other bodies in a fixed coordinate system. *Fourthly,* motion is a state, and rest is another state: hence in this conceptual framework both are logically on the same level. For a given state of a body (rest or uniform linear motion) to change, *force* is required, and thus, *fifthly,* the law of inertia "becomes self-evident". And a *sixth* point is this: a given motion of an object does not interfere with any other motions that body happens to be executing simultaneously: (a) a body may be in any number of states of motion at one time, which states (without interfering with each other) produce a given result (the actual course of motion within a fixed set of coordinates) according to purely geometric rules; and conversely, (b) a given motion can by the same geometric rules be resolved into its original component motions.

4. Newton's own formulation of his second law in the *Principia* is (when rendered into English) this:

"The alteration of motion is ever proportional to the motive

> force impress'd; and is made in the direction of the right line in which that force is impress'd."[47]

Cohen has drawn attention to the variants in the manuscript versions of Newton's second law (see (1974), p. 330); Newton, he notes, does not have the present-day mathematical version of it (*ibid.*, p. 334), but "the mere absence of an equation ... cannot mask the fact that Newton was perfectly aware of the second law in the form in which we know it ..." (*ibid.*, p. 334). The modern rendering of Newton's second law in current university textbooks may be exemplified by the following: "The rate of change of the velocity of a particle, or its acceleration, is equal to the *resultant of all external forces exerted on the particle* divided by the mass of the particle, and is in the same direction as the *resultant force*" (Sears and Zemansky (1963), p. 101, italics added).

In Newton's second law, thus, the effect of a force on a body is envisaged as being independent of any state of motion that body may be in and of any other forces simultaneously exerted, in other words, forces do not interfere with each other.

The mutual relationship between Newton's first and second laws is often found in current (university-level) expositions of physics thus construed:

> "Since Newton must have realized that the first law is a special case of the second, one may question why he stated the first law at all. The answer is undoubted that the *first law was discovered by Galileo and it enabled Newton to propose the more general second law.*" (Richards et al. (1971), p. 84; italics added)

Be it however noted that in the view of Koyré the matter is not so simple:

> "... though Galileo *never explicitly* formulated this principle [the law of inertial motion]..." (1968, p. 2; italics added)

Cohen, again, sees the matter as even more problematic, for he disputes the generally accepted view of the relation between Galileo and Newton:

> "... I certainly do not believe ... that Galileo should be credited with either having discovered (or having formulated or invented) Newtonian inertia, or *having made use of Newton's first law of motion*." (Cohen (1974), p. 327; italics added)

According to Cohen, then - and in contrast to what is widely assumed - Galileo did not posses Newton's first law. Who, then, did discover the law of inertia? In all likelihood the question in this form is not even amenable to an answer (see *ibid. passim*, Kuhn (STR), pp. 2, 52-76). The above may suffice as a reason why *in the present context* we cannot embark upon a discussion of such specific problems for the historiography of science, which no one has hitherto solved and whose difficulty is reflected in the remark of Cohen: "The debate on Galileo's theory or principle of inertia seems never-ending ..." (1974), p. 326, note 54). However, what has been said thus far may also serve to show that unreflecting dismissal of these problems is, from the standpoint not only of the historiography of science but also of its philosophy, indefensible in the light of modern research. Thus, without going into an account of Cohen's arguments for his conception of the relationship between Galileo and Newton, we may briefly take up one point in respect of which he sees these two scientists to stand distinct from one another:

> "In fact, Galileo never asked the question of whether *unsupported* motion could ever really continue for very long in a linear mode." (*Ibid.*, p. 321)

The fact that Galileo discusses supported continuous motion (e.g. taking place on a level) does not, however, lend such strong support to Cohen's conception that we could ignore the view of Koyrê that "[Galileo's] mechanics, *implicitly*, is based upon [the law of inertial motion]" ((1968), p. 2; italics added). (Newton's own view on the matter also deserves mention. He believed that Galileo *did* know the first law. In Cohen's view this indeed explains why Galileo has been read with Newtonian implications, that is, for Cohen erroneously (see (1974), p. 327).

5. We thus now pass over many (as yet unsolved) problems in the historiography of science and - in full awareness of the

complexity of the matter - speak simply of the *Galilean-Newtonian* conception, whose basic elements (see above) include the following (no account being taken of questions of independency): (GN1) Space is infinite and may be identified with the homogeneous space of Euclid[48]; (GN2) Nature (including dynamic phenomena) may be studied mathematically; (GN3) Motion may be regarded as a purely geometric shift from one point to another in a given coordinate system (motion *per se* exerts no effect on the moving body); (GN4) Rest and motion are conceptually on one level; (GN5) If a body is in a certain state of motion (either steady linear motion or rest), it will remain in this state unless forces applied to it cause it to change; (GN6) Simultaneous motions of a body (or different forces at work upon it) do not interfere with each other.

6. How do the conceptions of motion accepted prior to the tenets of modern (classical) physics relate to the views of the Galilean-Newtonian world-picture set out above?

In what follows we shall be considering the *Aristotelian tradition,* a predecessor of modern physics, confronting that tradition with the Galilean-Newtonian ideas. When we below use also the term 'pre-Galilean' to refer to the views of the Aristotelian tradition in question, which views actually were pre-Galilean, we in no way imply that that term, 'pre-Galilean', covers all conceptions of the movement of bodies *before* Galileo. As in the context of the Galilean-Newtonian conception, above, we also now in the case of the Aristotelian tradition pass over many problems, ideas and claims, as yet unsolved or highly controversial, in the historiography of science, thus leaving, at least, the medieval theory of *impetus* (see e.g. Koyré (1968)) outside our material, and consequently taking no stand on the role of the theory of *impetus* "between" the physics of the Aristotelian tradition and the Galilean-Newtonian system. Our decision here - made in full awareness of the complexity of the matter - seems quite justified for example for the following reasons: our intention among other things is to shed light especially on Kuhn's incompatibility thesis and on the problem whether theories of the same discipline make "progress towards the truth" (cf. Popper (1969), p. 226), and for these purposes, as we shall eventually see, our material may well be enough. - Of course we cannot aspire here to an account of Aristotelian physics but must be content to select, in a completely schematic manner, a number of its features most germane to our present discussion, taking our cue mostly from Duhem and Koyré.

According to Aristotelian physics (Duhem (1954), pp. 305-311, Clagett (1955), Ch. 6) (a) the formative elements of the particles about us may, in varying degrees, comprise fire (the hot element), air (the cold element), earth (the dry element) and water (the wet element); (b) the fifth element is manifested celestially (the moon, the sun, the planets and the stars being among its formations); (c) every element has its *natural place*; (d) if an element is in its natural place it will remain at rest; (e) if an element is removed from its natural place it will return to it by *natural motion* (in its natural place it achieves perfection); (f) the celestial world of the fifth element is governed by laws different from those governing the sublunar or terrestial world - celestial movement possessing only the tendency to uniform circular movement; (g) terrestial local movement, or the movement of bodies around us, may happen, if it is not rectilinear natural, violently: if we remove a body from its natural place, we have distorted the natural tendency of the body, thus prosecuting unnatural or *violent movement*. - Fire, for example, is essentially light and its natural place is above us, where it aspires by natural motion. "Earthly" things are heavy; their natural motion will take them to the centre of the universe. Air and water are likewise heavy, but less so. By natural motion the heaviest will seek to settle beneath the lighter, so that the respective elements are in their natural places when the surfaces of three concentric spheres separate water from earth, air from water and fire from air.

According to the way of thinking of the Aristotelian tradition (see Koyré (1968), Ch. I) the local motion of an object is regarded as a *process of change* in which the object *itself*, in addition to shifting its position in relation to other objects, undergoes a change. The goal of this motion, i.e. this process of change, is rest, which is regarded as a state. Further, if an object is undergoing a number of changes (motions) simultaneously, all of these will affect the object itself; in other words, simultaneous movements of an object *do* interfere with each other. And again, a mathematical approach to the study of nature is considered inadequate because mathematical concepts are not congruent with the data of the senses and because mathematical methods do not enable us to explain qualities or derive motion (in the timeless sphere of mathematics there is neither quality nor motion). In addition to this, the Aristotelian cosmos is finite, and it is unhomogeneous in the sense that the same laws cannot be applied to different natural places. And finally, the process of change in an object (i.e. local motion) cannot continue spontaneously but requires a continuous action of a mover or cause; an object undergoing this process will cease from it the moment it is isolated from its mover. (To the way of thinking of the Aristotelian tradition, then, motion as the law of inertia conceives it, is impossible.)

In sum the Aristotelian tradition, or the pre-Galilean view in the sense of the present work, includes the following basic elements in its theory of local motion: (AT1) The cosmos is finite and unhomogeneous; (AT2) An exclusively mathematical approach to the study of nature is inappropriate; (AT3) Motion is a process of change which cannot be regarded exclusively as a geometric transfer from one location to another and which does *per se* effect objects undergoing it[49]; (AT4) Rest and motion are conceptually on different levels; (AT5) The violent movement of an object will cease as soon as the mover acting on it is removed; (AT6) Simultaneous motions of one and the same body interfere with each other.

7. The following exemplary material of Koyré's illustrates the way that two different views of the motion of solid bodies, the system of the Aristotelian tradition and the Galilean-Newtonian theory, develop and diverge to culminate in the arguments propounded for and against the astronomy of Copernicus (see Koyré (1968), esp. Ch. I).

Having first eliminated peripheral considerations from the arguments of *Aristotle* and *Ptolemy* against the idea of a moving earth, Koyré rephrases these arguments in the following manner. If the earth were in motion, this would have the following effects upon objects resting on its surface: such a powerful centrifugal force would be generated that objects not fixed to the earth would be flung from it, or else would be "left behind" as the earth passed from beneath them (birds and clouds, for example, could not keep up with it). Correspondingly, if the earth were in motion, a cannon-ball dropped from a tower would not fall at the foot of the tower but, by reason of the earth having moved on, at a point some distance from it. The explanation of this is that an object thus dropped - even though it were involved in the movement of the tower - would not after dropping "remember" this involvement but would fall towards the centre of the universe regardless of either the motion of the earth or whatever happened to the point of its landing. (According to Aristotle, heavy objects *do not* fall to the earth in order to be one with the earth but move "of their own accord" towards the centre of the universe unless deflected from this course. If therefore the earth were to be transposed to lunar position, falling bodies would not fall to earth at all but towards that centre as before (see Duhem (1954), pp. 223, 228). Precisely this makes the Aristotelian theory of falling bodies understandable.)

Observation, however, will show that birds, clouds etc. are not left behind, and that objects dropped from towers pass in linear course to a point directly beneath that from which they are released. Must we not then say that Aristotle, on the basis of observation and *modus tollens*, had adequate methodological - indeed Popperian - grounds for rejecting the notion of a moving earth! Observation, moreover, *confirms* Aristotle's own theory. Must we not then say that he also had adequate methodological - indeed RV-type! - grounds for retaining his hypothesis whereby the earth does not move!

Another entailment of the physics of the Aristotelian tradition is this: a cannon-ball dropped from the mast of a stationary ship falls at the foot of the mast whereas one dropped from the mast of a moving ship is left behind - the further behind the faster the ship is travelling.

Copernicus' objection to the Aristotelian view is based on his own conception of a connection between the earth and "earthly" bodies; to the earth and its objects the earth's movement is natural, and falling bodies, clouds, birds and so on partake of that movement and, as a result of it, are not left behind. Here

Copernicus is giving impulse to the subsequent development whereby the Aristotelian conception of space as unhomogeneous is rejected. Copernicus also explains the *ostensibly* linear course of an object dropped from a tower by reference to the common movement of earth, object, tower and observer.

The next contribution to the evolution of the Galilean-Newtonian matrix is that of *Giordano Bruno.* Bruno realizes that a prerequisite to the new astronomy is replacement of the conception of a closed and finite system with one of an infinite and open universe which, for example, knows no "natural places"; space is seen as homogeneous, so that observation of a given movement may be referred simultaneously to different systems. This also means that the point of departure of a moving body has *of itself* no significance in determining the course of its motion. Setting out from these insights Bruno is able to deal with a more complex version of the above example of a ship in motion; his ship is passing under a bridge, and at precisely the same moment a cannon-ball is dropped from both mast and bridge; and from exactly the same height. His conclusion is that the body dropped from the bridge will fall along the normal of the river surface while that dropped from the mast will "share" the ship's motion, and although as observed from the ship it falls along the normal of the deck to the foot of the mast, its course as seen from without is not the normal of the river surface. (The fact that Bruno's reflections do *not* yet represent Galilean-Newtonian argumentation cannot be taken up in the present context.)

For any physicist who endorses the modern (classical) approach, Bruno's argument, besides on the one hand being partly incomplete or erroneous, is on the other hand trivial in its conclusion, and those who question his implication are simply being childish (since the matter could be confirmed by a simple experiment). This is not, however, the case. The insight, which Bruno here subscribes, belongs to those ideas in the history of science which representatives of the time have found difficult to comprehend. To forestall any attempt to explain this away on the grounds of myth, superstition, error, non-experimental spirit or the like, we may turn at once to the following figure in Koyré's "gallery", the best exponent in his day in the field of *observation*

(NB!). (It has in fact been said to be belittling this man to find no more superlative word than 'best' to describe him.)

The man in question is *Tycho Brahe*, who states outright that an object dropped from a ship's mast does not fall at the foot of the mast but to the rear of it - *the further from it, the faster the ship is travelling*. Correspondingly, in arguing for his own view that the earth does not move, Brahe claims that if the earth were in motion, a missile could not be propelled the same distance both east and west, for the earth's motion would exert its effect (possibly even preventing the movement of a missile propelled in the opposite direction). There are indeed clear-cut paradigmatic reasons why Brahe could not have endorsed Bruno's ship-bridge argument, as the following comment of Koyré's makes plain. If namely two bodies having the same point of departure, one of them being associated with the mast of a moving ship and the other with an immobile bridge, proceed to their common destination at the centre of the earth along courses differing by reason of their different associations, then these objects (thus we must obviously put the matter in Aristotelian terms) must be remembering what they were associated with, must be aware of their goal and must be capable of making for it; acceptance of Bruno's view could only have meant to *Brahe* adoption of a childish myth.

We may further make brief mention of *Kepler*, who retains many Aristotelian conceptions (for example the conceptual-ontological distinction of level between motion and rest), yet believes that the earth is moving. To overcome the - by then - age-old model problems (tower and cannon-ball, birds and clouds etc. etc.) Kepler proposes a force of attraction at work between material objects:

> "If in some place in the universe we were to put two stones close to each other and *beyond the sphere of influence of any body related to them*, these stones in the manner of two magnets would come to meet in a place in between, and the paths they would follow in order to meet would be in inverse ratio to their masses."[50] (Italics added)

The words italicized would suggest that Kepler envisaged a force of universal gravitation, but we may at least say that for Kepler the *earth* attracts *"earthly"* objects: objects are bound to the earth by elastic chains, which explains how they keep up with the earth's movement (see Koyré (1968), p. 11). Considering the nature of Newton's law of gravity, Kepler's view cannot be held a mere curiosity; nor is it an idiosyncracy that Kepler believed the motion of an object to cease immediately upon isolation from its mover - a point (another point) which should give pause to those who regard the law of inertia as trivial and all earlier conceptions of the matter as childish non-science.

8. Was Galileo an empiricist who - as he himself puts it in a standard English version of the *Discorsi* - "discovered by *experiment* some properties of [motion] which are worth knowing and which have not hitherto been either observed or demonstrated."[51]

As we have seen, he can hardly have been an empiricist in the sense of having by experiment been *able* to derive conceptions incompatible with the views of the Aristotelian tradition. But what was his own metatheoretical conception of himself - did he regard empirical procedure as the correct approach (as he has since been assumed to have done)? In his analysis of the ship-mast-shot example Galileo carefully dissects the principle of relativity involved (see Koyré (1968), pp. 12-15): we have the movement of the object in relation on the one hand to the earth and on the other to the ship. Without appeal to experience Galileo now concludes that the movement of the falling cannon-ball in relation to the ship is independent of the ship's speed. To his empirically (!) oriented Aristotelian opponent, who insists on experiment, Galileo retorts that no experiments have been nor need be made, for the matter *cannot* be otherwise.

Is this not in contradiction to the statement introduced at the opening of this section ("by experiment") and to the conception of Galileo generally prevailing? It is Koyré who above all stresses Galileo's non-empirical orientation; we may at the same time attribute in part to Koyré the demonstration that *succeeding generations* have been responsible for "empiricizing" Galileo: those important words in the opening quotation have been *added* in the English translation (see Cohen (1974), p. 338).

As indications of this subsequent process of "empiricization", Cohen mentions Salusbury's translation of 1661, which makes Salviati say

> "I am assured that the effect will ensue as I tell You; for it is necessary that it should ..."

whereas Drake's recent rendering has for the same passage

> "Without experiment I am sure that the effect will happen as I tell you, because it must happen that way ..."

so that Salusbury has deleted the words "without experiment" (*ibid.*, p. 340). At a suitable point the translators have thus *inserted* the words "by experiment" and at a correspondingly suitable point *deleted* the words "without experiment". Here - in addition to the textbook tradition in the field - we have a step in the process whereby Galileo has come to be treated as an empiricist.

Lest the complexity of the problem involved should be lost to view and here too the matter be oversimplified, be it further noted that in actual fact Galileo had conducted the experiment alluded to in the last citation, but did not wish to appeal to it (this appears from one of his letters; see *ibid.*, p. 340).

Although we cannot in the case of Galileo (or of the study of nature in general) endorse Koyré's view that "[g]ood physics is made *a priori* ((1968), p. 13), we may nevertheless now take it that on the one hand Galileo's image of himself is on more or less Platonic lines, and that on the other the relationship of his theory - as indeed of any other theory of nature - to experiment and observation is far removed from what those of empiricist persuasion would like to imagine.

9. Was Aristotelian physics mythical or childish? The (largely) Aristotelian approach to the tower-shot example rests on a principle whereby the movement of the point from which the object has embarked upon its falling course can be ignored as irrelevant once attention focuses on the object's progress after release. Koyré finds a parallel between the Aristotelian characterization of the falling motion and the modern description of the progress of light, and

concludes that the former cannot therefore be treated as absurd (1968), pp. 7-8).

It follows from the basic ideas of Aristotelian physics (Duhem (1954), pp. 309-310; see also above) that if the order of the universe *were* perfect, in other words, if a stable equilibrium obtained, then the world, if removed from this state, would seek (by natural movement) to revert to it. As Duhem (an outstanding French thermodynamist) points out, modern physics shows that under certain conditions all events pertaining to a given group of particles will tend to establish in the system in question a stable equilibrium; and finally, as Duhem puts it:

> "If we rid the physics of Aristotle and of scholasticism of the outworn and demoded scientific clothing covering it ... we would be struck by its resemblance to our modern physical theory ..."[52] (*Ibid.*, p. 310)

10.3. Reduction vs. Revolution

1. Our example shows how fundamentally the system of the Aristotelian tradition and the Galilean-Newtonian theory are incompatible. The incompatibility between theories will be taken up again in later sections. Before that we now focus upon changes in scientific *standards* in respect of a particular phenomenon, namely gravity, thus revealing how standards, too, may be seen as incompatible.

2. Aristotle's view was that a heavy body will move "of its own accord" towards a certain point (the centre of the universe) - if not obstructed - by natural movement; all objects will seek their natural place. As Kuhn points out, this did not - of course, we might add - strike Aristotle or Scholastics (of the same persuasion) as being the tautological play on words it had by the XVIIth century been made to seem ((STR), p. 104).

We shall now consider the *mechanistic-corpuscular* mode of conception (a significant trend above all in the XVIIth century) whose precise purpose it was to explain observable phenomena (such as colour, taste, even the weight of an object) by reference solely to the size, shape, position and motion of basic matter (formed, moving matter is all that exists) (Kuhn (STR), p. 104). If any other attribution was ever given to basic corpuscules, this was taken as resort to occult modes of explanation: such elements had by mechanistic-corpuscular standards no place in science, wherefore the accounts of nature in the Aristotelian tradition could be eliminated from science (*ibid.*, p. 104).

Let us now follow up Kuhn's thought regarding Newton (see *ibid.*, pp. 105-106). Newton's interpretation of nature set out from the conception of neutral basic corpuscules together with their motion and interplay, this all being performed in an infinite and homogeneous void, that is, space (see also Koyré (1955), pp. 11-12). From this point of departure he arrived at a theory of which the law of gravitation was one component. But what was the nature of this law? If (and when) gravity was conceived as an innate attraction between any two arbitrarily given particles, this was tantamount to the introduction into science of an element of the occult in precisely the same sense as had been applied in the Aristotelian notion of natural movement. Since the law of gravity was thus *unscientific* by mechanistic-corpuscular standards, there remained a choice between two alternatives:

(i) Newton's theory, applying what were by these standards occult principles (at least one: the law of gravitation), must be abandoned - and many chose this course.[53]

(ii) An effort must be made to find a mechanistic explanation for this law.

Newton himself sought - in vain - to pursue the latter course. What was then to be done, since without Nexton's theory one could get nowhere, and yet that theory could not be accommodated under prevailing scientific standards? The solution chosen is illuminating: *scientific standards were altered*, in other words, the inexplicability of the law of gravitation was acknowledged, and this, in Kuhn's words, means the following:

> "... scientists gradually accepted the view that gravity was indeed innate. ... and the result was *a genuine reversion ... to a scholastic standard*" (*Ibid.*, p. 105; italics added)

Kuhn holds that when in the present century Einstein succeeded in explaining the force of gravity, this explanation "returned science to a set of canons and problems that are, in this particular respect, more like those of Newton's predecessors than of his successors" ((STR), p. 108).

3. The scientific standards described above are incompatible in that if in a phase of scientific change one standard is accepted the other must be rejected. The function of standards in constituting scientific activity is so much an integral part of that activity that they may well be considered to belong to the paradigm in question; thus any given paradigm P would be an ordered quintuple P = [O, U, E, S, V], these sets being in corresponding order the ontological conceptions, symbolic generalizations, exemplars and standards and values governing research in the sphere of the paradigm P. In the context of the present work, however, we would not introduce such a definition but in all cases rather retain the conception of the given paradigm as an ordered quadruple P = [O, U, E, V], thus taking the standards of the above-mentioned class S to be entailed in the points of departure for research, which we have designated as being closely associated with those of the paradigm itself.

4. The theories evolved to explicate the movement of solid bodies shed an interesting light on the present theme of "Reduction versus Revolution". How could the various conceptions of the movement of solid bodies adopted at different stages in the history of science be analysed along the lines afforded by the paradigm view, or be rendered transparent by means of that view in the sense in which we have envisaged it? Be it noted at once that we cannot offer any precise answer to this question any more than anyone else has to this day been able to answer it in the form in which we pose it. Some inkling of the enormous complexity of the matter may be gathered from the fact that decision for example as to what laws in a given theory are to be regarded as constitutive of that theory - in other words as paradigmatic - proves problematic even in the case of a theory which happens to be that most thoroughly illuminated by the history and the philosophy of science, namely the very example

we have been employing, the Galilean-Newtonian system. Though we can thus not embark upon a thorough inquiry into this vast problem area - the history of conceptions of the movement of solid bodies from antiquity to modern times as embodied with all its ramifications in the paradigm view - something essential and illustrative we can nevertheless say on the basis of the paradigm view and the paradigm concept. We may now proceed to an examination of the two theories taken up in the foregoing, the system of the Aristotelian tradition ARIS and the Galilean-Newtonian theory NEWT, together with a third, the Einsteinian matrix EINS. Our attention is focused mainly on the paradigm-specific ontology O, the set of symbolic generalizations U and the closely related points of departure for research in each of these theories.

5. With regard to ARIS, NEWT and EINS we may state among other things the following.

The most basic views of ARIS contain the conceptions which we above named from (AT1) to (AT6): the cosmos is finite and unhomogeneous; local motions are processes of change, an understanding of whose nature rests upon the notion of a perfect order of the whole (in objects themselves processes take place with motion); in consequence of this latter circumstance motion cannot be considered mathematically as a mere geometrical transfer from one place to another; conceptually-ontologically rest and motion subsist on different levels; the (violent) motion of a body will cease the moment its mover is withdrawn; simultaneous motions (processes) of an object interfere with each other.

The paradigmatic views of NEWT (cf. the conditions (GN1) - (GN6), above), contain, or stand in a particularly close association with, conceptions incompatible with the above aspects of ARIS. Specifically the paradigm of NEWT contains or immediately involves the view that space is infinite and homogeneous, and that there exists a referential system absolutely at rest and in one way or another anchored in the void. Precisely these latter tenets make it possible to postulate Newton's first law: this law stands in a conceptual relationship to the ontological notion of space referred to. The law of inertia may be thought of as a special case of Newton's second law, and the place of this law is manifestly among the symbolic generalizations of the paradigm of NEWT, as Kuhn (POST) explicity states (pp. 182-184; see also (STR), p. 78); on the second law note also Kuhn (1976), for example:

> "Somehow the second law [of Newton] is *constitutive* of the *entire* mechanical tradition which descends from Newton's work." (P. 188; italics added)

Be it noted that the second law presupposes the effects of forces exacted upon a body to be independent of each other and of the state of motion of the body; in other words it is possible to speak of a resultant of forces: with this as starting-point the claim of the independence of *motions* can be derived. Nexton's second law - like his first - is thus an absurdity from the point of view of ARIS. We shall here pass over the question of whether Newton's third law is to be considered constitutive of Newtonian thought and thus to be included in the paradigm or matrix NEWT (see Kuhn (1976), pp. 188-190); on the other hand the answer to be given to Kuhn's question

> "Why is Newton's second law clearly constitutive of mechanics, his law of gravitation not?" (*ibid.*, p. 189),

receives its direction precisely from the extent to which (see above) the fundamental insight of the second law and that of its "rival" is in fact involved in all the model examples - tower and cannon-ball, ship, mast and cannon-ball, bridge, ship, mast and cannon-ball etc. - applied down the ages, and, by the same token, in the arguments for and against a heliocentric cosmology. And even more can be said: in the last analysis the basically Aristotelian ideas of the mutual interference of motions, a conception opposed to the principle entailed in Newton's second law, is constitutive not only of the pre-Galilean world-picture in our sense, but also of the entire thought-style, as regards the movement of solid bodies, in the history of science *before* Galileo (see Koyré (1968), pp. 10-11): the rejection of this thought-style is in a way *made manifest* in the acceptance of one single law, Newton's second.

Finally we come to the matrix EINS, which contains among other things the following (see for example Reichenbach (1951), Kuhn (1970)):

(E1) "The universe is not infinite but a closed Riemannian space of a spherical type", which is not to say that "the universe is enclosed in a sort of spherical shell, which in turn is embedded in an infinite space" (Reichenbach (1951), p. 209). This in turn implies that "the *total space is finite* without having a border" (*ibid.*, p. 209; italics added), so that "wherever we are, there is always some space around us in all directions, and no end is visible" (*ibid.*, p. 209), but - and the following thought experiment sheds further light on the paradigm-specific ontology of EINS - "if we move on a straight line, we shall some day come back to our starting-point from the other direction" (*ibid.*, p. 209).

(E2) Absolute rest and movement, like the Newton-anchored absolute coordinate system, are impossibilities.

(E3) The concept of mass diverges sharply from that in NEWT: "Newtonian mass is conserved; [that of EINS] is convertible with energy" (Kuhn (STR), p. 102).

(E4) Contrary to the conception in NEWT, movement affects the moving object. Particularly considering conditions (AT3) of ARIS and (GN3) of NEWT, we may note that the size and the mass of the body alter as a function of the behaviour of the moving body and of the coordinate systems in question: "Only at low relative velocities may the two [Newtonian mass and Einsteinian mass] be measured in the same way, and even then they must not be conceived to be the same" (Kuhn (STR), p. 102).

6. The above may serve to illustrate how the paradigms are incompatible in first place in respect of their basic points of departure. Here, however, establishment of their incompatibility takes place at the verbal or heuristic level; for example, "space *is* finite" versus "space is *not* finite". Yet this does not mean that the ontological conceptions involved are *themselves* mere verbal ornaments, for - as we have seen - in conjunction with the corresponding symbolic generalizations and exemplars they are constitutive of all empirical research activities pursued within the

framework of the paradigm in question. Now, secondly, at this empirical level the incompatibility is *manifested* in the course of actualization of the paradigms for example in mutually divergent predictions produced by the respective theories, let us call them T_1 and T_2. According to the accumulation theory of the development of science the former theory T_1 should be reducible to T_2 or be seen as a special case of it. Let us now proceed to examine the grounds upon which Kuhn considers the derivation of T_1 from T_2 impossible if the genuinity of the theory is to be preserved; *mutatis mutandis* his arguments also show - if indeed they are valid - that T_1, as a genuine theory, cannot be retained as a special case even when the deductive derivation of T_1 from T_2 is not a requirement (cf. for example weak reduction, also Popper, Chapter I, above).

7. Consider a scientific revolution P/Q, where a theory associated with the paradigm P, now in crisis by reason of anomalies encountered, is replaced by a theory associated with Q, *whose precise purpose is to explain the anomalies the earlier theory is faced with.* But - Kuhn points out - this means that the new theory will in certain respects produce different predictions from those of its predecessor, so that the two theories cannot be logically compatible (or consistent); this means that

> "[i]n the process of being assimilated, the second must *displace* the first" (Kuhn (STR), p. 97; italics added)

and further with regard to the case in point:

> "From the viewpoint of this essay [STR] these two theories [contemporary Einsteinian dynamics and the older dynamical equations that descend from Newton's *Principia*] are *fundamentally incompatible* ...: Einstein's theory can be acccepted only with the recognition that Newton's was wrong. Today this remains a minority view." (*Ibid.*, p. 98; italics added)

How does Kuhn justify this view which in 1962 he held to be a minority conception and which maintained that of two successive paradigmatic theories the former cannot in a genuine sense be reconstructed as a special case of the latter (as for example

Newton's in relation to Einstein's)?

According to reductionist principles the claim (C1) obtains: Newton's dynamics is a special case of Einstein's (or can be reconstructed as such). For Kuhn, in contrast, claim (C2) must hold: Einstein's and Newton's dynamics are mutually incompatible in the strong sense which does not allow the latter theory to be a special case of the former.

Kuhn sees the reductionist endorsement of (C1) to have the following objections to (C2) (*ibid.*, p. 99):

(i) Relativist dynamics E cannot show Newtonian dynamics N to be in error because the latter is still successfully applied in many contexts.

(ii) On the basis of E it can be shown that the predictions of N are (within the limits of available instruments) good in applications which fulfil certain marginal conditions (for example the relative velocities of bodies studied must be small compared with the velocity of light).

(iii) "Subject to this condition [see point (ii), above] and a few others, Newtonian theory seems to be derivable from Einsteinian, of which it is therefore a special case" *(ibid.*, p. 99).

(iv) But a theory (for example E) cannot be at odds with a special case of itself (N) (see point (iii), above). Therefore for example E is valid in the area W_E, N in W_N, and any application of N in the range $W_E - W_N$ is simply an error which does not belong to science (or which easily produces the illusion of (C2) to the effect that N and E are not compatible).

Kuhn's answers to the above points (i) - (iv) may be condensed as follows (see *ibid.*, pp. 99-102):

(a) By argumentation which rests upon the applicability of a theory to a certain area and (subsequently from the horizon of a new theory) restricts the scope of that theory to precisely that area in which it has proved effective, it is possible to rescue *any* theory whatsoever so long as it has had *some* success in application *somewhere;* in this manner for instance the phlogiston theory and Ptolemy's astronomy can be kept afloat. In the case for example of Ptolemy the situation is characteristic. This theory is still actually and successfully applied in certain calculations. If we now take the ideas of scope restriction and theory saving seriously must we not save Ptolemy, too!

(b) Acceptance of a principle which applies the above-mentioned restrictive conditions leads to a situation where, within the scope of a given theory, one can speak scientifically only of phenomena which are *already known*. But precisely this would mean cessation of all scientific progress:

> "... the result of accepting them [these prohibitons] would be the *end of the research through which science may develop further*." (*Ibid*., p. 100; italics added)
>
> "If *positivistic restrictions* on the range of a theory's legitimate applicability are taken literally, the mechanism that tells the scientific community what problems may lead to fundamental change *must* cease *to function*." (*Ibid*., p. 101; italics added)

The matter might also be put thus: restriction of a given theory to apply to what is *already* known would make of that theory something other than what we understand by an empirical scientific theory and would - in preventing new insights from flourishing - by the same token halt the *progress* of science.

(c) Kuhn's third argument against reducibility (and also against the proposal that the former of successive theories can be preserved as a special case of its successor by comparing the two for example in Popperian SUPERs, as we have termed them) is based on the notion whereby a point-by-point comparison of two paradigmatic theories is not possible by reason of the lack of a language common to both; even if, for example, we were to derive from the relativistic dynamics E, under the above-mentioned marginal conditions, a theory N' *resembling* the Newtonian dynamics, Newton's N is not derived from E, because N' is in fact a special case of *relativistic* mechanics whose variables and parameters still represent Einsteinian space, time and mass, whose spheres of reference are not identical to those of the same name in N (Kuhn (STR), p. 102; see also e.g. Kuhn (1976), pp. 190-196).

8. By a *reductio ad absurdum* Kuhn's above line of thought (a) leads to the result he intends, but a closer examination of the

matter allows us to discern behind this argument a particular concept of empirical theory. Likewise his argument (b) is based - not only on the observed development of actual scientific activity and *reductio ad absurdum* - also on a new meaning attributed to the concept of empirical theory, which, however, Kuhn nowhere systematically elaborates. In order now to proceed with our inquiry we must examine the concept of paradigmatic theory, which has featured in the foregoing discussion as an undefined entity - while in the same context we shall consider more closely the incommensurability thesis entailed in the argument (c) above.
- Before those considerations we wish to say a few words about the nature of our exemplary material.

9. We have in the course of our discussion given numerous instances of conceptions of some object of research which are, for example, incompatible. One essential function of these instances has been to shed light on precisely such concepts in the philosophy of science as incompatibility and incommensurability. Here it must be noted that not all of our examples are necessarily paradigms but possibly those conceptual matrices which Kuhn refers to as being 'something much like a paradigm' ((STR), p. ix). This circumstance in no way detracts from the force of our presentation. *In the first place*, the object of our examples was, as just pointed out, to shed light on certain complex aspects of the development of science, among them the incompatibility and the incommensurability of different paradigms and scientific change within a paradigm or from one matrix to another. These matters may at least in part be elucidated also by means of examples where what is involved is not a paradigm but an entity which is "something much like a paradigm". *Secondly* - and this is vital - we shall proceed in § 11 to examine the above and certain other central points from an analytical standpoint, thus "freeing" ourselves from examples (albeit employing examples here too by way of enlivening our presentation). *Thirdly*, as also pointed out in the above, it is not our task to write a history of science from the standpoint of the paradigm concept, nor need we in every case determine whether what is involved is a paradigm or "something much like one". It is thus also to be noted that the employment of

such entities as these latter for example in illustration of the incompatibility of *paradigms* in no way nullifies the description, nor, on the other hand, does it imply that what is involved *is* a paradigm. Offsetting these last-mentioned reservations, however, is the essential point that, *fourthly,* many of the instances employed in the foregoing discussion are among the clearests in the sphere of quantitative theories, and such as specifically involve paradigms and not entities whose nature might be described as "something much like a paradigm" (see esp. § 6, section 1 and § 10, section 7).

§ 11. PARADIGMS AND PARADIGMATIC THEORIES

According to Kuhn's theory, the task of normal science is the actualization of paradigms[54]. The analysis of this process will, as we shall eventually see, uncover the core of the paradigm view.

We shall first examine the nature of normal science and then the formulation of the concept of scientific empirical theory based on the ideas presented in the characterization of actualization, together with certain implications arising from this approach.

11.1 Normal Science and the Actualization of a Paradigm

1. A paradigm candidate achieves the status of paradigm, that is to say, comes to be widely actualized, only if it offers a better means of dealing with at least *some* recognized acute problems than does a rival paradigm or rival candidates. In the initial stages of the actualization process results obtained within the framework of a paradigm are few in number and often of a tentative nature (in the case of Newton's mechanics, for example, successive adjustments to his gravitation constant may be said to have continued into the present century). The fact indeed is that a paradigm emerging in the course of a scientific revolution allows a glimpse of a *potential* order of nature; the task of normal science is to render this potential structure *actual,* in other words to show the areas in

which, the means by which and the precision with which that paradigm can be applied (see esp. Kuhn (1961), pp. 167-168). We shall proceed now to an account of the various modes of paradigm actualization in normal-scientific research, basing ourselves, without detailed indication of references, on Kuhn (1961) and Kuhn (STR), pp. 23-34.

2. The success of a given paradigm in surpassing its rivals in a phase of scientific change derives initially in large part from *its promise of adequacy in actual research.* Grounds for confidence in it are the few instances of positive research outcome apparently indicating the superiority of this approach over its rivals as a basis for dealing with acknowledged acute problems. Actualization of a paradigm takes place by (A) opening up new areas of its application, (B) improving the degree of agreement between prediction and data in the field of its applications, and (C) *articulating* it (for example by adjustments to the magnitude of constants involved).

Since the nature of this process of actualization is, as noted, almost completely passed over in critiques of Kuhn hitherto, we may here - as a basis for eventual conclusions - schematize the modes of paradigm actualization. Be it noted that the lines of demarcation between modes (A) - (C) cannot be precisely laid down, for example improvement of the degree of agreement and calculation of constants are mutually dependent processes. Even more artificial than this categorization is a further subdivision into *factual* and *theoretical* normal-scientific research because data are theory-laden, as we have come to realize (see also below).

The *factual* objectives of actualization of paradigms may be set out as follows:

(I.A.) Establishment of facts which from the standpoint of the paradigm are particularly interesting and revealing. The definition of such facts is sought with ever greater precision and in ever more varied conditions. As examples of type (I.A) research one might mention, in *astronomy,* ascertainment of the position and size of heavenly bodies and the junctures of solar eclipse; in chemistry e.g. definition of boiling-points and determination of the optical properties of various substances.

(I.B) Establishment of facts not necessarily of interest in themselves but bearing comparison with the predictions of a paradigmatic theory. By this means an attempt is made to discover points of contact between theory and nature and to improve the degree of agreement wherever such points are brought to light (see types (II.A) and (II.B), below). Construction of the relevant apparatus here frequently presupposes the theory in question, and correspondingly, measurements carried out with these instruments will usually have no content *whatsoever* without the theory. The objective of type (I.B) research is "to bring *nature and theory into closer and closer agreement*" (Kuhn (STR), p. 27; italics added).

(I.C) Establishment of such facts as will facilitate the diversification and refinement or articulation of a paradigm. At the stage of articulation one will seek among other things to resolve any inconsistencies remaining in a paradigmatic theory and any problems such as could not have been even pinpointed except on the basis of the theory in question. This type of research may be subdivided into three categories:

(I.C.i) Calculation of constants. It frequently occurs that the magnitude of constants involved in a theory is not known in the initial stages of developing that theory. In the case of gravitation, for example, the first apparatus for measuring its constant was not constructed until some 100 years after the Principia. In the calculation of constants the dependency of research activity upon the operative paradigm becomes manifest: the very problem cannot even be posed, let alone solved, except in the context of the paradigm.

(I.C.ii) The search for new quantitative laws. The existence of such laws is often adumbrated by the paradigm years before equipment for their experimental determination is invented (see esp. Kuhn (1961), pp. 171-177, 185-190).

(I.C.iii) Application of the paradigm to qualitatively new phenomena.

The *theoretical* objectives of paradigm actualization may be correspondingly classified as follows:

(II.A) The search for new areas of application of the paradigm. For example, the basic application of Newton's Principia was to celestial mechanics. Newton himself extended his paradigm to embrace, among others, the following areas: on the earth's surface the motion of the pendulum and tidal phenomena, and in the air, study of the speed of sound.

(II.B) Research of type (II.A) brings out points of contact between theory and nature; by means of type (II.B) the degree of agreement between prediction and data is improved wherever such points have emerged. Research of this type stands in mutual relationship especially to types (I.B) and (I.C). Demonstration of agreement between theory and nature is often possible *only* by means of idealization and additional assumptions which together conflict with theory. Reduction of error arising from this conflict (i.e. improvement of the degree of agreement) often constitutes the chief and the most demanding form of normal-scientific (basic) research. In the exemplary case of Newton's mechanics, representatives of normal science have included Euler, Lagrange, Laplace and Gauss, all of whom exerted marked influence particularly in the field of type (II.B) research.

(II.C) Articulation, comprising in this case mainly the construction of new equivalent but logically more satisfactory versions of the paradigmatic theory.

11.2. Paradigmatic Theories

1. On the basis of the above we may now take Kuhn to mean the following: when a paradigm is actualized, this does *not* necessarily imply that as a result of type (A) research the paradigm can be extended to cover one universal field of application, but that in the process of this research a *variety* of applications are established for the paradigm, each of which is subsequently actualized by type (B) (for Newton's case see e.g. Kuhn (STR), pp. 30-31). In our view this is one of the basic themes especially of Kuhn (1961), which, while running implicitly to some extent throughout the work, finds quite explicit formulation in a general case (entirely independent of concrete examples) thus:

> "[Normal science's] objective is ... [B] to *improve the measure of "reasonable agreement" characteristic of the theory in a given application* and [A] to *open up new areas of application* and establish *new* measures of "reasonable agreement" applicable to *them*." (P. 171; italics and text in squared brackets added)

Let P be a paradigm (for example that possessed by Newton) and let $T^{(P,\Omega)}$ be an application of it in a given area (e.g. celestial mechanics). We shall then call $T^{(P,\Omega)}$ an *actualization theory* or *actualization (of the paradigm* P *in area* Ω). Since this actualization of a paradigm alters with time, we must in fact consider actualizations in a series indexed for time, thus

$$(11.1) \quad \ldots\ T_i^{(P,\Omega)},\ T_{i+1}^{(P,\Omega)},\ \ldots\ .$$

Provided there is no danger of confusion, however, we shall speak simply of the theory $T^{(P,\Omega)}$ (or of the theory T).

2. Let T be a theory[55] and

$$(11.2) \quad \begin{array}{l} \text{Predictions of the theory: } a_1, \ldots, a_n \\ \text{Experimental results: } a_1', \ldots, a_n' \end{array}$$

be a table with the figures a_i always corresponding to given predictions of the theory specified by initial conditions S, and the figures a_i' representing results of corresponding experiments or observations. We used above the concept of agreement in connection with tables of the form (11.2). By reason, however, of restrictions imposed by instruments and measuring devices one may speak of agreement between the figures a_i and the data a_i' in a table of the above form only approximatively. Approximations made for this reason we call *inessential approximations*. However, the figures a_i and a_i' in a table of the form (11.2) usually diverge from each other more than would be expected on the basis of instrumental considerations. This is due to *essential approximations*[56], which the following example of Kuhn's will elucidate (on the foregoing and the following example see Kuhn (1961), pp. 165, 169-171).

According to Newton's theory of gravitation, *all* celestial bodies exert a force of attraction upon each other. Now, even though astronomical predictions were simplified by taking account only of the sun and the moon and the six planets known in Newton's time, the result is found to be such a difficult mathematical problem that to this day no mathematical solution has been reached which would give precise figures for the case. Newton himself, to overcome this difficulty, made an essential approximation which was in conflict with the law of gravitation his theory entailed; he assumed that each planet was attracted only by the sun, and the moon only by the earth. Thus he was able to arrive at some kind of predictive result, from which, however, the positions of the planets clearly diverged: in fact the figures in the table of form (11.2) as such were not in agreement even after adjustment for the error margin arising from inessential approximations. To reduce this margin of error (to improve the measure of agreement between theory and data - type (B) of actualization) "perturbation" calculations were introduced, in the first of which Newton himself worked out a figure for the effect of the sun on lunar motion.

3. By way of generalizing on the foregoing example we might state the following. In the case of the actualization theory T* on the motion of celestial bodies as entailed in the paradigm Newton possessed (type (A) actualization which Newton himself *carried out*), his work was continued by others (type (B) actualization *instituted* by Newton and *continued* by others), and at each given stage there was a *consensus* of the scientific community with regard to each partial application of the actualization T* as to the magnitude of the real figure ε defining the range of variation

(11.3) $|a_i - a_i'| < \varepsilon$

of results (a_i, a_i') fitting tables of the form (11.2) acceptable as scientific in the special application in question.

Using the machinery here introduced we might now say that the refinement of the measure ε was the central task in the actualization of the paradigm Newton possessed in its actualization T*. For example the anomaly associated with the behaviour of the planet Mercury was earlier obscured by reason of excessively "lax" stipulations for scientificity of results; only when, as a result of *basic research* within the framework of normal science - the work of scholars like Euler, Lagrange, Laplace and Gauss - the ε measure expressing the range of permitted variance of scientific results was sufficiently reduced, could the anomaly in question be recognized (on the *example*, see Kuhn (1961), p. 170).

4. Tables of the form (11.2), or corresponding presentations, constitute an important element in the content of textbooks. Kuhn remarks that no textbook accepted by the scientific community contains tables which could test a theory presented, because the readers (novices in the field) are not in a position to do anything but accept the theory in question on the authority of the writer and the scientific community ((1961), p. 164). Kuhn is clearly right: but what is then the function of tables in textbooks?

With the above in mind we may presumably formulate Kuhn's reply in the following manner. For the precision *itself* of any ε measure arrived at for a given partial application of a given actualization of a given paradigm in a given phase of development in normal science, no external criteria can be given - what could they be founded on? In actual fact scientific texts define by means of tables of the form (11.2) the magnitude of this norm, that is, they simply express the consensus of scholarship on the matter: the figure ε is thus an irreducible part of the *theory*. The tables in textbooks do not therefore serve to discover anything new, they do not test theories, they do no more than state the prevailing consensus as to the permissible range of variance in results acceptable as scientific in the sense of (11.2) - (11.3): there are then no *higher* criteria for the agreement of the figures a_i and a_i' in the sense of table (11.2) - on the contrary, by their very presence in the text these themselves constitute the criteria of acceptability. (On the foregoing, see *ibid.*, pp. 164-166).

The magnitude of the figure ε is not, however, to be agreed upon, in spite of the fact that no measure of second order can be indicated for it and that it does represent the accepted view of this day; its magnitude is based on objective and repeatable results of normal science (cf. for example *ibid.*, pp. 176-177; Kuhn (STR), p. 28; (1970c), p. 263).

5. Let the various phases in the actualization of a paradigm P in an area Ω, suitably indexed for time, form a series

(11.4) $\ldots\ T_i^{(P,\Omega)},\ T_{i+1}^{(P,\Omega)},\ \ldots\ .$

whose continuity thus rests upon the paradigm[57]. On the basis of the above we now define the actualization theory $T_i^{(P,\Omega)}$ more precisely as an ordered quadruple

$$(11.5) \quad T_i^{(P,\Omega)} = [P,\ \Omega,\ \Gamma_i,\ METR_i],$$

in which

(i) P is the paradigm,

(ii) Ω is the area of actualization of the paradigm in the case in question,

(iii) Γ_i is a set of claims specifying what is known of the area Ω in the sense of paradigm P (i.e. in a *paradigm P-laden* or more simply a *paradigm-laden* or *theory-laden* sense) at a given time-point i, and

(iv) $METR_i$ is the set of ε measures relevant to phase i of actualization.

Be it noted that for the sake of simplicity we have omitted more precise indexing of the sets Γ_i and $METR_i$: to which particular actualization theory these factors belong is whenever necessary brought out in the relevant context. For brevity we may also for an actualization theory T write T = [P, Ω, Γ, METR].

6. Now we are in a position to formulate the main types of function of normal science as follows. Let P be a paradigm. The actualization of P will entail the following basic functions:

(A) Construction of actualization theories

$$(11.6) \quad T^{(P,\Omega_1)},\ T^{(P,\Omega_2)},\ \ldots$$

associated with the various areas Ω_1, Ω_2, ... means discovery of points of contact between P and nature.

(B) Let $T^{(P,\Omega)}$ be such a point of contact (an actualization theory). Here the task of normal science is to construct and refine ε measures for $T^{(P,\Omega)}$ complying with condition (11.3), in other words to develop for $T^{(P,\Omega)}$ series of the form

(11.7) $\ldots \geq \varepsilon_i \geq \varepsilon_{i+1} > \ldots$

in which the real figure ε_{i+k} in the particular case of the actualization $T^{(P,\Omega)}$ of the paradigm P at timepoint i+k in the development of normal science expresses the permitted range of variance for scientifically acceptable results in the sense of (11.3). (Any results obtained by an individual scholar which fall outside the limits laid down in ε will test the scholar, not the actualization or the paradigm.)

(C) The alteration with time of an actualization $T^{(P,\Omega)}$ as a result of *articulation* manifests itself for example as follows: (i) the ε measures become more precise; (ii) the constants in the theory become more precise; (iii) the number of quantitative laws belonging to the actualization increases, and (iv) equivalent but for example mathematically more elegant alternatives may be found for the theory $T^{(P,\Omega)}$.

7. Let the actualization theories of a paradigm P constitute a family $G(P)$. We now say that the family $G(P)$ of actualization theories is a *paradigmatic theory* (or a *theory* provided there is no danger of confusion) (*generated by the paradigm* P). Since over a period of time the theories in the family $G(P)$ alter and new actualizations are discovered, we must in fact consider a series appropriately indexed for time, thus

(11.8) $\ldots G_i(P),\ G_{i+1}(P),\ \ldots\ ,$

whose continuity derives from the paradigm. (Where no specifications as to time are involved and where there is no danger of confusion we shall simply speak of the paradigmatic theory $G(P)$.)

A paradigmatic theory $G(P)$ in the phase of actualization brings with it a promise (a potentiality) which in the process of normal-scientific activity is made actual. If it is desired to examine this theory as restricted to the level of actualization attained at a time-point t, with its progress halted so that its potential beyond the point t is excluded from consideration, we have a *frozen* theory, which we denote $\mathrm{FROZEN}_t(G(P))$, or simply $\mathrm{FROZEN}(G(P))$. Let T be an actualization of G(P). When T is frozen at some time point t, we have the frozen actualization FROZEN_t(T), or FROZEN(T) for short. Such a FROZEN(T) we call also a *calculator* (or the calculator T of the paradigm P) when applied solely to areas in which T has met with success at the time point t or before that point. (Sometimes we shall also speak of FROZEN(G(P)), when suitably restricted, as the calculator of P.) Calculators may now be employed for example in the making of predictions or in technological applications (e.g. in building houses) with no reference to the actualization of the paradigm in question, thus leaving the paradigm-specific ontology and, for example, the paradigm's claims, if not in the range of the calculator, aside. For instance the astronomy of Ptolemy is still in use for certain calculations and the Newtonian mechanics is still successfully used in technical applications, say, in planning flights to the moon - the use of those calculators, of course, with no intention to claim respectively either that the earth is in the centre of the universe or that the universe is homogeneous and infinite in the sense of the Galilean-Newtonian paradigm and in contradistinction to the Einsteinian matrix.

8. Be it noted that research results obtained in the course of actualization do *not* bring with them the possibility of deciding in advance that the paradigm will be applicable in some area to which it has not *yet* been applied. Thus although only such problems are brought into focus in normal science for which a given paradigm seems to promise a solution, and although problems involved in natural phenomena can - if indeed they can - be solved in normal science only within the framework of a paradigm, this does *not* imply that one could with this paradigm give in advance (before the potential has even been set on a course of actualization in the area at hand) necessary and sufficient conditions which would delimit a set of areas of application for the paradigm to which it has not yet been applied. We must indeed emphasize the significance of the simultaneous - and *apparently* contradictory - validity of the following two points:

(i) *Paradigm* P *and theory* G(P) *are closed* in the following sense (see Kuhn (STR), e.g. pp. 35-42 and §§ 5 and 6, above). Normal science is prepared to tackle only such problems which in the light of the paradigm appear amenable to solution; and correspondingly, solutions achieved in normal science have been possible only within the framework of a paradigm. If attempts to extend a paradigm to some new area fail, or if results do not fulfil certain conditions of precision in terms of actualizations hitherto attained, this does not necessitate entering into a new paradigm, nor does it test the paradigm (which is closed); test is made here of the investigator: he has *misunderstood* the potential order promised by the paradigm and has sought to construct an actual order where no such order exists, or else he has within the framework of an already known actualization obtained results falling below the level of refinement which the scientific community on the basis of previous achievements regards as criteria of scientificity.

(ii) *Paradigm* P *and theory* G(P) *are open:*

> "... commitment [to a paradigm] *must* extend [A] *to areas* and [B] *to degrees of precision* for which there is *no full precedent*." (*Ibid.*, p. 100; italics and text in squared brackets added)

This is one of the central normative tenets in the Kuhnian value constellation (see § 8, section 3, above). That a paradigmatic theory would not extend to previously *unknown* areas changes this theory into something other than an empirical scientific theory in the sense of Kuhn (see *ibid.*, pp. 100-101, Kuhn (POST), pp. 205-206, (1973)): it is not possible on the basis of a paradigm to define *in advance* sufficient and necessary conditions demarcating the family G(P). If such a definition were possible, empirical science in the sense we understand it would not be necessary in the first place: empirical science would become a calculator by means of which in a given situation it would be possible by fixing additional conditions to calculate the details of a result, in principle already known. The fact that normal science also - fortunately - arrives at *results* affording technology instruments for certain circumscribed and concise applications (for example in radio communication and the building profession) does *not* imply that normal science and routine might be mutually identifiable (cf. § 7.2, above).

The fact that the above conditions (i) and (ii) are simultaneously valid does not (of course) contain anything paradoxical.

As an example of actualizations of the paradigmatic points of departure entailed in Newton's *Principia*, Kuhn mentions among others the following applications devised by Newton himself (e.g. (STR), pp. 30-31):

(i) Celestial mechanics (the basic example).
(ii) Study of tidal phenomena (an "earthly" actualization).
(iii) The pendulum theory (an "earthly" actualization).
(iv) Study of the progress of sound in intermediary substance (in the air).

9. In normal-scientific research theory-ladenness derives from the paradigmatic points of departure, one of whose components comprises the exemplars[58], which in their turn give cognitive significance to the actualization theories. A paradigm must have a functional component (exemplars) before one can speak of it as a paradigm. In a way which is extremely difficult to define we must say that the basic exemplars exist "simultaneously with" (or even "prior to"?) the paradigm. The process of actualization cannot be thought to proceed in that we *first* construct a completely abstract paradigmatic "theory" which has no application whatsoever behind it and for which we then proceed to "deduce" applications. The actualization process must have exemplars to which it can refer as it proceeds. Actualization is thus by reason also of these exemplary instances a theory-laden process.

10. In order to be able to speak of the logical incompatibility of paradigms as we have been doing, the theories in question must be assumed to have a common, identifiable object in the case concerned (in the matter of incompatible predictions this object may be for example a point anomalous in respect of one and decisive for the other).

Above we considered the following type of example (see § 6, section 19). An Aristotelian observing a weight swinging at the end of a string sees it as a falling object tending towards the centre of the universe but prevented in this by the string and attaining rest at the lowest possible point only by a "devious" route and after a considerable interval of time. For Galileo, on the other hand, the swinging weight is a *pendulum*. One might now ask: Is not the statement regarding the weight swinging on the end of the string and eventually coming to rest neutral with respect to both the Aristotelian and the Galilean theory? Is not therefore a language in which both theories can be represented and thus neutrally compared, possible? In reply to this we would point out that since the statement in question merely localizes the weight swinging on the end of the string, and since the localizing statement has nothing to say of it *as part* of Aristotle's theory or Galileo's theory, this is not the case. Correspondingly, objects "baptized" with names - the planets, for example - may under these same names be transferred from the domain of one paradigm to that of another (see also esp. Kuhn (1979a)).

11.3. The Nature of a Paradigmatic Theory

1. As we have seen in the foregoing, Kuhn gives some of the paradigmatic starting-points in a way the character of logical truth for the scientific community. In this matter he employs two widely differing modes of expression. On the one hand he notes that for *those* who have committed themselves to a paradigm this point of departure constitutes *as it were* a purely logical statement which no set of observations can refute ((STR), p. 78). On the other hand he says that for example in the revolution associated with Dalton in chemistry a given paradigmatic mould became after the breakthrough a *tautology* "that no single set of measurements *could* have upset" (*ibid.*, p. 132; italics added).

A completely traditional interpretation of this would be that the informative constituents of a paradigm simply form a set of the most fundamental statements of the theory in question, of which statements

(i) part are of the nature of definitions and are thus not open to falsification; and

(ii) part are "normal" laws or hypotheses neutrally and independently subject to testing and are only "as it were" logical and are such only within the scientific community concerned and only for the following *psychological* reason: on account of their widespread acceptance and successful application they have attained a constitutive position in scientific tradition and hence have fallen under a kind of sociopsychological "law of inertia" whereby they are not abandoned and their falsification is not acknowledged even in face of convincing negative evidence without prolonged and arrogant opposition. The normal scientist is thus a dogmatist who will persistently *refuse* to see that a "paradigmatic" hypothesis he subscribes to has been falsified.

The object of Kuhn's analogy cannot, however, be to repeat the traditional distinction between a *definition* and a *testable statement*, simply clothing it in fatuous metaphor. In what follows we thus turn to a "third" alternative: at least part of the paradigmatic points of departure are not amenable to testing and yet do not belong to the logical sphere or to that of convention or definition.

2. Our question is now this: can the ontological commitments and symbolic generalizations of a paradigm in some sense of principle be the object of testing; can they be subject to the concept of truth in the sense that they describe the "real" essence of nature. Let us first examine the ontology of a paradigm.

We have no immediate and direct means of establishing whether nature is such or in some measure such as the ontology of a given paradigm would claim, for we cannot represent nature without some conceptual system. Hence the situation with respect to two successive paradigms P and Q is this. If the ontology of Q were thought to "approach more closely to the truth" than that of P, this assumption must imply that nature's ultimate constituents are more of the kind Q envisages than of the kind P has assumed. But the expression "more of the kind" is void for the same reason as is the expression "what nature is really like" (see e.g. Kuhn (1979c), esp. p. 265). The emptiness of the phrase "more of the kind" as it stands is manifest for example in that the very question "Is the universe "really" finite or infinite?", as addressed to the incompatible ontologies of two well-known paradigms, is in itself empty and insoluble. In such terms, then it is not possible to speak of the truth of the ontology of a paradigm.

Could the mode of expression "what is nature really like?" be rescued in some other way? The next step in the endeavour to find an ontological application for the concept of truth would be to pass to the level of actualization theories and to imagine that the success of predictions and for example technological applications of a given actualization theory represent evidence that nature is such as the underlying ontology envisages. However, given even such success, this does not allow us to see nature as it is in its ontological structure; confirmation of predictions made under some theory is no guarantee that the ontology of that theory holds true (see § 2.1 above).

It might next be sought to attribute ontological applicability to the concept of truth by passing to a consideration of the respective actualization theories of two rival paradigms, let us call them theories T_1 and T_2, and their empirical consequences. It would then be attempted, on the basis of these consequences, to carry out a preference mapping, defined in one way or another, whereby the better rated theory would be such that nature's ultimate constituents were more of the kind this theory holds them to be than of the kind claimed in the rival. (Here, then, the fact that one theory outdoes the other in this rating process would be an indication that a better account has been given of nature's ultimate order; in other words, truth has at the ontological level been approached; by the same token, then, it would have been possible to speak of "what nature is really like".) But such a step likewise encounters its own difficulties. In the first place finite data can always be derived from two incompatible theories (*ibid.*, p. 265). But empirical researches are always confined to finite material. How then could it be claimed that of two incompatible ontologies one is more like nature than the other? Secondly there arises the following difficulty inherent in the mechanisms of scientific progress. If the above kind of preference mapping is to be carried out, the assumption must be made that T_1 and T_2 and the relevant data can be exhaustively presented in the same (for example a third) language or that they can be exhaustively monitored within the framework of a third system. There are, however, obstacles to our obtaining by this means a view of "what nature is really like". If namely the language in which comparison is to be made is that of T_2, the theory T_1 must be examined as a frozen system - this because T_1 and T_2 are incompatible; to mention one example, it is not possible at one and the same time to actualize the conception whereby the movement of a body does not in itself affect that body and the conception whereby the body's movement does affect it. But here T_1 is not as an actualizable entity in the comparison, it must be treated as a frozen system. If again T_1 and T_2 are appraised from the standpoint of a third paradigmatic system, it follows that both must be frozen. Further, if this third framework is an artificial system, in other words one which has not emerged in the actual development of the discipline, the examination is once more concerned with frozen actualizations. Now, however, the preference mapping envisaged will be addressed to neither T_1 nor T_2 but to their frozen forms. And now, in any consideration of the frozen form of an actualization theory the paradigmatic sources of the actualization process, first of all the ontology, are by-passed, with the result that this procedure forfeits the very thing it was intended to attain, namely the possibility of applying the concept of truth to ontological points of departure! (Part of the above considerations have a bearing on the thesis of incommensurability, to which we shall address ourselves in § 11.4, below).

One further mode of tackling the problem of approaching the truth could be the following. Let P_1, P_2, ... be successive paradigms and let O_1, O_2, ... be their respective ontologies. If now one wants to speak of successive theories as approaching to the truth, that is, moving towards positions from which we may progressively more readily describe what the ultimate constituents of nature are "really" like, this must imply the existence of a borderline ontology O^* towards which the series O_1, O_2, ... converges if carried to infinity. Though we could never know this O^*, the approach to the truth - whatever it means at the ontological level but provided it indeed takes place - should in the long run be reflected as a perceptible *convergence* in the series O_1, O_2, ...

Since *recognition* of the truth of ontologies, as described in the paragraph, just above, proved an impossibility, the problem of approaching the truth might now be examined "empirically": is there in the actual development of science an approach to the truth at the ontological level in the sense of convergence? For the sake of representativity examples upheld here should cover as long a period of history as possible and should be taken from well-advanced disciplines. Our own example material in § 10, which is thus but one exemplar, especially fulfils these criteria; and these examples do not speak for convergence. Our exemplar is from a discipline which is regarded as the most advanced in the history of science. It is thus worthy of note that in this very context the referent of the expression "one theory gives a better account than the other of what nature is *really* like" is not methodologically identifiable, and the expression is also not "empirically confirmed". (On this section see e.g. Kuhn (STR), p. 103, (POST), pp. 206-207, (1970c), p. 265; cf. also Kuhn (1979a), p. 418.)

3. In illustrating paradigms and paradigmatic theories by means of example we have seen that the theory-specific ontology stands in conceptual ("cybernetic") relation to the symbolic generalizations of the paradigm in question (see § 10, above). For example the postulation of the interference of different simultaneous motions of the same solid body was in the view of the Aristotelian tradition *inseparably* bound up with the Aristotelian world view *in toto* (the symbolic generalization in question cannot be abandoned without abandoning that world view, and *vice versa*). In the breakthrough which brought transfer from the Aristotelian world-picture to the Galilean-Newtonian paradigm the discrepancy which precipitated the shift was precisely a matter of world order (theory-specific ontology) on the one hand and the interference vs. non-interference of simultaneous movements of solid bodies (certain symbolic generalizations or generalizations associated with these) on the other. In refuting the Aristotelian theory of motion it was of crucial importance in the analysis of the various forces affecting the same body (or of the various motions of the same body) to apply the principle of non-interference (which is included in Newton's second

law, whose consequence his first law is). When model examples (e.g. ship, bridge and cannon-ball) were solved to refute the Aristotelian view, it was precisely the non-interference assumption and the law of inertia which were invoked. But this first law of Newton's and the non-interference assumption together constituted a conceptual impossibility in the Aristotelian order, and to postulate them, at least historically, meant postulating infinite homogeneous space, together with certain ontological assumptions as to the nature of matter, which postulation was in conflict with the views of the Aristotelian tradition. This example illustrates how intimately the symbolic generalizations of a paradigm are conceptually related to its ontology; examples on this point can easily be developed, the relation between the Galilean-Newtonian paradigm and the Einsteinian matrix, for instance, offering the same conclusions as above.

Thus at least historically the symbolic generalizations of paradigmatic theories are conceptually dependent upon the ontology of the theory and *vice versa* (on this point see also esp. the Cartesian example in § 5). This circumstance must for its part affect the question whether truth distribution can apply to symbolical generalizations or whether these can be tested (the difficulty is thus on the one hand that a paradigm-specific ontology can hardly be amenable to testing and on the other that symbolic generalizations are bound to ontologies). Secondly, the problem of the testability of symbolic generalizations is aggravated by the fact that they derive part of their cognitive content from exemplars, which for their part receive their form to some extent as dictated by the symbolic generalizations (which latter, again, are bound up with their ontology). Clearly, then, the testing of symbolic generalizations is rendered particularly problematic by the *holistic* nature of the entity constituted by symbolic generalizations, ontology and examplars.

According to Kuhn, the symbolic generalizations, for example Newton's second law, function *partly* as laws and *partly* as definitions (so that symbolic generalizations have the function of partly defining some of the concepts they contain); in addition, these two functions of symbolic generalizations are inseparable one from the other ((STR), pp. 78, 132-133, (POST), p. 183). And the quarrel over these symbolic generalizations is by nature such that "neither experiment nor a change of definitional convention could be relevant" ((STR), p.132).

Bearing this and the above-mentioned holistic aspect in mind it can only be said that symbolic generalizations are not conventions because in a state of crisis they cannot be adjusted by a mere change of definition; *nor* are they empirical hypotheses, because "no single set of ... measurements could ... upset [them]" (*ibid.*, p. 133).

Symbolic generalizations as a part of the holistic entity comprising also the paradigm-specific ontology and exemplars, are thus at odds with the traditional distinction in that they are not definitions, *yet* they are immune to direct falsification.

What then is the nature of symbolic generalizations? In the first place we must eliminate once and for all the *synthetic a priori* possibility: since in a scientific revolution the previous paradigm must be *abandoned*, the symbolic generalizations as a part of it cannot be synthetic *a priori*.

Symbolic generalizations, "third propositions"[59], constitute a problem whose resolution is promoted by a consideration of the way they "perturb" the traditional categorization into testable vs. inaccessible to testing, empirical vs. non-empirical, analytic vs. synthetic. We shall not set out upon a discussion of the problems which flood in when we problematicize the relationship of these distinctions to each other and to the distinction *a priori* vs. *a posteriori*. In the following we examine the placing of symbolic generalizations in the three categorizations mentioned. (On the matter of how the problem of "third propositions" has been thought to involve a breach of the distinction *a priori* vs. *a posteriori* itself, see note 59.)

Let P be a paradigm. Are the symbolic generalizations of P empirical, or non-empirical, or something else, in which case they would conflict with the generally accepted distinction? If they were non-empirical it would obviously be difficult to understand why they should be in any substantial way necessary to the process of actualization (e.g. in the formation of new P-laden experimental laws). Secondly, it would be difficult to see how such non-empirical principles would lead to anomalies. And thirdly, how is it possible that non-empirical principles must by reason of the incompatibility thesis be abandoned in a scientific revolution? Must it not be concluded on the basis of what has been said here that symbolic generalizations are expressly empirical? In other words, the criteria for the empiricity of the symbolic generalizations will be the following:

(U1) Symbolic generalizations must (for their part) make the actualization of the paradigm and its *encounter* with anomalies possible (success in actualization, discovery of anomalies and scientific crisis render the empiricity of symbolic generalizations transparent).

(U2) Symbolic generalizations must be eliminable in a scientific revolution (scientific revolutions render the empiricity of symbolic generalizations transparent and show that they are not synthetic *a priori*).

Symbolic generalizations are thus empirical, and correspondingly we might say that their "validation" takes place precisely in actualizing the paradigm as a holistic entity, and their "falsification" takes place in a scientific revolution. Thus the criticism levelled against metaphysics by the logical empiricists and put (prior to Kuhn's work in the field of the philosophy of science) by Feigl in the form

> "The two senses in which the word "metaphysics" covers enterprises that seem objectionable to the logical empiricist are of course (1) transcendent, i.e. in principle untestable, assertions, and (2) the belief in factual truths that could be validated a priori, i.e. in complete independence of the data of observation." ((1956), p. 22)

does not find its mark in the case of Kuhn's theory, even though this has often thought to be the case (see e.g. Chapter III, above). We may now define the *testing of a paradigm* or *a paradigmatic theory in Kuhn's sense* precisely as an actualization operation in normal science and as an extraordinary operation in crisis science. In the positive case such a Kuhn test *validates the paradigm* or *paradigmatic theory in Kuhn's sense;* in the negative case the outcome may be a scientific revolution in which the previous *paradigm* or *paradigmatic theory is falsified in Kuhn's sense*. The falsification and rejection of a theory in Kuhn's sense thus presupposes the introduction of an at least partly established new paradigmatic theory.

4. Are symbolic generalizations then at all "third"? They are if we cling to the traditional categories. According to the discriminative power of the traditional distinctions "third propositions" would fall outside the classifications, breaching for example the analytic-synthetic dichotomy. On the other hand, if the "real world" factor of the accepted dichotomies be in each case reconstructed so that this factor is not bound to be the target of *traditional* testing - that is, a target of direct falsification, where the limits on "directness" may be only proof-theoretical or test-theoretical - the categorizations themselves change so that symbolic generalizations cease to be "third" and revert to a renewed empirical, testable and synthetic sphere.

5. To close the discussion on testing and truth we may now note that in the process of articulation the testing of a P-provisional law γ intended for membership in the set Γ of the actualization T of G(P) will proceed analogically to traditional test procedure because in actualization of the theory T, the hypothesis γ, its possible rivals and the relevant data *are* represented in the same language, namely in that of G(P). Note, however, that the testing of such a hypothesis γ does not take neutrally but with a P-loading. The testing is nonetheless objective because, for instance, negative results may falsify γ in a P-laden manner. The concept of truth may then have its intra-theoretical application in G(P) (see Kuhn (1970c), esp. pp. 264-265).

6. Finally, with regard to the concept of theory itself we may note the following. According to the traditional view of science an empirical scientific theory can be exhaustively defined as a set of statements which in the ideal case can be axiomatized; further essential to it is that the theory be independently testable. The theory concept of the paradigm view thus diverges from the

traditional view in a fundamental manner in respect of both structure and functional attributes.

11.4. How to Compare Paradigms? Paradigm Values and the Resolution of the Paradox between the Incompatibility and Incommensurability Theses

1. We have above examined the values which function as criteria governing the progress of science (see § 8, section 3). The value constellation and its function is according to Kuhn's theory the following. Choice of paradigm cannot be made definitively by appeal to observation and logical argument; the representatives of various paradigms employ a persuasive approach appealing for example to the achievements and prospects of their particular "protégé" in the light of the values invoked as criteria, these values being (at least) the following five (see Kuhn (STR), pp. 153–159, (POST), pp.188–195, 199–200, (1973) *passim;* we have slightly modified the constellation): (i) the *simplicity* of the theory; (ii) the *scope* of the theory: it should explain much more than the particular phenomena in whose connection it was introduced; (iii) the *precision* of the theory: its consequences should be in good, and with time ever better, harmony with observations; (iv) the *fruitfulness* of the theory: in the first place the surprise element (hitherto unexpected solutions), secondly the number of problems it solves, thirdly the nature of these problems; (v) the *consistency* of the theory: it should be in good agreement with other knowledge (and within itself it should involve no contradiction).

It is easy to see how, on the one hand, these criteria may be in conflict among themselves and how, on the other, they may if applied with bias lead to cessation of progress in science. For example value (v), requiring that a theory be in harmony with knowledge hitherto acquired, and value (iii), requiring precision, may well come into conflict with value (iv), the requirement of surprise, for new discoveries (at least those of any magnitude) are not always in harmony with previous knowledge, and will scarcely ever *initially* come up with precise results. This being the case, if the requirements of values (iii) and (v) are set high enough (as is quite possible in a scientific community), nothing new can ever be accepted. The inflexible application of (iii) and (v) would thus eventually freeze science into a conservatory and repair shop for old theories. Here we would have to concede that the progress of science had ceased (or at least we would have to concede that the nature of scientific progress had decisively altered compared to what the history of science records). With the number of problems solved as the sole criterion of scientific progress, again, the result would be an enormous amount of utterly futile knowledge. The number of trifling and futile solutions would swell (at the cost of important and essential achievements), so that by definition we would have to say that the state of science was advancing even though we would surely know that progress had ceased.

The value constellation cannot then provide an *algorithm* for choice, as the above examples make manifest; besides, different individuals may apply the values with different weighting. Yet the argumentation invoked within the constellation is completely rational. In the application of values appeal is - tacitly - made to two fundamental, *non-relativistic* assumptions: any two successive paradigms have (1) objective achievements which (2) *can* be compared. (On the basis of comparison a choice of paradigm is reached and thereby gradually a new phase of normal science.) (On this section see esp. Kuhn (1973).)

2. But is not this rational discrimination between paradigms, which Kuhn postulates as early as STR (pp. 153-159), at odds with Kuhn's own incommensurability thesis, for this thesis, after all, was taken by Kuhn to have as consequence (see § 6, above) that the debate surrounding the choice of paradigm is circular in that no other *standard* enters into it but the assent of the scientific community in question.

If thus comparison and rational consideration of paradigms appealing, within the value constellation invoked, to the respective achievements of the candidates, is possible, then the paradigms cannot be incommensurable - and if they *are* incommensurable, they cannot be compared from the horizon of paradigm values!

This inherent paradox in Kuhn's theory may also be brought out in another context[60], most crucial from our own point of view. According to the incompatibility thesis two successive paradigms are incompatible, for in given situations they give e.g. different predictive results (as emerges from the fact that the later paradigm solves some of the anomalies left by its predecessor). Successive paradigms are thus comparable, and with such preciseness that by logical means we show that *both* theories cannot hold simultaneously. But according to the thesis of incommensurability successive paradigms are incommensurable, which brings us face to face with the following paradox: If the paradigms are incompatible, they are comparable and cannot be incommensurable; if again they are incommensurable, they cannot be incompatible (since the establishment of incompatibility presupposes commensurability).

Is Kuhn's theory contradictory? Are the theses of incommensurability and incompatibility "incompatible"? Or is the paradox thus derived amenable to resolution? Be it noted that Kuhn himself has not, as far as we know, given apt attention to this conflicting aspect of his two theses, let alone sought to solve it (see, however, Kuhn (1976), pp. 190-191). Thus how are we to eliminate the paradox described above as subsisting between the thesis of incommensurability and the thesis of incompatibility?

3. We have in the foregoing conceived the incompatibility of paradigms in three ways. Firstly, incompatibility can be shown at the level of paradigms themselves; for example, "The universe is infinite" versus "The universe is finite" (see §§ 5, 6 and 10, above). Secondly, the actualization processes of two competing paradigms will be such that they cannot be carried out at one and the same time: how could we simultaneously actualize incompatible principles which constitute the entire research processes of the normal sciences they govern? After the manner of Wittgenstein we may speak of the incompatibility of life forms of different scientific communities, particularly clearly manifested for example in mutually exclusive standards regulating their respective research activities (see §§ 5, 6 and 10.3, above). Thirdly, we have referred to the logical incompatibility of paradigms, meaning by this the logical

incompatibility of the respective actualization theories of each: when for example these actualization theories produce at the *empirical* level predictions which do not coincide, they are logically incompatible (in other words, the sets Γ_1 and Γ_2, being components in that order of two actualization theories T_1 and T_2, are such as to be incompatible; see §§ 5, 6, 10 and 11.1-11.3, above). Thus from incompatible paradigms we end up, by way of incompatible actualization processes, with for example mutually conflicting predictions at the empirical level, these revealing the actualization theories in question to be logically incompatible in the sense we have envisaged. (If in some context it is not necessary to specify the form of incompatibility in question, or if this is otherwise obvious from the context, we shall speak simply of the incompatibility of paradigms or paradigmatic theories; here it must nonetheless be pointed out that in the interpretation we have given to the paradigm view we have specified the concept of incompatibility as involving three different dimensions.)

Provided that given competing theories have been adequately analysed in the light of the paradigm concept, incompatibility at the level of the paradigms themselves is "logically" easy to detect: for example (1) "The universe is infinite" versus (2) "The universe is finite" or (3) "The movement of a body does not *per se* affect that body" versus (4) "The movement of a body *per se* affects that body", where (1) and (2) on the one hand and (3) and (4) on the other are mutually exclusive. It must however be noted that the detection of incompatibility between some paradigmatic points of departure in two given paradigms in isolation from their actualization is a particularly "abstract" operation in that the cognitive significance of the constituents of a paradigm is determined for its part by the very process of actualization. Even though the detection of incompatibility between the paradigmatic points of departure of different paradigms can be effected, as it was in the above examples and as for example the researcher in the history of science can accomplish it, in considerable measure as a kind of "disputative" operation, these paradigmatic points of departure, as parts in the holistic entity of their own paradigms, are far from being in themselves mere verbal elements, for as parts of their paradigms they are constitutive of all empirical research pursued within the framework of their respective normal sciences. Some of the terminal points of the actualization processes of competing paradigms are of the kind in which one paradigm encounters an anomaly which the other paradigm clarifies. In such cases the detection of incompatibility takes place in particularly concrete terms.

The incompatibility of paradigms was ascertained in the foregoing by means of examples counterposing the Galilean-Newtonian paradigm and the Einsteinian matrix at the level both of the paradigms themselves and of the predictions they produce, and the principle of incompatibility was applied in a number of instances in examining the development of science. A comparison of paradigms has thus been made. How then - if at all - can paradigms be incommensurable if they can be compared in this apparently eminently lucid manner?

Let P and Q be successive paradigms and T_1 and T_2 their respective competing (and incompatible) actualization theories. We shall now clarify the principle of incommensurability by the following method. Let us inquire what would happen or what it would mean if (a) we wished to reduce T_1 to T_2, stipulating that T_1 be derivable from T_2; or if (b) we wished to see T_1 as a special case of T_2 without requiring the derivability of T_1 itself from T_2; or if (c) we wished to make such a comparison between these two theories as would enable us to specify *exhaustively* which components in the one correspond to which in the other on every point where "there is" such a correspondence, without however requiring T_1 to be reducible (point (a)) or to be weakly reducible (point (b)) to T_2.

If now after the manner of point (a) above we seek to reconstruct T_1 as a special instance of T_2 in the sense that T_1 should be deductively derivable from T_2, we shall have the following situation. Since T_1 and T_2 are logically incompatible in that T_2 is able to clear up some anomaly in the sphere of T_1, the area of application of T_1 must in the envisaged reduction be restricted to that in which T_1 has proved successful, and this restriction will take place from the horizon of T_2, as Kuhn puts it (see § 10:3, above). However, with a view to the progress of science this procedure would hinder the function of the mechanism by which the further development of science takes place and by which the scientific community is apprised of problems which may lead to a fundamental scientific change as Kuhn again puts it (see § 10.3, above). At the same time the Kuhnian characteristic of empirical theories, their *openness*, would be eliminated, with the result that T_1 would no longer be involved in the envisaged comparison as a theory but as a frozen entity. Even though the logical incompatibility of T_1 and T_2 were by appropriate restriction of the scope of the former successfully eliminated, T_1 and T_2 as theories, that is, as actualizable entities, remain incompatible at the level of their paradigms, and they remain logically incompatible (provided in the latter case that the restrictions placed on T_1 be overlooked). This means that in the comparison of the theories we have instead of the theory T_1 a frozen entity, namely FROZEN(T_1), the paradigmatic points of departure of T_1 being for the purposes of the frozen comparison *left aside*. Such a frozen actualization theory can of course - as pointed out in the foregoing - be employed as a calculator in some technical applications, as for example Newton's mechanics can be for example in the planning of flights to the moon. Nevertheless such a use of T_1 (Newton's mechanics) as a calculator alongside the *theory* T_2 (relativistic mechanics) is *not* an actualization of T_1, *nor* is it normal science pursued according to T_1, *nor* does it mean acceptance of the paradigmatic points of departure of T_1. This comparison of theories will thus be carried out with the basic unit of scientific progress, the actualizable entity T_1, replaced by a technological instrument. The theory T_1 is not then itself, as a theory, involved in the reduction, because the limits of T_1 cannot be defined at least in keeping with what we conceive to be the true progress of science. Further, the holistic entity composed by the paradigmatic points of departure of T_1 cannot as an actualizable entity be embedded in T_2, because the actualization of T_1 and T_2 as a composite T_1-T_2 theory is impossible in the first place by reason of their incompatibility. Secondly, we cannot conceive what sort of thing this T_1-T_2 would be as an actualizable entity; how for example would we proceed in actualization with a theory whereby (1) the universe is infinite and finite; (2) the movement of a body does not *per se* affect that body and *per se* does affect it; (3) we can proceed on a straight line *ad infinitum* advancing in homogeneous infinite space ever further from our starting-point and on the other hand "if we move on a straight line, we shall some day come back to our starting-point from the other direction"; and so on (see our example material in § 10). In this sense - in the sense, that is, that we cannot imagine what sort of a thing T_1-T_2 could be as an actualizable entity - the theories T_1 and T_2 are incommensurable. But the actualization *processes* of T_1 and T_2 are also incommensurable, for when these theories approach unknown territory it is not possible on the basis of one of them to "predict" what will happen next in the process of actualizing the other. It follows ultimately from the quality of openness of empirical theories that T_1 and T_2 as holistic, actualizable and open entities, are not amenable to point-by-point comparison even if partial comparison in the FROZEN sense can be made: in this sense T_1 and T_2 are also incommensurable. For the reasons set out here competing paradigms, paradigmatic theories and the last-mentioned corresponding actualization theories are incommensurable, but nevertheless at the same time incompatible.

Competing paradigms are incommensurable. This incommensurability may be metaphorically illustrated in the following way. Imagine two different, perpetually open jigsaw puzzles. It would be impossible to conceive how these puzzles could be completed by selecting pieces alternately from one box and the other, and it would be impossible to say what kind of a picture would eventually result from each puzzle. Nevertheless the partially completed pictures of each may contain meaningful images, frozen sections. These *may* be compared with each other - without however any possibility either of comparing the completion *processes* as genuinely progressive sequences or of comparing the respective pictures as completed wholes.

The above considerations apply in our opinion *mutatis mutandis* likewise to points (b) and (c). Thus we can say that successive paradigms, paradigmatic points of departure and their respective actualization theories are incommensurable but nonetheless incompatible and comparable in the manner described above. Finally we would stress that in the context of the actual development of science the comparison of paradigmatic theories takes place invariably from the standpoint arrived at in the latter of successive paradigms (this obviously also where comparison is carried out in artificial languages not drawn from the history of real empirical science).

11.5. Against Normal Science?

1. What in fact is normal science? What is its structure and nature? What are its functions?

Resting upon what has been brought out hitherto we may briefly and conclusively state the following. The task of normal science in respect of actualizing a given paradigm P and paradigmatic theory *G*(P) is to render known actualization theories more discriminative and comprehensive, and to seek new actualizations. A paradigmatic theory and its actualization theories change with time: prerequisite to theory-specific adaptation is the normal-scientific research whereby information pertaining to reality but bearing the loading of the paradigm within whose framework it is gleaned, is fed back into the paradigmatic theory in order to modify it. Normal-scientific research is thus not in the main - to put it as laconically as possible - the *application* of a given theory (an engineering job) but, in the best sense of the word, basic research, in the course of which the projections of our senses - theories - are adapted to reality.

2. Further, normal science makes possible the adaptation of scientific knowledge over long intervals of time in the following manner:

(i) In actualization, a theory-specific adaptation is realized up to a certain limit.

(ii) Normal science possesses a built-in mechanism by means of which it can free itself of commitment to condition (i) and, through crisis, brings about scientific revolution; and thus

> "though a *quasi-dogmatic* commitment is, on the one hand, a source of resistance and controversy, it is also instrumental in

> making the sciences the *most consistently revolutionary* of *all* human activities." (Kuhn (1963b), p. 349; italics added)

This comment of Kuhn's on *quasi*-dogmatic commitment as a factor leading to scientific revolution comes in the very lecture of 1961 which Toulmin would place in his Kuhn phase 2 (see § 9, section 2, *phase 2*, above). As regards Toulmin's misconception we may quite briefly note here that the scientific change

(11.9) ... $G_i(P)$, $G^{\cdot}_{i+1}(P)$, ...

is not brought about in a manner comparable for Kuhn with the way changes are effected by religious dogmas; no more is the progress of science in sense (11.9) a function of dogmatism.

The overlooking of this point, namely that (11.9) is precisely a function of normal science, is what also leads Feyerabend astray (see § 7.4, section 2, above): although the "jobbing" carried out on the strength of the results of normal science might sometimes remind one of Feyerabend's safe-cracker league, nevertheless not even his *Dillingers* (Feyerabend (1970a), p. 200, note 2) would ever make pioneers of (11.9), - (11.9) after all usually represents quite complicated basic research.

Popper's analysis of normal science, too (see § 7.2, section 3), fails altogether to grasp the nature of actualization: normal science really exists, but, contrary to what Popper thinks, it is not a peripheral phenomenon gradually brought into being by mass training: to eliminate it would eliminate science (not for sociological but) for epistemological reasons - a point which, setting out from Popper's assumptions, is understandably difficult to appreciate. It is further true that the representative of normal science has learnt techniques of application, but here again Popper fails to realize that exerting an influence on the sequence (11.9) by a given technique *is* basic research, and anyone who does not exert an influence on sequence (11.9) has no place in normal science. Normal science is not (as Popper thinks) the offspring of technology which Kuhn erroneously projects over the entire history of science; *on the contrary*, an indispensable condition for the adaptation of knowledge over long periods of time is precisely normal science, whose typical representatives - with Newtonian science in mind - must, on Kuhnian criteria, include Euler, Lagrange, Laplace and Gauss, of whom none, to the present writer's way of thinking, was "badly taught" or "a victim of indoctrination" (as Popper says of the normal scientist) - nor is there any more justification for thinking that the normal science these Newtonians represent is - as Popper would have it - "a danger to science and, indeed, to our civilization".

CHAPTER V
ANSWERING CRITICISM OF KUHN'S THEORY IN THE LIGHT OF OUR KUHN-INTERPRETATION

In this chapter we shall first assess the criticism levelled against Kuhn's theory which we set out systematically in Chapter III: at the same time opportunity is afforded to further articulate our own position as stated explicitly in Chapter IV. (It may be noted that part of the criticism of Kuhn referred to has in fact been evaluated in § 11.5 above.) Thereafter we may pass to a consideration of scientific progress as Kuhn conceives it and conclude the critique commenced in Chapter III.

§ 12. EVALUATION OF THE ANTI-KUHNIAN CRITICS INTRODUCED IN CHAPTER III AS AN ARTICULATION OF OUR KUHN-INTERPRETATION

12.1 Toulmin

1. In order to evaluate Stephen Toulmin's criticism of Kuhn (see § 9, above) we shall codify that part of our list of references to Kuhn's production which is relevant to the context in the following manner.

After each respective source we shall note in squared brackets the first known year of publication[61], followed in a number of cases by the date of actual writing or the first public presentation of the material in question (many of these studies were first written for scientific congresses or academic lectures and only later for publication):

Kuhn (1957)[1957]
Kuhn (1961)[1961][62]
Kuhn (1962)[1962]
Kuhn (1963a)[1959]{1959}[63]
Kuhn (1963b)[1963]{1961}[64]
Kuhn (1963c)[1963]{1961}[64]
Kuhn (1964)[1964]
Kuhn (1967)[1967]
Kuhn (1968)[1968]
Kuhn (1969a)[1969]
Kuhn (1969b)[1969]
Kuhn (STR)[1962]{during the fifties}[65]
Kuhn (POST)[1970]{1969}[66]
Kuhn (1970b)[1970]{1965}[67]
Kuhn (1970c)[1970]
Kuhn (1970d)[1970]
Kuhn (1971)[1971]
Kuhn (1974a)[1974]{1969}[68]
Kuhn (1974b)[1974]{1969}[69]

Toulmin's first prominent criticism of Kuhn's theory appeared in Toulmin (1970). Thereafter he extended and systematized his critique in his work of the year 1972, focusing also for example on Kuhn (1970c) and proposing that Kuhn's thought involves five distinct phases of progress. It is somewhat surprising to find that although Toulmin (1972) thus takes up Kuhn (1970c), it passes in silence over the two conclusive counterstatements that work contains on Toulmin's previous work (1970). For this reason the following consideration of Toulmin's claim regarding the development of Kuhn's thought will also embrace Kuhn's reply (with appropriate references to Kuhn

(1970c)). (For obvious reasons Kuhn in writing (1970c) could not know Toulmin (1972) and to our knowledge did not reply to this at any later stage.)

Toulmin thus puts forward the view that Kuhn's thought passed through five phases (see § 9, section 2, above). Of the existence of the first such phase (*The Copernican Revolution,* (1957)) we need have no doubt, for - if we wish to distinguish it from others - it stands out by its very emphasis (stage 1 primarily exemplifies a historiography of science, whereas the works Toulmin sees as belonging to stages 2, 3, 4 and 5 are chiefly concerned with problems of the philosophy of science). As to the existence of these later phases as manifesting a change in Kuhn's thinking Toulmin is to our mind on less firm footing. By way of denying any such change we shall proceed to question whether in fact there were the stages 2, 4 and 5 Toulmin claims to discern (see also Kuhn (1970c), pp. 249-251).

Toulmin focuses particular attention on Kuhn's article (1963b)[1963]{1961}, upon which (and upon which alone) he constructs his Kuhn stage 2 (see § 9, section 2, above), and which he claims to have been "Kuhn's *first public* unveiling of the explanatory theory of revolutions"[69] (Toulmin (1972), p. 107; italics added). The fact of the matter is however this:

(i) Kuhn (1963b)[1963]{1961}, which Toulmin sees as phase 2, is a radically curtailed version of the manuscript of STR, that is, of Toulmin's Kuhn phase 3 - a fact which emerges in the first note on the first page of the article Toulmin singles out as phase 2! (Kuhn points this out on page 249 of (1970c), but without the slightest impact upon Toulmin (1972) in which the "phases" are actually constructed.) It might also be added that the précis in question matches the content of the book as fully as any précis could (for the central themes see e.g. pp. 349, 351-354, 356-361, 363, 368-369 of the article).

(ii) Kuhn had - contrary to what Toulmin in (1972) would have us believe - *publicly unveiled* the basic ideas for his model *prior to* Toulmin's phase 2 in at least two published articles, namely Kuhn (1961)[1961] and Kuhn (1963)[1959]{1959}. Thus although Toulmin is concerned to study the stages by which Kuhn's theory evolved, he is not familiar with the first of these two articles which is mentioned in the note on page 361 of Toulmin's Kuhn phase 2 (and which to our mind is in fact the most compact presentation of the theory); it is extremely odd, moreover, that in his search for stages of development Toulmin did not come across the latter article either, in spite of the fact that it is mentioned in the note on the first page of the very work he singles out as Kuhn's stage 2!

What Toulmin construes as Kuhn's second phase has thus never existed. (The criticism Toulmin makes of "phase 2" has been dealt with, see § 11.5, section 2).

In his *Kuhn phase 4* (see § 9, section 2, above) Toulmin counts "a *series* of transitional papers written [by Kuhn] in response to criticism of his theory, and delivered between 1965 and 1969" (Toulmin 1972), pp. 107-108; italics added); however, to the reader who succeeds in penetrating the technique of reference employed in Toulmin (1970) and (1972), it will emerge that this *series* of articles in fact shrinks to one single article, Kuhn (1970b)[1970]{1965}, although - but this is *all* apart from the singleton in question - Toulmin does urge the reader "See also [Kuhn (1974a)[1974]{1969}]" (Toulmin (1972), p. 108, note 1).

According to Toulmin, Kuhn's theory is watered down in the phase 4 article Kuhn (1970b)[1970]{1965}, in which "... he [Kuhn] has moved away from the original 'normal'/'revolutionary' dichotomy" (Toulmin (1972), p. 43) and in which microrevolutions are so readily attributed significance that in the end all scientific change is "revolutionary" (Toulmin (1972), pp. 112-114).

The reader is particularly struck by the realization that with regard to the article Kuhn (1970b), which Toulmin construes as phase 4, the situation is in actual fact the following:

(i) Toulmin does not found his views on this article upon source references. For example Toulmin (1972) contains *but one* apparently contentual allusion to phase 4, in a note on page 113. The attentive reader will however discover that the note in fact refers to a note on page 108 of Toulmin's *own* book containing nothing beyond bibliographical data on Kuhn's writings.

(ii) In the article in question Kuhn does *not* consider a diminishing of the conceptual distinction "normal" vs.

"revolutionary"! (See Kuhn (1970c), pp. 249-250, which made no impact on Toulmin (1972).)

As regards the "appendix" to Toulmin's "series of articles", i.e. Kuhn (1974a)[1974]{1969}, the situation is the same as that described in (i) and (ii) above, except that here Toulmin makes *not a single* reference to content, not even a reference disguised as such, in constructing his phase 4.

Like phase 2, then, Toulmin's phase 4 is a fantasy.

Toulmin's *Kuhn phase 5* (see § 9, section 2, above) comprises the articles Kuhn (POST)[1970]{1969} and Kuhn (1970c)[1970], which in Toulmin's view seal the impasse inherent in phase 4: in its concern with microrevolutions Kuhn's theory no longer deals with conceptual change at all.

In his conclusion 5 of the stages of Kuhn's development Toulmin holds that the difference between "normal" and "revolutionary" is reduced in Kuhn's thinking to what in the last analysis is a dichotomy between (T1) "*propositional* changes which involve no conceptual novelties, and so lend themselves to some kind of a deductive or quasi-deductive justification" and (T2) "*conceptual* changes which go beyond the scope of all merely formal or deductive procedures" ((1972), p. 115), so that all scientific change would involve something normal and something revolutionary. The result of this micro-revolutionary way of thinking is an ultimate watering down of Kuhn's whole theory (see § 9, section 2, above).

What scientific changes can take place in the evolution of science over long intervals of time? On the express assumption that Toulmin's category of propositional change is not void in respect of empirical scientific knowledge, we may distinguish three types of change in connection with paradigmatic theories. Let P be a paradigm and *G*(P) the corresponding paradigmatic theory. Here we may say that at least the *frozen* actualizations of *G*(P) may be employed as instruments of calculation in sufficiently restricted situations in such a manner that in Toulmin's sense we could speak of propositional change. In *normal* science, however, other kinds of change may also occur. Of the normal-scientific change brought about by the process of actualization and articulation, part is of a kind *not* to be construed as propositional in the sense intended above.

Such normal-scientific *conceptual* change includes the extension of a paradigm to hitherto unknown areas; and moreover the family of theories $G(P)$ changes *conceptually* when observations deriving from new situations are fed back into the paradigmatic theory to effect its adaptation to the new areas. The meaning of the concepts employed is in part defined, let us say as of moment i, only as a result of the actualization process in the sense that at moment i it is not possible to define the future limits of the family $G(P)$; one only passes beyond the limits attained by time-point i - if indeed one does - by means of concrete normal-scientific research.

The third type of scientific change is that conceptual change which occurs in the course of scientific breakthrough P/Q, in which P, the point of departure for actualization, is replaced by a new paradigm Q incompatible and incommensurable with it.

We may thus distinguish three types of scientific change: (N1) propositional change in normal science, (N2) conceptual change in normal science and (R) conceptual change in scientific revolution. Although Kuhn does not make the distinction (N1) - (N2) but speaks only of *two* types of change, in our terminology (N) and (R) (see Kuhn (1970c), p. 250), the tripartite division is nevertheless clearly applicable. Be it noted that the scientific changes (N1) and (N2) are cumulative, while change of type (R) entails a phase of development which is non-cumulative.

Toulmin's dichotomy (T1) - (T2) is thus not sufficiently discriminative to distinguish all three types of scientific change. This lack of adequately sensitive apparatus is apt to lead Toulmin into a search for Kuhnian criteria of revolutionary change in the wrong direction: it is not the case - even if Toulmin would have it so - that for Kuhn the "magnitude" of scientific breakthrough could determine whether what is involved is a revolution; a revolution is a conceptual change of type (R), even though not all conceptual change is revolutionary.

Toulmin's Kuhn phase 5 is therefore likewise an unfounded construct, or at least it is incompatible with our interpretation of Kuhn. Although small-scale revolutions - "microrevolutions" - indubitably form an as yet unexplored field in which the Kuhnian theory has still to be articulated, consideration of these need in no way mean that the dichotomy (N) - (R) or the trichotomy (N1) - (N2) - (R) is rejected. In this context the following is also worthy of note. Doubtless *nobody* at this moment is in a position to categorize *every* episode of change in the history of science into the dichotomy (N) - (R). But that this is so does not render the dichotomy void, because there are clear-cut instances in which "normal" and "revolutionary" types of change can be distinguished, as we have seen above.

It may also be pointed out that the revolution P/Q "concerns" - if we may put the matter so - only those who participate in the actualization and the breakthrough, that is, those who have gone native: the outsider apprehends only the *results* of these steps - it is only in a FROZEN sense that he knows what is achieved in the area in question.

In a word: with regard to Toulmin's "phases" it has to be emphasized that the distinction between three (not two!) types of scientific change cannot be based on "magnitude" of change, but in the first place on whether the change takes place within the "same" paradigm as an actualization of it (a normal-scientific change (N)) or whether it is a question of the replacement of a given process of actualization by another process *incompatible* and *incommensurable* with it, and secondly upon whether the change observed in normal science is propositional (N1) or conceptual (N2).

(Kuhn points out ((1970c), p. 249) that although the nature of microrevolutions is little known, their relevance to his theory is taken into account *already* in STR; on "smaller" revolutions in STR see pp. 6-7, 49-50, 92-93, 116. For the sake of clarity we may cite on this point *The Structure of Scientific Revolutions,* page 49: "The introduction to this essay suggested that there can be small revolutions as well as large ones, that some revolutions affect only the members of a professional *sub*specialty ..." (italics added). Note also this: "I have never intended to limit the notions of paradigm and revolution 'to major theories'" ((Kuhn (1969a), p. 412).)

No such change regarding the theory of scientific change as Toulmin would wish to discern and would see as a watering down of that theory has thus ever taken place in Kuhn's thought. The real distinction between normal-scientific change (N) and revolutionary change (R) is not reduced or watered down by Kuhn in any of the "phases", *nor* does the distinction rest upon the magnitude of revolution, as we have shown in the foregoing. As to how Kuhn's thought may otherwise have altered, this is not the place to discuss it (in this connection see Kuhn (1970c), pp. 249-250).

2. It is Toulmin's view that according to Kuhn scientific revolution P/Q entails such a thorough conceptual change that the respective representatives of P and Q cannot possibly understand each other (in the background, Toulmin would have it, is the Kuhnian idealism); and in this connection paradigms "make their own laws", the result of which is a chaos of relativism (see § 9, section 1, above).

Toulmin moreover holds that "Kuhn's position displays such close parallels to Collingwood's that a glossary can be established for translating between them" ((1972), p. 99). Toulmin indeed claims that Kuhn's theory, *mutatis mutandis,* tallies with Collingwood's thought, whose kernel he describes as follows:

> "[Collingwood] makes a fundamental distinction between [1] the *synchronic* (or *logical*) relations holding between the presuppositions of any one particular culture, phase or epoch, and [2] the *diachronic* (or *historical*) relations holding between the presuppositions of successive cultures, phases or epochs. Within a given milieu men normally share a particular constellation of presuppositions, and operate at the fundamental level within a common conceptual system; so they can discuss all their disagreements in ... *rational* terms ... By contrast, at moments of transition from one intellectual epoch to another, the strains within a given system of thought become insupportable and need to be removed. When this happens, the *absolute presuppositions* of the era are themselves called in question, and normal rational *debate* ceases to be possible (*Ibid.,* pp. 99-100; figures in squared brackets and italics added)

In Toulmin's translation between Kuhn's and Collingwood's thinking (*ibid.,* see esp. pp. 100-101)

(KC1) the Kuhnian normal-scientific change is made to correspond to Collingwood's synchrony; and

(KC2) the Kuhnian revolutionary change is made to correspond to Collingwood's diachrony.

Thus Toulmin - thinking also that "a paradigm has the same logical role as a constellation of absolute presuppositions" (*ibid.*, p. 100) - draws a parallel between *scientific* paradigmatic change and Collingwood's change in presuppositions:

> "At this fundamental level, conceptual changes can be discussed only in terms of unconscious thoughts, socio-economic influences, and other such causal processes." (*Ibid.*, p. 100)

This is in fact Toulmin's thesis of irrationalism as applying to Kuhn. In our view this thesis, together with the above-mentioned conceptions Toulmin entertains regarding Kuhn, derive from the fact that Toulmin takes one of Kuhn's metaphorical statements literally. This "metaphorical equivalence" of Kuhn's concerns the nature of paradigmatic starting-points: we say now that only in a metaphorical sense these may be placed on a parallel with logical truths (consult again §§ 5 and 6, above). The purpose of this comparison is to give *some* indication of the nature of paradigmatic points of departure, *for example* to show that they are not a target for *modus tollens*. It is *not*, on the other hand, the purpose of the "equivalence" to say that paradigmatic matrices are in *every* respect comparable to logical truths, least of all to say that they *are* logical truths (see Ch. IV, above). This and certain other observations indeed serve to render the irrationality thesis and conditions (KC1) - (KC2) questionable. *In the first place* the function of normal science is not *debate*, it is the actualization of a paradigm. *Secondly*, normal-scientific change is not unexceptionally synchronic - though Toulmin claims it is - on the contrary, a significant part of normal-scientific change is diachronic (historical) change, in which a paradigmatic theory adapts itself with unprecendented precision to hitherto unexplored areas (a process which is not of a synchronic (or logical) nature in any normally accepted sense). *Thirdly*, the non-absolute, empirical character of the paradigm is manifested as success in actualization, failure where anomalies arise and - precisely in revolution, where for *rational* reasons an old paradigm is replaced by a new (one such reason is that the new paradigm solves at least some of the anomalies left by the old). *Fourthly*, as just introduced, paradigmatic change is effected by *rational*, persuasive argumentation applying the criteria of the values of the paradigm. In the application of values (e.g. problem-solving capacity and precision) it is a prerequisite that both paradigms *have* achieved something. Achievements, again, are the result of actualization. In *debate* upon choice of paradigm one thus applies viable criteria to *objective results* which are not the outcome of debate or disputation but of actualization (read: basic research). Although these values do not provide an algorithm for selection of paradigm, the selection event is thus clearly a rational process (in contrast to what Toulmin's irrationality thesis would imply).

Toulmin's Kuhn-Collingwood parallel, and the accompanying claim that no communication whatsoever can take place between paradigms, are thus without support.

Kuhn elucidates the change taking place in a scientific breakthrough in terms of a gestalt switch (the D/R effect as we have named it; see § 6, section 19, above). This approach of Kuhn's to the question of scientific revolutions and the analyses he makes in the context are apt to lead to the conclusion - Toulmin's conclusion - that Kuhn leans upon an idealist theory of knowledge. In order to counter this accusation, however, we only need to refer in the first place to our argumentation introduced above in the Kuhn-Collingwood context, and secondly to note that comparison of the breakthrough P/Q with the D/R effect is a metaphor which is not to be taken literally. If this metaphorical equivalence is so taken, the cognitive function of it is being misconceived, as it is for example when one attempts to "falsify" the metaphor "Man is a wolf" by pointing out that man does not have four legs. As to the taunt of idealism no more needs thus to be said.

We have in § 10 above considered the long-drawn-out process of scientific change of which the establishment of Copernicus' astronomy was a part. Although our chief concern in that connection was to bring out the points of discrepancy between the old way of thinking and the new one, it is quite clear that in Kuhnian terms that breakthrough was an event in which the concrete achievements and the problem-solving potential of the new approach afforded sensible grounds for choosing it and abandoning its predecessor.

The aforesaid may suffice as an assessment of Toulmin's views on the question, though more in fact will be said on the problem of relativism in § 13 to come.

12.2. Shapere

1. To the initial criticism of Dudley Shapere, which focuses on STR (see § 8, section 2, above), our considerations suggest the following reply.

To begin with, Kuhn's theory need *not* - though this is clearly what Shapere insists upon - be "extracted from a mere [!?] investigation of how things *have* happened" (Shapere (1964), p. 386); or does Shapere require that although natural scientific theories cannot be thought to be "extracted from a *mere* [!] investigation of how things *have* happened", such "extraction" must in these case of theories of the philosophy of science be possible? Does Shapere mean that the claim of the philosophy of science that natural scientific theories cannot be "extracted from a *mere* investigation of how things *have* happened" must itself be "extracted from a *mere* investigation of how things *have* happened?"

It must secondly be pointed out that Kuhn's theory, by reason of its diachronic aspect (which embraces conceptual change on the one hand in normal science and on the other in scientific breakthrough; see § 12.1., above), diverges fundamentally from the *conventional* view, which also stresses the function of basic sets of concepts in channelling the nature and course of research. This is the case because in the conventional way of thinking this function of the terms from which research sets out is usually restricted to the propositional aspect and to a synchronic context. The paradigm theory, as a theory which takes account of the various aspects of the diachronic perspective, is better able - though Shapere would have it otherwise - than the conventional model to illuminate the nature of scientific change, as we have seen.

Shapere's comment to the effect that the ease of locating and the difficulty of precisely defining a paradigm constitute a point upon which Kuhn fails to come to terms with himself, is to some extent justified, but the conclusion he draws from this is not: in the history of the natural sciences for example, we may with ease put our finger on this theory and that, yet the *concept* of theory is not hereby defined in such a way as to cover satisfactorily the nature of all theories thus pinpointed - and *for all that,* we do *not* imagine that the term 'theory' covers anything and everything as long as it allows the researcher to do something, and that a theory of theories is just void.

Although Shapere himself in a number of contexts stresses the need to take account of the dynamic, time-dependent aspect of theory development, he seems nevertheless to demand that Kuhn's philosophical theory be ex origine complete and that it should be capable of resolving every problem it has ever encountered, including for example the problem of defining the paradigm concept. Finally the fact is that the term paradigm - contrary to what Shapere conceives - *has* a relatively unambiguous meaning, or can be given such, as we above have seen.

Further diverging from Shapere's conception, the matter of choice of paradigm has its rational basis within the framework of the set of values applied without the necessity of abandoning the incommensurability thesis. Thus Shapere's reference to chaotic relativism is largely unwarranted (we shall return to the subject later). Shapere seems not to have observed that Kuhn had *already* analysed the function of the constellation of values governing choice of paradigm in STR, not exclusively in his "altered later phase" (see (STR), e.g. pp. 42, 79, 110 and 153-159).

Finally, although Kuhn in his parallel between breakthrough P/Q and the D/R effect employs expressions which may smack of idealism, it must be borne in mind that he is speaking metaphorically in a manner which should not lay him open to the accusation of idealism as such (see above), as is also implied in STR:

> "... though the world does *not* change with a change of paradigm, the scientist afterward works in a different world" (P. 121; italics added)

2. With regard to Shapere's characterization of Kuhn's theory as having changed markedly by 1970 (chiefly in the articles Kuhn

(POST), (1970b) and (1970c)) and his criticism of the "changed" theory we have the following to say (the numbering in what follows corresponds to that of Shapere's critique (i) - (vi) in § 8, section 4, above):

(i) It is true that Kuhn later (e.g. in (POST)) presents the paradigm as a matrix specific to respective disciplines, bringing with it symbolic generalizations, metaphysical concepts, exemplary instances and governing values for science. In this, however, it is not to our mind a question so much of a change in his conception of the paradigm as a clarification of it, for the group of factors mentioned appear in a paradigmatic function quite definitely as early as in STR (see on each group of factors in the order introduced e.g. pp. 5, 10, 40, 78, 103, 109, 133; vii, 5, 7, 40-41, 102-103, 109; viii, 10-11, 23, 46-47; and 42, 79, 110, 153-159). In particular it must be emphasized that the function of values is already examined in STR, so that in this connection *no change* in principle has taken place in Kuhn's thinking. Nor does the introduction of matrices alter the categorization of scientific change or the thesis of incommensurability, so that the fundamental conceptual, distinctively Kuhnian differentiation between normal-scientific and revolutionary change remains. When in addition the normal-scientific diachronic perspective is likewise unaltered, Kuhn's view cannot be said to have undergone any shift in the direction of traditional thinking according to which the governing function of the initial conceptual equipment devolves to a mainly propositional level. (As to how Kuhn's view has possibly altered in other respects need not detain us here.) The conception of the paradigm as a matrix can also presumably scarcely eliminate the holistic nature of the paradigm, for especially if we define the paradigm as an ordered n-tuple, for example as an ordered quadruple, the holistic interdependence of the components in the matrix is stressed in the sense brought out above.

(ii) It is misleading to say that Kuhn had by 1970 withdrawn from the extreme view of STR to the conventional stand which accepts the objective world (*nature*), which poses the *problems*, for precisely such a stand is not only an explicit factor but also a constitutive feature of normal science in STR:

> "*Nature* itself must first undermine professional security by making prior achievements seem *problematic*." ((STR), p. 169; italics added)
>
> "The scientist *must* ... be concerned to solve problems about the behaviour of nature." (*Ibid.*, p. 168; italics added)

As has been shown in the foregoing, the fact that Kuhn in his "new" phase appeals to values, is *nothing* new (see (STR), esp. pp. 153-159). The application of values, again, does not necessitate abandonment for example of the incommensurability thesis, normal-scientific diachronics or scientific revolution, so that the claim of a return to conventional viewpoints can only mislead.

(iii) The adaptation of value constellations to concrete situations may lead to conflicts. To the question of whether, as Shapere would have it, Kuhn's "new" theory therefore leads likewise to extremes of relativism, we shall revert in § 13.

(iv) The interparadigmatic communication prerequisite to the application of value constellations is *not* in conflict with the thesis of incommensurability, not at least with the version of that thesis envisaged in the present work.

(v) In a word: Kuhn does not shift from "an extreme stand" to a conventional one, since his stand is not "extreme" and since it is in every "phase" anything but conventional.

(vi) Kuhn's views do not lead into extreme sociologism, as Shapere imagines they do ,for taking account of the nature of actualization, of extraordinary research and of scientific revolution, we may say that indeed "scientists proceed as they do because there are objective reasons for doing so". This objectivity does not, however, "proceed from nature" defined by nature alone (objectivism). Nor, on the other hand, is it the case that scientists proceed as determined by a "given" set of basic concepts whose content is clinched in advance (possibly exchangeable in their atomic components with new "frozen" elements as the situation requires), approving only of what fits this mould and rejecting all which does not (the conventional notion of the function of basic conceptual equipment as determining the course of research). Rather it should in our opinion be said that the concepts science sets out guide research while conversely the process of research, as a function of time, "guides" the cognitive significance of the terms it operates with.

12.3. Feyerabend

1. In the interpretation of Paul Feyerabend (see § 7.4, above) Kuhn's claim that normal science is an essential condition for revolution is divided into two claims: (i) the paradigm is an essential conceptual prerequisite to high-standard research; and (ii) in high-standard research one can build only upon one paradigm.

Feyerabend (like many others) is of the opinion that claim (i) is undeniably true, but that it contains nothing new, because even in the completely conventional view a basic set of concepts (Kuhn's paradigm) is indispensable to research. Thus only (ii) introduces anything new, i.e. the stand that research can set out from only one paradigm.

This cliché, however, of the governing function of initial conceptual equipment in research activity, must always be more explicitly defined. In Kuhnian thought this is done in such a way that the outcome is a position at odds with the traditional and conventional view, as we have seen in the discussion hitherto. Feyerabend's criticism is thus misleading.

Claim (ii) Feyerabend further subdivides into two components: (iia) a *descriptive* reading of Kuhn: in mature science *de facto* research has always been monoparadigmatic; and (iib) a *normative* reading: confinement to a single paradigm is to be *recommended*.

The former reading (iia) is in Feyerabend's opinion untenable, firstly because it leads to the absurdity we call here the Dillinger dilemma (see § 7.4, section 2), and secondly because there can in fact scarcely be any such thing as normal science (see § 7.4, section 4). The Dillinger dilemma we resolved above. As to the existence of normal science we may refer to what has been said in our work so far (see also, however, section 2 below): there is such a thing, and its nature diverges from the characterization of it to be found in the critics (for example in Feyerabend).

The normative reading (iib) Feyerabend further subdivides into two questions: (iibI) Are revolutions desirable? (iibII) Is the way in which confinement to one single paradigm produces revolutions desirable?

According to Feyerabend (iibI) cannot be answered within the Kuhnian framework:

> "Now I do not see how the desirability of revolutions can be established by Kuhn. Revolutions bring about a *change* of paradigm. But following Kuhn's account of this change, or gestalt-switch as he calls it, it is impossible to say that they have led to something better." ((1970a), p. 202)

Here, however, Feyerabend overlooks the fact that the incommensurability thesis can be retained, while allowing comparison and value-based persuasive argumentation between paradigms. Before passing on to claim (iibII) we shall devote the following section to a consideration of the monopoly-proliferation problem in the light of Kuhn's theory.

2. The problem of whether (one and) only one paradigm can prescribe the course of research is a difficult one - its analysis is rendered especially difficult if the matter is dealt with on an instant question - instant answer basis. In general - so we may say - research can be guided by only one paradigm. It must however be noted in particular that though we grant that

> "... there are circumstances ... under which two paradigms can coexist peacefully ..." (Kuhn (STR), p. ix),

this does not entail abandoning the theory of Kuhn procedure (see also Masterman (1970)); what is involved is the pinpointing of a problem for the philosophy of science; in connection with this problem Kuhn's "philosophical paradigm" may be further articulated (at least as long as the problem in question is not shown to be an anomaly of that paradigm).

If it were indeed the case that theories were such entities as the traditional conception of science (e.g. that of Popper or RV) envisages, the requirement of theory proliferation would be understandable enough. But here we have the heart of the matter: the development of a scientific theory is not simply the presentation of assessments and generalizations and their (independent) testing but a complicated process of actualization which has to be followed through on the strength of the assumption that the theory in question possesses a potential which can be actualized. Not wishing to repeat what has already been said so far, we may content ourselves now with the following observations, in the light of which the proliferation Feyerabend and others require appears in the *general* case unrealistic:

(i) The emergence in the course of history of theories regarding the motion of solid bodies (see § 10, above) exemplifies extremely well a point which Feyerabend (and many others) forget: In a period of over two thousand years only a few viable alternatives for the analysis of the movement of solid bodies have been forthcoming. For example Popper's claim (and Feyerabend implies the same) that we may at any moment break out of the old mould only to find ourselves in a new one, is quite naive, as may be illustrated thus: set a haughty Popperian the task of breaking out of the finite series of conceptions whose one member is the Aristotelian view, another the Galilean-Newtonian paradigm and another the Einsteinian matrix, and of telling us what more spacious *theory* (not speculation) on the movement of solid bodies he has come upon, and see what he will answer.

(ii) In the absence of a point-by-point comparison method for *genuine* theories, any talk of all possible theories explaining certain fixed matters gives an erroneous conception of the nature of scientific theories - a conception in which the demand for proliferation would indeed be natural; the problem is that that conception itself does not meet the challenges of the philosophy of science.

(iii) Only when actualization has in the case of a given theory proceeded far enough can we say (a) *why* a new theory must be found; and (b) *what* this new theory must answer. The basically Popperian demand for proliferation misses the point with regard to the nature of the growth of empirical knowledge. The development of empirical knowledge does not proceed by the formation, testing, acceptance or rejection of theories explaining the data analogically to what is prosecuted deductively in logic and mathematics, for instance in the mathematical theory of generative grammars, in which "proliferation" is just to the point. In contradistinction to mathematical theories empirical sciences are concerned with reality, existing independent of us. Only when some aspect of reality, as it is seen in the light of a paradigm, has actually been handled can we know (a) that we have to do with a paradigm, not speculation; (b) that this paradigm enables us to go on in actualization; and possibly (c) that for this or that reason certain counter-examples are anomalies of the theory in question, for which a new paradigm must be found. This latter knowledge, however, that there are unsolved problems, is (almost invariably) possible only after the paradigmatic theory in question G(P) has been actualized up to the point at which these problems emerged; usually these are not in fact problems at all until G(P) has been actualized to these points of which we have hitherto remained unaware, that is, to these anomalies. Usually it is only when such points have by means of G(P) - speaking in metaphor - been given coordinates, that the *demand* for a new theory G(Q) can have content, and likewise usually only then can the *theory* G(Q) have content. The demand for proliferation contains precisely this unrealistic stand: it envisages the instigation of new theories without on the one hand the *reason* provided by the predecessor, and without, on the other, the *impulse* only this can give. When G(P) has brought out anomalous points, there is reason to look for a new theory, but at the same time G(P) has become a springboard from which to set out on this search, for without the questions raised by the earlier no one could understand what the successor is supposed to answer.

3. Bearing in mind what has been said in this work by way of interpreting Kuhn (see esp. also section 2, above), we must now point out that Kuhn's theory *itself* is neither a descriptive nor a normative system, it is a theory about the nature of empirical scientific theories and about their development, a theory which operates at the level of the philosophy of science. Thus (a) descriptions can be given on the basis of it, whereby it receives articulation and modification; and (b) it opens up perspectives from which normative conclusions may be drawn. The monopoly-

proliferation problem does not therefore lie on the descriptive-normative axis: Feyerabend's question (iibII) - is the way in which confinement to one single paradigm produces revolutions desirable? - is not to the point, for if the practice of science *is* successful and if Kuhn is right, the form scientific development takes cannot be influenced by acts of will. Feyerabend's accusation of crafty contrivance between descriptive and normative theory is thus unwarranted: a paradigm theory which functions as horizon for descriptive theories and impetus for normative conclusions is *in itself* no more descriptive than normative.

The above-said will explain why it is that Feyerabend fails in his attempt to articulate by examples his "Kuhn theory". The example pertaining to the descriptive aspect (the Dillinger dilemma) has been conclusively dealt with. As to the *normative* aspect, Feyerabend makes a thought experiment which - so he claims - shows Kuhn's model to lead to this: on Kuhnian criteria a non-mature discipline can apparently be rendered mature by weeding out all points of view in that discipline except one and proceeding with all the operations which such a "reduction" requires. Feyerabend fails, however, to realize that the outcome of such an artificial procedure is not a paradigm imbued by its very nature with various capabilities as generator of a family of theories (e.g. functioning examplar(s) and problem-solving capacity), but a system of beliefs whose imposition would force the community, in disregard of the actual pre-paradigmatic phase of the science in question, to live *as if* the paradigmatic phase had been reached. This would *prevent* that science from ever reaching that phase, for in this false mode of community life it is more than likely that the first paradigm proper would never emerge. Such an act of compulsion would thus be apt to severe the line of development of a discipline towards its mature stage and would thus be at odds with the very principles upon which *Kuhn's* own model functions. In reply to Feyerabend's remark

> "... I am fortified in my behalf by the fact that almost every reader of Kuhn's *Structure of Scientific Revolutions* interprets him as I do ..." ((1970a), p. 198)

one can only say that he and his "almost every reader of Kuhn" have succeeded in turning matters upside-down.

12.4. Lakatos

1. Imre Lakatos' interpretation of Kuhn's theory (see § 7.3, above) contains an example of astronomers being converted in a Kuhnian crisis and revolution to astrology: Lakatos seeks here to demonstrate the untenability of Kuhn's mode of thought. The example is indeed illuminating - not in the sense that it has anything to do with Kuhn's theory but in that it reveals the extent of Lakatos' misconstrual of it: (i) in a situation in which astronomy is making progress it cannot occur that (ii) a crisis arises, for crises arise for objective reasons (anomalies remain unresolved, problem-solving meets with continued setbacks, new actualizations cannot be found); and finally (iii) even a genuine Kuhnian (not Lakatosian) crisis cannot result in astronomers transferring allegiance to astrology, in whose framework actualization - as far as the present writer's knowledge serves him - is not possible.

2. This example of Lakatos' rests upon the idea that the onset of a crisis is an irrational event. But since also, according to Lakatos, criticism is prohibited in normal science and standards are prohibited in revolution (ultimate truth lies in power), Kuhnian development of science at every stage (in normal science, extraordinary research and scientific revolution) is an irrational event which can be examined only in the terms of psychology, sociopsychology and sociology (if Lakatos is right, in the same manner as religious movements are studied in the psychology or sociology of religion). The phase of irrationalism in normal science, that phase where standards must not be criticized, would however constitute scientific progress: according to Lakatos it is in fact the only mode of scientific progress Kuhn's system envisages.

This interpretation of Kuhn's account of progress in science is as far from appropriate as can be: normal science, crisis science and scientific revolution are functional components in scientific progress, and correspondingly, normal-scientific change and revolutionary change are indispensable functional elements of it, so that from the standpoint of scientific progress it is impossible to say which is more "important", normal-scientific change or a breakthrough proper.

In the phase of paradigmatic development scientific change can be seen to fall into three main types: propositional change and conceptual change in the normal sequence of actualization

(12.1) ... $G_i(\mathrm{P})$, $G_{i+1}(\mathrm{P})$, ...

and conceptual change in a scientific revolution

(12.2) $G(\mathrm{P})/G(\mathrm{Q})$.

But now, contary to what Lakatos proposes, normal science, crisis and its reasons and scientific revolutions involve no irrational factors, for:

(i) The achievements in the sequence (12.1) are objective and they are effected on rational criteria of a P-laden nature. This loading in no way eliminates or threatens its objectivity: the procedure can for example be repeated (consensus is a manifestation of objectivity, not of power policies). Further it is rational to proceed with the actualization of the sequence (12.1) even though in some specific area of application anomalies make this impossible.

(ii) The factors underlying a crisis (anomalies and recurrent failure of attempts at problem-solving) are not irrational; apprehension of the failure of a paradigmatic theory and the transition to crisis science are likewise rational. Even crisis, then, involves nothing irrational.

(iii) Neither is there anything irrational in scientific change (12.2), because choice of paradigm is undertaken with reference to definable grounds. The paradigm Q is by reason of its *achievements* and the *promise* it holds superior in terms of the set of values applied to P: the paradigm Q allows of the solution of at least some anomaly of P, Q retains a significant proportion of the solutions attained by P and further will usually offer *prospects* of better success than P. (This superiority of Q applies (of course) also to its relationship to other possible paradigm candidates $Q_1, \ldots, Q_n$ emerging in the phase of crisis.) These grounds for choice are rational, and the fact that in the selection process (12.2) one appeals to a constellation of values does not mean degeneration into sociologism, for it is a condition of application of values that the objects evaluated (the theories) have displayed success measurable by these criteria, albeit with varying weight. (If this were not the case, value constellations could not be applied.)

Such a set of values does not, however, afford an algorithm for selection: in this sense Kuhn *is* a relativist. The absence of an algorithm, on the other hand, does not allow one to say that selection takes place on the basis of anything but good reasons and rational argumentation.

3. If the concept of truth is applied, it must follow from the above that in Kuhn's theory truth is not a matter of dictates or arbitrary taste; the structures of actualization and of revolution are independent of will. What is further striking in Lakatos' "power theory" critique of Kuhn is that at bottom Lakatos' *own* theory makes truth a matter of power - if not indeed a matter for Lakatos himself - for to protect the core of a research programme he needs a norm or directive (negative heuristics) whereby the core *must not* be falsified.

Kuhn's theory does not constitute a passage from analysis of the entities in Popper's world III to the sphere of Popper's world II. Kuhn's theory of scientific change is such that one may well speak of *Kuhn's* world III.

All in all: Lakatos' attempts to boil Kuhn down to a matter for social psychology or sociology fall short of their objective.

4. According to Lakatos "Kuhn is right in objecting to naive falsificationism, and also in stressing the *continuity* of scientific growth, the *tenacity* of some scientific theories" ((1970), p. 177). In point of fact, however, Lakatos' own theory was constructed in an attempt to meet precisely those challenges which Kuhn's theory implied for the traditional conception of science; what is astonishing here is that Lakatos' own views are particularly Kuhnian in their

objectives (see e.g. *ibid.*, pp. 93, 155, 159, 177 and *passim*), even to the point of his asserting:

> "Indeed ... *my* concept of a 'research programme' may be construed as an objective 'third world' *reconstruction* of Kuhn's socio-psychological concept of '*paradigm*'..." (*Ibid.*, p. 179, n. 1; original italics partly deleted)

Lakatos appeals, moreover, entirely after the manner of Kuhn himself, to the history of science, to show among other things that "tests are - at least - three-cornered fights between rival theories and experiment ..." (*ibid.*, p. 115).

Lakatos' own view, however, is ultimately based on the traditional conception of science, whereby scientific theories are exhaustively sets of statements having specific structural and functional properties (see Ch. I, above). On the other hand, however, he endeavours at the same time to take into account points which Kuhn puts forward and substantiates, for example to the effect that theories are not abandoned in consequence of counter-examples in the manner falsificationism envisages (see e.g. *ibid.*, p. 115). Attempting thus to reconcile the traditional view of science - to which he himself subscribes - and the conception of the history of science propounded in the new philosophy of science (by Kuhn in particular), Lakatos arrives at his own theory, sophisticated methodological falsificationism, where in fact one is *forced to prevent* by methodological norms the falsification of the core to ensure that the methodology and the actual development of science are brought into some degree of harmony - this because Lakatos conceives of a theory as a set of statements (or alternatively a conjunction of statements), so that the core cannot be considered a core otherwise than by forcible means. Contrary to what Lakatos assumes, however, such a procedure does not yield a Lakatosian reconstruction of the paradigm concept (nor of the concept of paradigmatic theory): this we feel has been implied in the foregoing considerations, which it will hardly be necessary to repeat here.

12.5 Watkins

1. John Watkins' critique (see § 7.1, above) seeks among other things to show that Kuhn's theses of incommensurability and incompatibility are - to use an expression appropriate to the context of the present work - mutually *incompatible*, in other words that Kuhn's own theory is incoherent and untenable. The example Watkins employs to show that incommensurable notions cannot at the same time be incompatible is this:

> "If someone holds that, say, Biblical myths and scientific theories are incommensurable, belong to different universes of discourse, he presumably implies that the Genesis account of the Creation should *not* be regarded as logically incompatible with geology, Darwinism etc.: they are compatible and can peacefully co-exist just because they are incommensurable." ((1970), p. 36)

On the other hand, as Kuhn uses the term, incommensurable notions (Newton vs. Einstein, cf. § 10, above) are *rivals*, so that there can be no question of peaceful co-existence (*ibid.*, p. 36).

Watkins' example is in itself inappropriate in that the incommensurability thesis applies to actualizable paradigms and empirical paradigmatic theories, not religious myths. Moreover, although Watkins in fact realizes the relevance here of Kuhn's statement that what "emerges from a scientific revolution is not *only* incompatible *but* often incommensurable with what has gone before" (*ibid.*, p. 36; italics added), he nevertheless fails to appreciate that successive paradigmatic theories apply in part to the same area: a succeeding theory solves at least some anomaly in its predecessor, so that the two are incompatible and deal (in part) with the same phenomena. In spite of this the theories may also be incommensurable: their processes of actualization are incommensurable and no point-by-point comparison can be made between them as far as they are left to be genuine theories (see Ch. IV, above).

2. Watkins' article prompts the following further comments:
(i) The claim that in normal science testing is prevented by psychological and sociological means must be considered from a

variety of angles. To begin with, neutral testing in the traditional sense is excluded, but this exclusion does not derive from the community factors Watkins intends but from the fact that (in paradigmatic research) neutral testing and theory evaluation does not exist. Secondly - and in contrast to what Watkins claims - specifically in normal science testing is carried out in the following sense. With regard to an envisaged addition of specific hypothesis γ to the set Γ in a given actualization $T = [P, \Omega, \Gamma, METR]$ of the theory $G(P)$, testing is undertaken analogically to the conventional mode. However, the testing is not neutral but P-laden. Nonetheless the acceptance or (of course) the *rejection* of γ is in no respect an arbitrary matter. In the sense described, specific hypotheses are tested, so that such a procedure is in no way excluded in normal science. Thirdly, it is not the task of normal science to test *paradigms*; its task is to actualize them: of prime importance from the paradigmatic standpoint is the making of new discoveries with the aid of some paradigm, not the testing of that paradigm. Fourthly and finally, a part of the paradigmatic points of departure are not amenable to testing at all in the above sense: there is thus no question of preventing their testing, since no obstacle to it could even be devised. (If for example a gradually extending failure in the problem-solving process, leading eventually to the overthrow of a theory in a scientific revolution, is *defined* as testing (with some kind of "falsification" as its outcome), even this is not prevented: on the contrary, it is precisely the process of actualization which leads to these results (see § 11.4, above)).

(ii) It is not true to say that for Kuhn normal science is "normal" and crisis science "abnormal" (to the extent that only normal science is proper science): scientific revolutions are an indispensable condition of scientific progress.

(iii) It is not true to say that Kuhn "evaluates" normal or crisis science (one way or the other): he is concerned to reveal the structure of development in science.

(iv) It is not true to say that Kuhn identifies science with normal science, which Watkins regards as a phase of stagnation.

(v) It is not true to say that normal science is a phase of stagnation.

(vi) Nor is it true that phases of theoretical stagnation (*such as indeed occur*) constitute normal science: from being normal science a theory which has reached this stage of stagnation becomes a matter of "engineering".

(vii) It is not the case that Kuhn dislikes scientific revolutions, but also (of course) not the case that (as a theoretician) he likes them.

(viii) Watkins' chain of conclusions (see § 7.1, section 4) rests upon his claim that in Kuhn's theory no paradigm whatsoever has a prehistory, that paradigms are discovered at the drop of a hat and ready-made. Watkins holds such a claim to be untenable, for

> "I [Watkins] do not know how much a *single genius* might achieve *in the middle of the night*, but I suspect that this thesis [that paradigms are discovered all at once and complete] expects too much of him." ((1970), p. 36; original italics removed, new added)

Watkins reaches his conception apparently by taking literally the metaphorical parallel between revolution and gestalt-switch. On the basis of the considerations set out for instance in § 10, however, it will be clear that Kuhn's theory contains no such "instant postulate". Be it finally added that it is precisely Kuhn who points out - contrary to what has usually been assumed - that it is often impossible even in principle to decide which (*single*) scholar was responsible for a given invention and when the discovery in question was made (see e.g. (STR), pp. 52-65); moreover it is precisely Kuhn who says that

> "... a new theory ... is seldom or never just an increment to what is already known. Its assimilation requires the reconstruction of prior theory and re-evaluation of prior facts, an intrinsically revolutionary process that is *seldom* completed by a *single man* and *never overnight*" ((STR), p. 7; italics added)

and - for instance - that

> "... Newton's second law of motion ... took *centuries* of difficult ... research to achieve ..." (*Ibid.*, p. 78; italics added)

12.6. Popper

1. Popper's critique has for the most part already been assessed in various contexts in the foregoing (see, however, in connection with relativism § 13 to come).

By way of summarizing we might draw attention to the fact that Kuhn's rejection of the Popperian falsification approach does not imply catastrophic irrational consequences. *In the first place modus tollens* cannot be applied to a paradigm (or a paradigmatic theory) in the sense that we could envisage the paradigm (or paradigmatic theory) as thereby falsified. *Secondly,* it is rational and non-dogmatic to retain a paradigmatic theory whose prospect of actualization still holds and whose counter-examples cannot with certainty be classified as anomalies. *Thirdly,* it is neither irrational nor dogmatic to actualize a paradigmatic theory successfully in some given area, even though in some other area it may have encountered anomalies. *Fourthly,* abandonment of a theory in crisis - when no alternatives are forthcoming - is if anything irrational: *neither* irrational *nor* dogmatic is it to cling to the only theoretical alternative in sight.

§ 13. KUHN'S 'RELATIVISM'

1. Successive paradigms are comparable in respect of their achievements in the manner described. Kuhn's theory does not therefore represent the chaotic scientific relativism of which he has been accused (e.g. by Popper, Shapere, Toulmin, Feyerabend and Lakatos).

In making comparison between paradigms on the basis of results attained, appeal is made to certain values which function as criteria; for example, a theory should be simple, it should reach beyond its original area of application, it should be as precise as possible, fruitful (in particular, it should generate surprises), and consistent with knowledge acquired hitherto (as with itself). Applying these criteria it is possible in an actual succession of theories to distinguish them in order of descent, later from earlier, without previous knowledge of their sequence (Kuhn (POST), pp. 205-206). The later theory will from the standpoint of the value constellation invoked be *superior* and therefore displace its predecessor. Kuhn is thus not (in the sense intended) a relativist even in the context of scientific progress.

We may say, then, that Kuhn's theory is *non*-relativistic - quite the contrary of what his learned critics insist - in two crucial respects: (1) comparison of paradigms as a synchronic operation is possible and is carried out on rational grounds; and (2) from the diachronic viewpoint, the latter of two successive paradigms is *superior* to the former, on the criteria or values of evaluation applied, in respect of its problem-solving capacity.

Thus we may write off as misconceived the accusations of chaotic relativism which have been levelled at Kuhn.

Kuhn's theory *is* on the other hand relativistic in some senses, as we shall now see.

2. The value constellation does not constitute an algorithm for selection, as was pointed out above. Since, in addition, no higher second order criterion can be formed for the application of these values, the result - at least at first glance - is a relativistic position. This relativism is, however, seen to be qualified when we answer the following questions. What success has the value constellation met with in application down the centuries? Could those values be eliminated - or what would be the effect of eliminating a given value as against its remaining in force but with different individuals attributing different weighting to it?

History shows that the constellation of values has been most delicately applied (see e.g. Kuhn (1973) *passim*), and indeed with such success that, for many, science has become the yardstick of progress (in which case scientific progress need not be defined at all).

Different individuals may - and do - apply the values in different ways. However, this does not mean the chaotic relativism Shapere imagines, as we may see from the following. *In the first place*, normal science is governed by consensus, which, bearing in mind the process of actualization, shows that common agreement is reached on objective grounds. *Secondly,* although different individuals may, especially in a phase of crisis, *weight* the values differently (without ignoring any of them), it does *not* follow that application of them can lead to chaotic relativism. This we may appreciate if we imagine what would happen if values were applied without any attempt to balance them (see § 11.4, section 1, above), or particularly if we imagine the effect of removing one of them from the constellation altogether or applying one to the exclusion of all the rest. If for example the requirement of novelty were removed and replaced with the requirement of exactitude excessively emphasized, the progress of science would be halted and science would become a conservationist movement. With the requirement of novelty as sole value, again, the consequences would be catastrophic - empirical science would be doomed to retrograde to the pre-paradigmatic phase. Thus: although different individuals may apply the criteria of the value constellation with varying emphasis, this does not render those criteria meaningless in the development of science, for if any one of them were to be removed, the result might be something other than science; at least, for example, removal of the requirement of problem-solving capacity would wipe out science as we know it (see also Kuhn (POST), pp. 208-209).

And finally: application of the value constellation does not ultimately involve relativism, *if* we take the following realistic stand: the function of this set of values is an ingredient indispensable to the progress of science - how could it then reflect *relativism* if without its balancing effect the progress of science would be halted! For the rest, we may content ourselves with the following comment. The values in question are applied in a concrete situation determined by the scientific knowledge acquired up to that point in time and by the anomalies known on the basis of that body of knowledge: it would be irrational to insist that in such a situation it be possible to lay down in advance algorithms for procedure in future situations which within the framework of present knowledge cannot even be conceptually expressed. The fruitfulness of applying the value constellation lies precisely in this, that different weightings can be given to its various components as a function of both subjectivity and varying situation. (On the foregoing, see also Kuhn (1973).)

3. We have in earlier analyses in our work considered how the bounds of a paradigmatic theory cannot be defined without that theory losing its position as an empirical theory, that is, as a means of actualization. The openness of a theory is linked on the one hand with scientific progress in *normal science*: the cumulative development of normal science - in that it extends into hitherto unknown territory and attains to an increasing measure of precision - is progressive, as we have pointed out. In *scientific revolutions* the progress of science likewise entails the Kuhnian concept of empirical theory and the values of openness and productivity, which should be realized also in the process of adaptation taking place over protracted phases of scientific development, that is, over and beyond scientific revolutions. An anomaly of a given paradigm P represents a scientific challenge, and when the succeeding paradigm Q offers a solution to the anomaly science has taken a step forward: something that could not previously be assimilated within its framework is now part of it, even if in the process P has been lost. This loss is for its part crucial, because what is lost is a theory-specific world-picture as an instrument of actualization. The fact that the calculators of theories abandoned in the sequence of paradigms may be retained in no way diminishes the loss. This is manifest in our own examples: even though, let us say, Newton's mechanics can still be applied in flights to the moon, the passage from Newton's theory to Einstein's has meant a transition to an entirely new fundamental conception of the world order, whose actualization entails rejection of the old. This is an example of the loss taking place alongside the gains achieved in the development of science. Losses may also take place on this level:

> "In the transition from an earlier to a later theory, there is often a loss as well as a gain of explanatory power." (Kuhn (1961), p. 184)

Kuhn justifies this view of his by a variety of examples taken from the history of science (see e.g. (STR), Chs XI and XII *passim*,

(1961) *passim*; see also (1976) *passim*). The fact that Kuhn adopts this view implies a certain degree of relativism. We have also noted in the foregoing that according to the Kuhnian conception the ontologies of successive paradigms are such that we cannot in their case conclude whether the paradigms in question have brought us closer to - or further from - a knowledge of what the ultimate constituents of nature are "really" like; on the other hand an "empirical" examination of the possible convergence of successive ontologies in the actual history of science would not appear to speak for an approach to the truth (see esp. §§ 10 and 11, above).

Kuhn's theory is thus, besides being relativistic in the sense described in Section 2 above (Kuhn's "first" relativism), also relativistic in the sense of the following two theses (Kuhn's "second" and "third" relativism):

(R1) Scientific revolutions involve genuine losses.

(R2) In the case of the ontologies of successive paradigmatic theories one cannot speak of an approach to the truth.

Concerning Kuhn's relativism, *first*, let the point nevertheless be stressed that Kuhnian relativism (R1) - (R2) is not chaotic: as a matter of fact (R1) and (R2) *themselves* cannot be formulated except on the basis of the possibility of *comparison* between successive paradigms! *Secondly*, when we take into account that, in respect of the value constellation, Kuhn's "first" relativism is such that without it science could not make progress, it is further questionable whether what is involved in Kuhn's "first" relativism can rightly be called relativism at all. Is unrealistic non-relativism on this point to be so avidly sought after that scientific progress is brought to a halt? Or is the progress of science the prime concern, so that we might relativize the concept of relativism and withdraw the indictment of Kuhn with respect to his first "relativism"?

All in all, it would appear that Kuhn may be justifiably held a relativist only in the "second" and "third" sense, i.e. only in the sense of (R1) and (R2).

CHAPTER VI
REFLECTIONS ON SCIENTIFIC PROGRESS AND ON THE LATER KUHN CONTROVERSY

We have in the foregoing given our own interpretation of Kuhn's theory. Now we shall pursue the articulation of our conception in the particular context of later criticism of Kuhn, paying attention especially to certain aspects of the nature of scientific theories and to the problem of the progress of science.

§ 14. THE PARADIGM CONCEPT

> "He [Kuhn] does not know what he means by 'paradigm'; he does not know what a paradigm is ..."
> Joseph Agassi ((1971), p. 161)

1. As has emerged in the course of our discussion, some prominent critics of Kuhn regard the paradigm concept as either ambivalent or entirely void of meaning content - a conception which is faithfully adhered to in the later literature on Kuhn (see also Putnam (1981), p. 69). In addition - in addition, that is, to the view that this

concept is either equivocal or *entirely* void of meaning - many of these learned anti-Kuhnians are apparently able to show how this (non-existent) content of the paradigm concept *changes* in the course of Kuhn's production - indeed an unusual entity which in first place does not exist, and then changes into the bargain! The alleged change is said to be particularly clearly evinced if we compare STR with its postscript POST of 1969; the change rests - so these critics would have it - in Kuhn's substitution of the paradigm by a disciplinary matrix, and constitutes such a drastic modification that his theory is watered down.

If what has been brought out in the course of the present work proves tenable, the term 'paradigm' is quite unambiguous. (The fact that application of the concept may in concrete case studies turn out to be an extremely complex and difficult undertaking does not - of course - in any way undermine this.) In the sense described in earlier parts of our work the sets O, U and E of a given paradigm P = [O, U, E, V] form a holistic entity as regards both cognitive significance and testing, and set V has the function of guiding the progress of science. (Bearing in mind this holism and directive function we cannot for example endorse the analysis of Masterman (1970) any more in respect of its criticism of Kuhn than as an *apologia* for him.)

The paradigm concept is thus - if we are right - not ambiguous. Furthermore - as we have sought to bring out - we must consider less than apt those claims of a drastic change in Kuhn's outlook, for the following reasons.

Firstly, the constituents of a paradigm (at least types O, U, E and V) are clearly present in STR, as we have shown; for the sake of certainty, however, be it emphasized that the function of the set V of values is no more an innovation by the "later" Kuhn; a detailed and systematic analysis of it is likewise to be found in STR (see (STR) *passim* and esp. pp. 153-159). Kuhn (POST) and Kuhn (1973), where usually his critics at last discover the value constellation, contain in principle no additions and in this respect no change as compared with STR.

Secondly, as again we have brought out, there is no shift in Kuhn's conceptions regarding the nature of scientific change, at least none of the kind which would water down the Kuhnian concept of change in respect for example of incommensurability, incompatibility and theoretical types of change.

Thirdly, Kuhn's introduction in his later works of the disciplinary matrix seems rather as a clarification of the paradigm concept than as a fundamental alteration or retraction.

2. Assessing the relationship between the Kuhn of STR and the later Kuhn of (POST), (1970b), (1970c) and (1974a), Alan Musgrave - having first claimed that the later Kuhn loses the whole idea of normal science (Musgrave (1980), p. 41) - writes:

> "Several people criticised Kuhn for exaggerating the degree of consensus normally prevailing in the scientific community. Now although Kuhn still makes [in his later phase] it a defining condition for a community that its members are agreed on something ..., this consensus condition becomes innocuous when the scientific community dissolves into numbers of microcommunities. *Whole sciences* need *no longer* be given over, for long periods, to the articulation of a *single, universally accepted paradigm*." (*Ibid.*, p. 42; italics added)

According to Musgrave, then, (1) in the STR phase paradigms cover entire sciences, but (2) for the later Kuhn the communities responsible for paradigms dissolve into "numbers of micro-communities", which renders the consensus of normal science innoccous but *at the same time* destroys the whole idea of normal science (see also *ibid.*, p. 41 and *passim*).

It is indeed unfortunate that Musgrave should have failed to see that the paradigm concept was never at any stage intended to cover "whole sciences". A paradigm generates a paradigmatic theory, and one paradigm and its corresponding paradigmatic theory by no means necessarily encompass the whole branch of science to which they belong (and if in some particular case and in some historical period this were the case, this does not mean that the paradigm *concept* carries any such "all-embracing" clause). Musgrave has thus misunderstood the monopoly thesis pertaining to paradigms, since one and the same branch of science may have any number (there is in principle no upper limit) of *different* paradigms simultaneously undergoing actualization. A brief review of the examples used in the present work to illustrate the progress of science and certain paradigmatic theories may serve to elucidate the true content of the monopoly thesis and the error of Musgrave's interpretation. For example, the Galilean-Newtonian paradigm and the Einsteinian matrix are incompatible, incommensurable and subject to the monopoly thesis (see § 10, above): actualization may base itself - and this does not depend upon psychological factors - upon only one of these paradigms. This does not, however, mean, of course, that 'paradigm' is a concept whose realizations, e.g. the Galilean-Newtonian and the Einsteinian paradigms, dominated "whole sciences" (such as physics). This becomes absolutely clear when we remember from the examples in § 6 (Section 1) of the present work that in addition to the aforementioned paradigms in physics there runs another well-known series in the field of optics: P_1 (Newton), P_2 (Young & Fresnel), P_3 (Planck, Einstein & successors). Here, then, we have already two simultaneous series of successive paradigms within one and the same "whole science" (physics). Without further expanding on these examples, which would be quite an easy task, we may state unequivocally that the monopoly thesis does not mean the exclusive domination of a given branch of science by one single paradigm; on the contrary: if we take any given branch of science - let us say physics - then its development, *in as much as this means its paradigmatic development* - comprises in principle an unlimited number of series of successive paradigms; in other words we have a matrix

$$\begin{array}{l} P_1,\ P_2,\ \ldots \\ Q_1,\ Q_2,\ \ldots \\ R_1,\ R_2,\ \ldots \\ \vdots \end{array} \qquad (14.1)$$

where successive paradigms pertaining to the same problem field - say S_i and S_{i+1} - are subject to the monopoly thesis, while on the other hand the monopoly thesis does *not* apply to simultaneous paradigms - say P_i, Q_j, R_k, ... - dealing with different problematics.

As to the claim of Musgrave (see point (2) above) that Kuhn's "micro-thinking" renders his model (and normal science) void, we may briefly note, with reference to our Chapter V, that the three types of scientific change (propositional normal-scientific change, conceptual normal-scientific change and conceptual revolutionary change) are not related to the "size" of revolution involved, so that the nature of scientific progress and of normal science is not obscured in Kuhn's "micro-approach".

In order to clarify the paradigm concept we may take up the following critical comment of Musgrave's concerning Kuhn. Musgrave wonders how the later Kuhn can in certain normal-scientific contexts allow the controversy over theories of matter pursued in the history of physics (*ibid.*, p. 42, see Kuhn (POST), p. 180). According to Musgrave this means that the later Kuhn can countenance metaphysical reflections on and polemicization of the fundaments of science at the level of normal science - and this, of course, would be entirely at odds with the Kuhn of STR, one of whose leading ideas was that analyses of the fundaments of science do not belong to the sphere of normal science (Musgrave (1980), pp. 42-43).

In his argumentation Musgrave does not to our mind, however, take into account the fact that in normal science this waiving of inquiry into the fundamentals means expressly unreserved acceptance of the points of departure of whatever paradigm is in operation. The ontology of the paradigm - part of these points of departure - is *theory-specific,* specific to the paradigmatic theory in question. The theory-specific ontology is part of a *functioning* theory, a theory which has its actual exemplars and certain symbolic generalizations (these latter being actualized for example in those exemplars noted as model cases). These three groups of factors (ontology, symbolic generalizations, exemplars) form a holistic entity, so that - if we are right - "also" with regard to theory-specific ontologies the normal-scientific consensus obtains. *Musgrave's* controversy over theories of matter was not, at least earlier, i.e. before say 1920, a factor in the theory-specific ontology of any *functioning* paradigm - if we have interpreted Kuhn rightly (see e.g. Kuhn (POST), pp. 179-181). With respect to the later period, that subsequent to 1920, which Kuhn mentions as some kind of turning-point, the complexity of the question forbids us to take it up within the present context (see, however, *ibid.*, p. 180). Those earlier discussions, for their part, are manifestly to be regarded as disputations of a general nature, not to be identified with a functioning part in the actualization process of any paradigmatic theory. A distinction must thus be drawn between the following:

(i) The *theory-specific ontology*, part of the basis of the paradigmatic theory and together with the other of the paradigmatic starting-points giving direction to the process of actualization. This process is - *per definitionem* - the rendering actual of precisely those paradigmatic starting-points; in other words, the theory-specific ontology is a prerequisite for normal science, so that Kuhn's description of normal-scientific consensus and quasi-dogmatism is to the point also in respect of the theory-specific ontology (see however Kuhn (POST), p. 184).

(ii) The kind of *general* (e.g. *metaphysical*) *disputation* which, for instance, at least the earlier controversy over theories of matter most obviously represented. Standpoints put forward in the course of such general discourse on the nature of reality are *not* paradigmatic, because by their very nature they are/may be put forward *without* the set of exemplars E, which in normal science (as opposed to metaphysical speculation) must be functioning.

Thus from the Kuhnian, and explicitly from Kuhn's own, standpoint such an exchange of thought and polemic on the "fundaments" of science as Musgrave is concerned with is *not* in conflict with the mode of function envisaged in the concepts of paradigm and normal science: since the actualization of a fixed paradigm is independent of such general considerations and metaphysical speculations, (a) representatives of one and the same normal scientific tradition may disagree over those "fundaments" (without provoking any alteration in the actualization sequence of the paradigm they are operating with), and (b) it is equally possible for the representatives of successive, rival, and incompatible paradigms to *agree* on the "fundaments".

If what has just been said is correct, the paradigm concept receives additional clarification on whose basis Musgrave's question

"According to Kuhn's *Postcript* fundamental metaphysical controversy, either between communities or even within the same

> community, *can* accompany "normal research". This takes us far from one of his original central theses ... that in normal science fundamental metaphysical controversy ceases. Kuhn still insists that the members of a community practising normal science "take the foundations of their field for granted" ... . But now "foundations" needed no longer include metaphysics - so *what does it include?*" (Musgrave (1980), pp. 42-43; italics added)

and Musgrave's complaint

> "And yet, as we already saw with atomism, Kuhn no longer insists that metaphysical beliefs be shared by all members of a given group. This means that the "normal scientific" work of a group can contain disagreement about the interpretation of their results, about whether a proposed explanation or "puzzle-solution" is adequate, and about whether an unsolved problem is a significant one or not. Previously, Kuhn suggested that such disagreements were eliminated from normal science by consensus over "metaphysical paradigms" ... (*Ibid.*, p. 45)

receive their definite answers.

With regard to the "metaphysical" component of a paradigm, we wish next to draw attention to the view recently expressed by V. N. Sadovsky. In his critique of various aspects of the Sneed-Stegmüller formalization Sadovsky writes:

> "... it is obvious as well that at least two components of Kuhn's disciplinary matrix - the *metaphysical statement* and *values* - are not expressed in the Sneed-Stegmüller formalism. The main difficulty here is associated with the fact that in Kuhn's paradigm the scientific proper and *philosophical* (*metaphysical* and methodological) elements coexist, and formalization is applied only to contain homogeneous (in this case, scientific proper) entities." ((1981), pp. 56-57; the original italics removed, new added)

It is evident that Sadovsky here - speaking of the metaphysical component as philosophical - adopts such a view of the paradigm concept that our above critical comments on the over-philosophical interpretation of paradigms are equally applicable to Sadovsky (our conception is supported by his subsequent remarks, pp. 59-60).

3. The view has been put forward that natural languages are exemplary cases of Kuhn's paradigm concept; for example:

> "The ordinary language in which we, and *mutatis mutandis* men of other cultures and environments, describe the physical objects and persons which we find around us, may be claimed to be like a large-scale example of one of Kuhn's 'paradigms' in that it enshrines and encapsulates a set of general expectations about how things will turn out, about the general ordering of the world of our experience as extended in space and time." (Meynell (1975), p. 83)

Now natural languages and paradigms may of course be compared, when it is possible by means of analogy - considering for example the problems involved in the translatability of natural languages (see e.g. Kuhn (1976), pp. 190-191) - to shed light on certain properties of paradigms. The analogy, however, is a metaphorical equivalence which it is not permissible to interpret as meaning that a given natural language *is* a paradigm. Without going into the actual intent of Meynell's paper from which the above citation is taken - is a natural language one example of Kuhn's paradigms or is what is at stake a metaphorical comparison? - we must definitely state that a given natural language cannot exemplify Kuhn's 'paradigm' if only because natural languages are too "comprehensive" to be paradigms. (The "natural language as paradigm" viewpoints contain on a larger scale the same misplaced interpretation of the paradigm concept as that rejected in sections 1 and 2 above: paradigms are not so all-embracing as the Kuhn critics mentioned would suggest.)

The all too "universal" conception of paradigms is to be found in the literature in a variety of forms (see also above); for example Sadovsky accepts the view that the "systems approach ... is to be considered as a new scientific paradigm in contrast to the analytic, mechanistic, linear-causal thinking methods of classical science" ((1974), p. 33). But a paradigm is in no wise so general, a fact which is manifest if one considers what the counterpart of the "systems paradigm", the "classical science paradigm", would embrace: a wide range of different symbolic generalizations, ontologies and exemplars such that it would be possible to select from this diffuse constellation (a) a symbolic generalization and an exemplar which have absolutely nothing to do with one another (the exemplar might exemplify something quite unconnected with the generalization); and (b) two such components which are in conflict with one another. *If* we assume that the systems approach involves functioning theories with functioning exemplars, the same absurdity applies in principle to this "paradigm". In a word: a paradigm *cannot* on the one hand constitute too universal an entity; on the other, it *is* a holistic entity.

4. Stefan Amsterdamski likewise understands the paradigm concept in a "overall" sense, claiming that

> "... the principal simplification of Kuhn's book ... is the conception that the development of knowledge in a given discipline in a definite epoch can be described as a result of the reign of a *single* paradigmatic point of view ..." ((1973a), p. 55)

or that

> "... it is difficult to deny that Kuhn's concept of paradigm plays in [STR] the role of the proverbial bottomless well, into which one may cast various factors which, although they undoubtedly condition the evolution of knowledge, cannot be theoretically encompassed under category." ((1973b), p. 120)

In his analysis of the paradigm concept Amsterdamski does not, however, at the *theoretical* level succeed in pinning down those "various factors", and has at the level of *examples* in fact nothing illuminating to say (see Amsterdamski (1973a) *passim,* (1973b), e.g. pp. 87, 95, 116–129, 135–140, 162–168 and (1975) *passim*); in Amsterdamski's interpretation Kuhn's paradigm is conceived only in a very general sense to be on the one hand something which determines a very "wide" area and on the other something which by its diffuse nature is not to be embraced within one theoretical category. Both ideas have been refuted in the course of our work, for a paradigm is *no such* comprehensive operator, and it *is* a holistic, specific entity.

Setting out from the basis described above, Amsterdamski proceeds to "sharpen" (Amterdamski (1973a), p. 54) Kuhn's view (see Amsterdamski (1973a), (1973b) and (1975)), pointing out among other things that

> "*one scientist* is in his research work a slave of several paradigms of different levels of generality, which may

> superimpose upon one another, may conflict *etc.*" ((1973a), p. 57)

and that

> "*Since* the development of science or of a discipline and the scientist's cognitive activity are not determined by one single paradigm, which in time of revolution falls into crisis, *then* a revolution is not an abrupt discontinuation of the development." (*Ibid.*, p. 57; italics added)

Amterdamski's "sharpening" comprises the following main lines of thought. *In the first place,* the passage just cited implies to our mind this: *if* a discipline - let us say for some chance reason - were determined by one single paradigm, *then* a revolution would be "an abrupt discontinuation of the development". *Secondly,* Amsterdamski's "improved" version of the paradigm concept would attribute to a given branch of science (e.g. physics or biology) a number of paradigms of varying levels of generality, so that he feels he can speak for example of the current paradigm of physics ((1973a), p. 56) and of biology ((1975), p. 24); and if we take an example from the latter, then according to Amsterdamski this biology paradigm falls into more specific sub-paradigms in such a way that in a particular subsidiary area of this discipline "specialists ... accept some *additional paradigmatic* assumptions *besides* those accepted by all the biologists" (*ibid.*, p. 24; italics added). *Thirdly* the continuity of science is ensured, so Amsterdamski believes, in that a researcher is slave at once to a number of "paradigms" of varying levels of generality, so that a revolution in the sphere of *one* paradigm does not exclude rational agreement:

> "The possibility of rational agreement - at least in science - between adherents to the old and the new "points of view" in a discipline that suffers from a crisis is not completely lost. Some paradigms that are common to them survive, and there is still a more comprehensive "consensus" left than what can be threatened by the revolution. Even if it is true (and I think it is) that the old "point of view" and the new one established after the crisis are partially incommensurable, it does not follow that the transition from one to the other could have been effected in an irrational way, by "conversion"." ((1973a), p. 57)

But - if we are right - one cannot speak of a paradigm as a paradigm of physics or of biology or of any other entire discipline, from which more specific paradigms could be "derived" by *adding* new "assumptions" to it, and no such hierarchy of paradigms as Amsterdamski envisages can be constructed. (Paradigms are not in his sense "additive".) Further, if we take two successive paradigms P and Q, there is not between *them* "an abrupt discontinuation of the development", as we have shown in the course of our discussion. Thus, finally, the rational continuity of science does not reside in Amsterdamski's diffuse additive model.

Without going any further into Amsterdamski's attempt to "sharpen" Kuhn's focus we may say, in conclusion, that it would seem as a whole to suffer from an unsharpened conception of paradigms (manifested among other things in the misconstrued function Amsterdamski attributes to the progress of normal science; see e.g. Amsterdamski (1973b), pp. 126–129).

5. In an article on Kuhn in which he analyses scientific communities, normal science, anomalies and scientific change, Maurice Mandelbaum, as if to *challenge* Kuhn's model, has the following to say:

> "What must not be overlooked, however, is that every scientist belonging within a scientific community is also a member of other communities, and through them he will have come under other influences. One might suppose that these influences will not be likely to alter a *scientist's* basic orientation within a period of *normal science* unless *scientific anomalies* have arisen, but even this may not always be true. *For example*, one can reasonably argue that it was *not* primarily because of anomalies within the accepted systems of classification of plants and animals that *Lamarck* was led to formulate a sharp and explicit contrast between his evolutionary *theory* and the doctrine of special creationism. Rather, it has been held that this contrast was closely connected with his general scientific, philosophical, and religious views." (Mandelbaum (1977), pp. 446–447; italics added)

But Mandelbaum is here overlooking the point that in Kuhn's model a change in basic orientation in normal science (a revolution) concerns paradigms and (paradigmatic) theories. Lamarck's views were *not* paradigmatic, *nor* did they constitute a theory, if only because

Lamarck did not have a paradigm: he had no set of functioning exemplars *together* with which he could have propounded his ideas so as to form a holistic entity imbued with problem-solving capacity (paradigm/paradigmatic theory). Here Mandelbaum is making a very typical mistake in thus construing the Kuhnian conception of science: normal science, the normal-scientific community, anomaly, actualization and so on comprise an indivisible whole, one part of which, the paradigm, fulfils a constitutive function only if it is equipped with functioning exemplars: speculative constructs are not theories.

6. To our knowledge Harold I. Brown is the only assessor (from the immanent point of view) of Kuhn's theory to contribute to the literature a systematic analysis of the nature of paradigmatic points of departure and their role in the development of science. In his article "Paradigmatic propositions" he introduces the notion of such entities, which, he holds, bring clarification to Kuhn's paradigm (Brown (1975), p. 85), and which have the following attributes:

> "(1) They are "protected" propositions, i.e., they are not open to straightforward empirical disconfirmation, but they are not analytic propositions.
>
> (2) They are constitutive of both research and experience.
>
> (3) They are presuppositions of scientific knowledge supplied by the knower, but they are not eternal truths." (*Ibid.*, p. 85)

In proceeding now to illustrate his paradigmatic propositions Brown employs as his focal example the revolution in astronomy and the problem of Aristarchus as a forerunner of Copernicus' system. Brown sets out to pinpoint paradigmatic propositions in the history of science (astronomy) - propositions, that is, which on his conditions (1) - (3) should occupy a position diverging from that of "normal" empirical hypotheses in respect of testing - drawing attention to the suggestion of Aristarchus in the third century B.C., which was in conflict with the geostatic conceptions accepted in his day, in positing that the earth revolves about the sun during the period of one year (Brown (1975), p. 86). If now Aristarchus were right, there ought to be an observable shift in the apparent positions of the stars (*ibid.*, p. 86). But when no such stellar parallax was to be perceived, this circumstance was taken as a counter-example to Aristarchus' hypothesis and his contemporaries thus rejected it (*ibid.*, p. 86). Thereafter Aristarchus sought to explain this discrepancy between his hypothesis and the evidence of observation by proposing that the distance between the earth and the stars was greater than supposed and the parallax thus simply too small to be detected; his suggestion was not taken seriously, however, and his hypothesis was not accepted (*ibid.*, p. 86). Here Brown now draws attention to an interesting point:

> "[t]he astronomical equipment necessary for the observation of this parallax was *not developed until the 19th century*, well after the Copernican version of this *same* hypothesis [the moving earth thesis] had been generally accepted by the scientific community. Why *was* failure to observe the predicted parallax taken as a counter-instance in the third century B.C. and *not* taken as a counter-instance in, say, the 18th century A.D.? Certainly the logical relation between the two propositions in question had not changed; modus tollens was as valid in one century as in the other. And what is even more striking is that the 18th century astronomers did accept Aristarchus' own explanation of the failure to observe stellar parallax, i.e., that while this parallax exists, due to the great distance from the earth to the stars, it is much too small to be observed by the unaided eye." (*Ibid.*, p. 86; original italics removed, new added)

Brown goes on to stress that for example Copernicus and Galileo found the absence of observations on the parallax an adequate reason to undertake determination of the distance between earth and stars, whereas Tycho Brahe took the same circumstance as an argument against a moving earth. Finally, in explanation of the difference in attitude to one and the same proposition in different eras Brown points out that by the 18th century "the moving earth thesis" had by merit of the work of men like Copernicus, Galileo, Kepler and Newton become a proposition which "now served as one of the principles which astronomers used to interpret their observations rather than as a proposition subject to empirical confirmation or disconfirmation - it had become a *paradigmatic proposition*" (*ibid.*, p. 86; italics added).

Brown's idea calls for the following comments. His "paradigmatic propositions" do *not* convey the nature of paradigmatic starting-points in the Kuhnian sense, because Brown's entities seem *in principle* to be "ordinary" empirical hypotheses except that the scientific community in a certain period *refuse* for example to falsify them. Brown's interpretation of this paradigmatic propositions is thus entirely conventional (cf. § 11.3, section 1, above). That Brown indeed intended for them such a conventional role becomes manifest in his article:

> "[Paradigmatic propositions] are the propositions that scientists consistently *protect* against refutation even though *they are logically possible candidates for refutation* and even in those situations in which they seem to be the most likely candidate for refutation." (Brown (1975), p. 88; italics added).

If we might be inclined to call certain paradigmatic starting-points - those which in *some* sense could be called "propositional" - paradigmatic propositions, even these would scarcely prove identifiable with Brown's.

§ 15. THE LATER KUHN CONTROVERSY AND METAPHORICAL EQUIVALENCES

1. We have suggested in the foregoing that Kuhn employs metaphors as an aid to the philosophy of science in an attempt to elucidate certain basic points in his theory which have proved difficult to express in traditional and conventional terms. It is not always appreciated in the literature, however, that his theory contains metaphorical equivalences, as we like to call them. This we have already pointed out. Of these equivalences we have discussed four; the first was between the solving of scientific problems and puzzle-solving, the second between paradigmatic points of departure and logical truths, the third between scientific revolutions and political upheavals and the fourth between a scientific revolution P/Q and the gestalt-switch, or, as we called it, the D/R effect. We shall proceed now to consider a number of points connected with this mode of expression in the context of the later Kuhn controversy.

2. A *literal* understanding of normal-scientific research as puzzle-solving leads Stefan Amsterdamski to list the known properties of puzzle-solving processes (see e.g. § 5, above) and to draw from these the conclusion "that the task of normal science does not lie in the seeking of new facts or theories" ((1973b), p. 126), and that normal science is the same thing as applied research and practical skills (e.g. (1975), pp. 26–27). Precisely the same conception of the nature of normal science is reflected for example by Satosi Watanabe: "[D]uring the period of normal science *no essential progress* has been made and only *applications* of the *same* theory to *other examples* have been made" (1975), p. 116; italics added). Amsterdamski and Watanabe – and in this they are two among many – thus on the one hand identify normal science and *techne* (see esp. Amsterdamski (1975), p. 27), and on the other end up identifying a normal-scientific theory with a calculator; *in our words* they would identify a paradigmatic theory G(P) and the FROZEN (G(P)). All in all, such interpretations of normal science would equate outright normal-scientific change and propositional normal-scientific change, ignoring entirely the level of conceptual normal-scientific change. This latter oversight implies that the writers in question have either omitted to analyse the actualization process or have completely misunderstood its nature, in which case they hold an unfortunate conception of normal-scientific progress and of scientific progress in general.

3. Kuhn illustrates the various aspects of scientific change by comparing it on the one hand to political revolution and on the other to the D/R effect. Naturally enough, the former metaphor is generally not taken literally (see e.g. Cunningham (1978); see also, however, Kmita (1975) e.g. p. 67 for its "slightly more" literal analysis). In the case of the latter metaphor, on the other hand, the situation is more complicated; for example Mandelbaum – who brings out the *analogy* between political and scientific revolution – holds that Kuhn's theory "does in fact lean heavily on [psychological studies]" ((1977), p. 449). However, Kuhn's theory does not, to our mind, lean on psychological studies, it simply makes use of a metaphor to shed light on certain aspects of scientific epochs. When one takes into

account that what he is speaking of is the change P/Q, one can hardly endorse Mandelbaum's interpretation whereby Kuhn is taken to be claiming that "the whole perceptual world thereby undergoes transformation" (*ibid.*, p. 450; see in this connection also Mandelbaum (1979), p. 414).

4. A particularly "radical" interpretation of Kuhn's theory is that of Aaron Ben Zeew.

Ben Zeew holds *in the first place* with regard to normal science that according to a given normal-scientific paradigm "cognitive disputes within that science are resolved" ((1979), pp. 487-488). It is not clear from the context what exactly he means by his "cognitive disputes". If he means that cognitive disputes over *paradigmatic points of departure* are resolved within the science in question, his claim is misleading in many ways. To begin with, paradigmatic points of departure are not from the outset transparent, so that we cannot say that cognitive considerations connected with them are "resolved". And again, attention must focus on the process of actualization, where these points of departure become actual. But now, cognitive problems associated with this process cannot be settled by disputation, so that there is no *sense* in saying that within the paradigm "cognitive *disputes* are resolved". And lastly, if we consider the nature of the process whereby actualization proceeds, it is not *true* to say that within the paradigm "cognitive disputes are *resolved*", for the actualization process, which may take hundreds of years, is not "resolved" once for all within the paradigm.

Secondly, in connection with paradigmatic changes Ben Zeew holds that the dispute between two paradigms cannot be settled on the basis of the greater truth value of one or the other because in the shift P/Q the criterion of truth changes; and besides, in Kuhn's view "the change in the paradigm is not only a change in scientific thinking, but also a change in the *world* to which the scientists respond" (*ibid.*, p. 488; italics added). Because different paradigms look for different contents, there is no rivalry "and therefore no cognitive priority of any one system" (*ibid.*, p. 490). The absence of an objective characteristic which would settle the superiority of theories renders it impossible to set them in order of preference, and Ben Zeew in fact observes regarding Kuhn's view: "Cassirer arrives at the same conclusion [as Kuhn] in his *Philosophy of Symbolic Forms;* in which it seems that science has no cognitive superiority to myth and religion" (*ibid.*, pp. 488-489).

If we have been right in what has been said in this work, paradigmatic science does not have the features Ben Zeew describes; however, it remains to consider the kind of focal conceptions in which his construals have become entangled. His basic point of departure, which he mentions *expressis verbis* only in a note, is this:

> "This term ['paradigm'] has several meanings in Kuhn's book, it also means *a priori* premise, in which sense I shall take it in the following discussion." (*Ibid.*, p. 493, note 24)

Ben Zeew has thus taken certain of Kuhn's metaphorical analyses literally and has identified paradigmatic points of departure with *a priori* principles. Apart from this manifest error Ben Zeew claims without grounds or examples that the term 'paradigm' is ambiguous. Nor is this all: he nowhere defines or characterizes the paradigm concept, nor gives a single example of a paradigm.

Ben Zeew also reads word for word Kuhn's analogy of the gestalt-switch: "Kuhn bases his position on the psychological theory of Gestalt, which shows that whenever our pattern of perception changes, the content of perception also changes" (*ibid.*, p. 488). If one sets out from the view that paradigms are *a priori* and that Kuhn's theory is based on the Gestalt theory (different paradigms, which are always *a priori,* always have different contents), one of course will end up with the kind of nihilistic conception of Kuhn's model which Been Zeew is offering us.

5. Let us imagine that a change took place in the human species such that a person were capable of discerning in a given image on paper both a duck and a rabbit, in other words that the duck/rabbit effect no longer obtained. Would the relation between successive paradigms be thereby altered, would their incommensurability be overcome, would conversion be replaced by

proof, would the nature of scientific progress be changed? If we consider how the complicated process of actualization, with its various steps and stages, its apparatus and its calculations, goes about the business of rendering a given theory actual, it seems that the disappearance of the duck/rabbit effect would not eliminate the incommensurability of processes, so that the shift from one paradigmatic theory to the next would remain a matter of "conversion". On the development of *science* the disappearance of the D/R effect (the acquisition of "double vision") would then have no effect - on the other hand the imagined change in the species would affect the development of the (metaphorical) *description* of scientific progress: this would no longer be possible by means of the D/R analogy.

§ 16. KUHN'S IRRATIONALISM, AUTHORITARIANISM AND ALL THAT: REFLECTIONS ON THE PATH OF SCIENCE

1. The paradigm view is a metatheory embracing the nature, structure and development of scientific knowledge. Within its framework certain values are attributed a normative function in directing the course science takes: in particular, these criteria play a crucial role in choices between different paradigms.

The functional mechanism of these values is, as we have already seen, set out in STR, so that there is no further call to discuss the view that these standards are "later Kuhn" (see, however, McCarthy (1975), p. 356 on such a conception). There is, on the other hand, reason here to take up two modes of interpreting the paradigm concept in which the paradigmatic development of science in the Kuhnian sense is held to be governed by categories which value theory has traditionally investigated.

The first of these readings to be found in the literature envisages choice of theory as an *aesthetic* choice in which objective criteria are entirely lost and the course of science is dictated by subjective and incommensurable viewpoints which thus give access to all manner of psychological and sociological factors in influencing its development. Such an extreme interpretation of the paradigm concept is articulated by Tibor Machan (see Machan (1974)). In his view the process of selecting between alternative paradigms at points of breakthrough in the development of science can in the paradigm conception have none but aesthetic, subjective grounds (*ibid.*, p. 357), so that at the very least the *turning-points* in scientific development lie beyond the sphere of rational assessment:

> "In Kuhnian terms, the choice of a new paradigm must be subjective (aesthetic, non-objective) ..." (*Ibid.*, p. 370)
>
> "Kuhn takes the chooser's aesthetic preference as his *final* point of reference when dealing with *selections* from among competing, incommensurable paradigms." (*Ibid.*, p. 366; italics added)

In justification of his claim that the paradigm concept rests in the last analysis upon aesthetic preferences, Machan refers to a passage in STR in which the aesthetic dimension of theory selection is in fact considered in association above all with the *simplicity* requirement (see Kuhn (STR), pp. 155-158, Machan (1974), p. 357) - and simplicity of theory, while no "simple" definition is likely to be found for it, can scarcely be "so" aesthetic as Machan's reading would seem to take it. This, however, is by no means our main point in taking Machan thus to task; we shall show in what follows that he in fact completely misconstrues the mechanism by which science in crisis takes new direction.

The influence of aesthetic and subjective considerations on the solutions reached by certain *individuals* is explicable, so Kuhn believes, by the fact that the struggle between two rival paradigms takes place in a situation where the newcomer is but a promise, and in this situation "[s]*omething* must make *at least a few* scientists feel that the new proposal is on the right track, and *sometimes* it is only personal and inarticulate aesthetic considerations that can do that" ((STR), p. 158; italics added). But that an aesthetic preference should constitute the "final point" (see Machan (1974), p. 366) in determining the *choice* of theory, is not what Kuhn means, as Kuhn himself *expressis verbis* emphasizes in a passage which Machan - indeed unfortunately - has not troubled to read:

> "This is not to suggest that new paradigms *triumph* ultimately [in the "final point"] through some mystical aesthetic." ((STR), p. 158; italics added)

Some individuals, it is true, may take a risk on "aesthetic" grounds, such being in the first place rarely the case (*ibid.*, p. 158) and, secondly, leading frequently to this: "those who *do* turn out to have been misled" (*ibid.*, p. 158; italics added). The use here of the word 'misled' can only mean that the gamble in question has not given birth to a new normal-scientific tradition, has not brought the emergence of a new paradigm. But on the other hand - if we contemplate the progress of science - some risk is inevitable, for "if a paradigm is ever to triumph it must gain some first supporters, men who *will* develop it to the point where hardheaded arguments can be produced and multiplied" (*ibid.*, p. 158; italics added). But even these arguments "are not individually decisive" (*ibid.*, p. 158), for although "scientists are reasonable men" (*ibid.*, p. 158) and although therefore "one or another argument will ultimately persuade many of them" (*ibid.*, p. 158), nevertheless "there is no *single* argument that can or *should* persuade them *all*" (*ibid.*, p. 158; italics added). If we now focus our attention on the italicized points in the above citation we may summarize as follows. That choice eventually falls on a given paradigm expresses normal-scientific *consensus*. This, however, is not reached on the basis of any *one single* standard, that, for example, of simplicity, nor *should* any such restrictive selection procedure be sought, because the application of one criterion to the exclusion of others would halt the progress of science as far as long-term adaptation was concerned. (One need but think of the consequences of upholding to the detriment of other values the one which requires the theory to be in harmony with previous knowledge.) The emergence of a new paradigm and a new normal-scientific consensus means a balance achieved over many phases in the mutual weighting of the numerous standards forming the values, this naturally constituting an assessment of theories based upon standards and the (objective) achievements of paradigms. In confining himself to a consideration of a single "standard", "aestheticity", and giving this an entirely subjective function, thus to conclude that "Kuhn's problem is essentially that *no standard* can be invoked when we make a choice between competing paradigms" ((1974), p. 357; italics added), Machan succeeds in turning the whole matter upside-down. (It is also of especial note that in defence of the above claim Machan's account contains a note, i.e. note 12, which refers the reader to page 102 of the 1962 edition of (STR), page 103 in the 1970 version. The page in question in fact affords no grounds for his assertions.) In this light, and in the light of the foregoing, Machan's analysis of the paradigm concept, which he sums up thus: "[I]n the last analysis it is so called subjective considerations that will determine" ((1974), p. 355), cannot be considered appropriate, for it ignores all but "aesthetic" values and at the same time completely overlooks the complicated process of balancing out numerous and diverse values which, at certain points in the development of science, directs scientific opinion from discrepancy to consensus.

According to the other extreme reading of the paradigm concept the basis of theory selection and of progress in science is *moral* or *ethical;* such is the view subscribed to by Robert Hollinger (see Hollinger (1975)). One might think, he says, that if the scientific claims put forward within the framework of any theory whatsoever cannot be conceptually distinguished from value commitments, then assessment of the *ethical* aspects of a given theory cannot be held apart from assessment of the scientific merits of that theory (*ibid.*, p. 304). And according to Hollinger this is (also) Kuhn's position, associated "with a doctrine of internal relations, according to which the distinction between norms and scientific descriptions or hypotheses virtually disappears" (*ibid.*, p. 304). And since, in addition, a *theory* is in Kuhn's view invariably a matrix of norms and conceptually loaded descriptions,

> "it becomes logically impossible to distinguish between the *scientific* merits of a theory, and its normative (including, on some views, *ethical*) merits: any claims about the scientific truth of a theory are inseparable from a *moral* assessment of it." (*Ibid.*, p. 304; italics added)

Be it noted that the reservation placed on the scope of the concept 'normative' in the above citation - "including, on *some* views, ethical" (italics added) does not apply to Kuhn (see *ibid.*, p. 304, paragraph 4, first sentence), so that we may in the following consider Hollinger's extreme claims to obtain without qualification in the case of Kuhn. These claims may be summarized thus: *for Kuhn* "any assessment of the *scientific* merits of ... a theory" - if Hollinger is right - "will in part be a *moral* argument" (*ibid.*, p. 304; italics added). Hollinger's mode of presentation and the title of his paper ("Can a Scientific Theory Be Legitimately Criticized, Rejected, Condemned, or Suppressed on Ethical or Political Grounds?") gives grounds to consider the matter further along the lines of an inquiry into whether the paradigmatic development of science is governed by ethical or political factors.

In Hollinger's view an empirical theory is according to the paradigm conception always "a matrix of norms and conceptually laden descriptions" (*ibid.*, p. 304). Without going into this diffuse notion of Hollinger's regarding the structure of paradigmatic theories (in which many important aspects are overlooked), we would draw attention to the crucial point that Hollinger first imbeds norms in matrices and then interprets *these* norms in such a way that he can thereby introduce the ethical and moral aspect - *as opposed to* the purely scientific - into the paradigm concept (as he understands Kuhn to have intended it). But this bit of smuggling is - if anything is - contrary to the principles of the paradigm concept. *In the first place*, no single value in the set of values V in any given paradigm is associated with moral or ethical considerations, and the moral factors Hollinger has in mind have no access, through V or via any other route, to the processes of appraisal or choice of theories. *Secondly*, Kuhn repeatedly emphasizes that scientific communities, those worth the name, are typically *not* such as are set up tailor-made for society; on the contrary, they may tend to isolate themselves from problems which cannot be formulated within the framework of a paradigm *even though* these problems may from the human standpoint (on ethical or for example political grounds) be of the utmost urgency (see e.g. (STR), pp. 35-42). *Thirdly*, the *set of norms* (!) Kuhn himself acknowledges and distinguishes in the activities of a scientific community is designed to exclude ethical as much as political argumentation from the appraisal of the scientific merits of theories whereby the progress of science takes its direction. Such characteristics of the scientific community are, for example, the following: the objective of a researcher is the solution of clearly defined problems; the results obtained by a member of the scientific community should enjoy wide acceptance - not however in the sense that a head-count of supporters would carry any weight but in the sense that the research community in question endorses his conclusions; and "[o]ne of the strongest ... rules of scientific life is the prohibition of appeals to heads of state or to the populace at large in matters scientific" (*ibid.*, p. 168).

It is clear from the above that the idea of ethical (or political) arguments intruding into the scientific process as the paradigm conception envisages it - intruding in such a way, that is, that the evaluation (and choice) of theories would proceed on moral grounds (Hollinger (1975), p. 304) - or on political grounds - cannot be countenanced.

2. The claim that Kuhn's model involves an element of the non-rational, irrationalism, authoritarianism, sociologism, psychologism and so forth - even to the inclusion of political factors - is a common feature in the later Kuhn controversy (see also earlier parts in the present work).

According to I.C. Jarvie the paradigm model contains two main defects in respect of the development of science. The first is - as Jarvie sees it - that for Kuhn the rational progress of science is confined to normal science, whereas a scientific revolution is "a time of change, uncertainty, irrationality, community breakdown, loss of direction and standards, possibly not science at all" (Jarvie (1979), p. 487). The second defect is that "because there was *no line of succession* from one overthrown paradigm that would connect it up with the reign of the next, paradigms had to be seen as incommensurable" (*ibid.*, p. 487; italics added). These two defects, for their part, - if Jarvie is right - would bring with them the following: (1) The crucial turning-points in the development of science would boil down to sociological, psychological, political or economic questions - the basic problem is "how the community *behaves*" (*ibid.*, p. 487; italics added), so that, in a word, "science is grounded in society" (*ibid.*, p. 487). (2) A consequence of incommensurability is lack of progress in science and the "Balkanization" of the scientific community "into rival schools unable to communicate" (*ibid.*, p. 487).

If we assumed that Jarvie is right - and Jarvie of course assumes he is - we must accept as perfectly logical his conclusion that Kuhn's position in the debate of scientific progress and the rationality of science is to be described as an "irrational sociology of authoritarianism and indoctrination" (*ibid.*, p. 496).

In a rather similar vein Watkins (1975) reverts to criticism of Kuhn and construes him as a modern representative of one branch of what he calls *presuppositionalism* (on Watkins' earlier critique of Kuhn see §§ 7.1 and 12, above). The doctrine of presuppositionalism originates with Kant, according to whom certain principles which are *unaltered* throughout the development of science, are prerequisite to scientific research (Watkins (1975), pp. 115-116). But when for many scholars presuppositionalism in Kant's strict sense proved in the light of the history of science to be untenable, certain philosophers in the present century reacted to this (for example to the fact that classical physics had to be set aside) "by modifying Kantian presuppositionalism into what may be called *relativistic presuppositionalism*" (*ibid.*, p. 117), in which view

> "the presuppositions of science *change* [italics added] in the course of time. But if we take a *particular* [italics added] scientific epoch we find that certain common presuppositions are being made by the scientific investigators of that time; moreover, relative to that time these presuppositions are *absolute* in that they ultimately and silently control the scientists' thinking and cannot themselves be brought under *any* [italics added] critical control." (*Ibid.*, p. 117)

Thereafter Watkins refers to Collingwood as one formulator of relativistic presuppositionalism and - as did Toulmin - equates Kuhn with him:

> "You may say that Collingwood is not nowadays taken very seriously within the philosophy of science. But here are, in Collingwood, anticipations of the more influential ideas of ... Kuhn." (Watkins (1975), p. 117)

And now, *firstly* in respect of normal science, although "[f]or Kuhn, that which controls scientific thinking at a given time in a given domain is by no means *wholly* tacit and inarticulable" (*ibid.*, p. 117; italics added), nevertheless Kuhn's theory, along with its alleged relativistic presuppositionalism, is branded by Watkins as philosophical *propaganda,* propaganda which defends and sponsors a *non-critical* acceptance of the presuppositions of normal science (*ibid.*, p. 118). *Secondly,* as regards scientific breakthroughs

> "Kuhn's account of the way in which one paradigm gets replaced by another resembles Collingwood's account of the way in which one set of absolute presuppositions gets displaced by another at least in this respect: the process is *non-rational* (Kuhn likens it to a gestalt-switch)." (*Ibid.*, p. 118; italics added)

If Watkins were right, the development of (mature) science would in Kuhn's model fall in each separate field into a succession of periods alternating between non-criticism (normal science) and non-rationality (scientific crisis). But Watkins is not right. If we first examine normal science, it is impossible to see how the application of paradigmatic presuppositions and their establishment as foundation to a world-picture in the discipline in question - *when, that is, the actualization of the paradigm proceeds successfully* - could be effected uncritically in any customary sense of this word. Matters are just the opposite of what Watkins and for example Popper (together with his followers in general) imagine: in the situation in which the actualization of a paradigm proceeds and normal science is making headway, it may from the standpoint of scientific progress be incredibly irresponsible, uncritical and irrational to revert to indulgence in "critical" and highly "intellectual" disputation and debate as to the "real nature" of the object of research in a given discipline.

We have at an earlier point in our discussion referred to a weighty historical example of such a state of affairs: while throughout the XVIIth century certain centres of science in Europe were practising high-standard normal-scientific research in the field of astronomy, elsewhere, for example in Sweden-Finland, progress in this field was systematically suppressed by means of "critical discussion" of the various systems and the presuppositions they entailed (see Lehti (1979)). In actual fact, apart from academic lectures and textbooks, a prominent role was taken in Sweden-Finland by a special *literature of disputation* (*ibid.*, p. 9) as the main channel for this "criticism". The *peculiarities* of this Swedish-Finnish literature in the field (*ibid.*, p. 6) are not attributable to the remoteness of these regions and hence to ignorance of the trends of thought prevailing in Europe's main centres of scholarship (*ibid.*, p. 6 and *passim*). For example Laurentius Paulinus Gothus was lecturing in 1599 in Uppsala on the systems of Ptolemy, Copernicus and Tycho Brahe (*ibid.*, pp. 9-10), and for example the focus in one disputation appearing in 1649 was on a comparison (sic!) of these systems (*ibid.*, p. 12). As a matter of fact the compilation of a disputation paper expressly required a knowledge of the various views prevailing in astronomy at the time, together with a "critical approach". However, what was *not* required in these disputations and in the astronomy practised in Sweden-Finland was exemplars E, work with these, and the actualization of paradigms (see *ibid.*, *passim*), to express the matter in the terms of the present work. In its eminent "rational criticality" the academic community succeeded almost completely in preventing the progress of astronomy in Sweden-Finland throughout the XVIIth century. If we transfer the matter to a general level we might describe the situation in our own conceptual terms thus. With regard to normal science the state of affairs is the *opposite* of what Watkins and the Popperians claim and of what their criticality concept would presuppose. In normal science there is, it is true, a stubborn clinging to common capital, which makes it *appear* dogmatic. But the normal scientist must always have functioning exemplars E, and he must achieve *objective* improvements in his own discipline. Without true criticality this is not possible. Disputations and intellectual discussions, for their part, might *seem* critical, but by their very nature they not only are something *other* than actualization, they are also especially apt - in becoming the yardstick for the critical practice of science - to *hamper* actualization and the progress of research into nature. In fact, then, disputations, bold speculations and critical rationality fall rather into the sphere of uncriticality and dogmatism: it is as uncritical and dogmatic to cling to one's presuppositions as to alter them in "critical" discussion if this entails neglect of the task of actualization. And the very fact of undertaking this task (actualization) indeed makes the criticality in the spirit of Watkins and Popper superfluous: this kind of "criticality" either does not enter into the process, or, if it *does*, it will tend to suppress any responsible and critical attitude to the progress of actualization.

Before we pass on to the alleged non-rationality or irrationality of scientific breakthroughs it may noted that the literature abounds in examples of the way indifference to the actualizing function of normal science results in a false conceptualization of the development of science from the standpoints of "criticality", "debate" and "disputation" (see by way of example Hattiangadi (1978), Quay (1974b) and Swarts (1976)). Quay places research along the lines of Kuhn's model on a par with theology ((1974b), pp. 355–359). But in overlooking the nature of the actualization process he also overlooks the Achilles heel in his own comparison, namely the fact that in a certain sense theology, however dogmatic its practice may be, is - surprisingly enough - precisely "critical" disputation in which views are defended and refuted in a manner which - and this is what Quay fails to notice - is entirely at odds with the requirements imposed upon science in Kuhn's model (see earlier parts of the present work and further also Kuhn (STR), pp. 167–169). Hattiangadi for his part sees the whole course of the history of science as a *debate*, so that the difference between the development of paradigmatic science and pre-paradigmatic practice manifestly disappears; on the basis of this theory of scientific development there would for example appear to be no distinction between natural philosophy and physics (Hattiangadi (1978), e.g. p. 14). In ignoring the actualization function and upholding his disputation model of progress Hattiangadi can go so far as to claim the following:

> "Kuhn proposed an interpretation of scientific research in which the scientist is normally immersed within his scientific traditions, to the extent that *he does not find it necessary to articulate* ... the basic propositions of his scientific traditions." (*Ibid.*, pp. 8–9; italics added)

Let us now revert to Watkins' claim that for Kuhn a scientific *breakthrough* is a non-rational event. This claim is extremely frequent (in various disguises) in the later Kuhn controversy. We may now consider it as it appears in the writings of a number of scholars.

According to Larry Laudan Kuhn not only claims that in the actual history of science examples are to be found of irrational decision-making in the selection of theories, but that, in addition, "choices between competing scientific theories, in the nature of the case, *must* be irrational" (Laudan (1977), p. 3; italics added). Laudan sees this Kuhnian irrationalism to go even so far that "scientific decision making [between competing theories] is basically a *political* and *propagandistic* affair" (*ibid.*, p. 4; italics added). The reason, he holds, is this. Kuhn sets out from the premise that rationality is exhaustively defined by a certain model of it - "[he] takes Popper's model of falsifiability as the archetype" (*ibid.*, p. 4). And when Kuhn then observes that "the Popperian model of rationality will do scant justice to actual science" he concludes that "science must have large irrational elements, without stopping to consider whether some richer and more subtle model of rationality might do the job" (*ibid.*, p. 4).

Here we need but refer the reader to earlier parts of the present work (for example chapters IV and V) for a reminder of how far Kuhn is from any such endorsement of Popper's model as the paragon of rationality. Laudan's "corollary claim" that Kuhn contents himself with irrationalism in a situation in which the traditional model has proved ineffectual, is to our mind to be seen in the main as a propagandistic and pejorative gibe whose emptiness is laid bare when we recall that the creator of a new model - one designed to overcome the impasse facing the old one - is, if anyone, Kuhn himself (see earlier parts of the present work, incl. Chapter I and especially its introduction).

Though we shall not in the present framework go into the picture of Kuhn's theory Laudan gives us, we may nevertheless point out a number of erroneous or misleading conceptions he appears to hold. In the first place he says the paradigm concept is ambivalent; in this he appeals to Masterman and particularly to Shapere, whose criticism of Kuhn he thinks "excellent" (*ibid.*, p. 231, n. 1; see also p. 73). This matter, also as it involves Shapere, has been dealt with in detail at an earlier point in our work, and the argumentation need not be repeated here. Secondly Laudan would have it that the ambivalence of Kuhn's analyses, originating in the ambivalence of his paradigm concept, "has been multiplied as a result of Kuhn's later retractions of many of the basic ideas of the first edition of [STR]" (*ibid.*, p. 231, n. 1). This claim likewise - one commonly put forward in the Kuhn controversy - has been dealt with above and need not be reiterated here, particularly since Laudan offers no grounds for his claim and in fact actually confesses himself "[u]nable to follow the logic of [Kuhn's] later changes", so that he is "forced [sic!] to characterize Kuhn's views in their [STR] form" (*ibid.*, p. 231, n. 1). Thirdly and finally, we would draw attention to the following notable claim Laudan makes regarding the distinction Kuhn draws between mature (normal) and immature (preparadigmatic) science:

> "It is worth pondering what motivates the search for a distinction between immature and mature science. My guess is that the quest harkens back to the old inductivist-positivist conviction that "proper" science only began [in] the seventeenth century. Although eschewing inductivism [Kuhn proposes] a demarcation criterion between mature and immature science which resurrects the inductivists' search for a definite point in time at which science became genuinely "scientific"." (*Ibid.*, p. 238, n. 30)

Laudan is indeed mistaken: there is, Kuhn explicitly states, no definite point in time at which *science* can be said to have commenced, even if with regard to various *theories* one may speak of transition from an immature phase to a mature one. Even here it must be borne in mind that the immature (preparadigmatic) phase is the springboard for mature (normal) science. It is advisable to stress, too, that Kuhn does *not* consider science to begin with Galileo and the great revolution.

H. I. Brown, too, expresses the conception that choice and establishment of paradigm is at bottom a sociological and political event (Brown (1972), pp. 230-231). This, Brown holds, derives for its part from the Kuhnian irrationalism which - surprisingly enough and in contrast to Kuhn's critics in general - Brown sees to be in some measure justified, so that Kuhn's error is in fact only in *exaggerating* it:

> "This [my argumentation] is not intended as a rejection of Kuhn's analysis of non-rational factors in the process of scientific change, for I believe that Kuhn has done an important service in directing attention to the role that such factors *do* play in the actual process of accepting of scientific theories. [But real science] is not nearly [sic!] as illogical nor as irrational as Kuhn makes it out to be." (*Ibid.*, p. 238; italics added).

Further, Brown sees this "extreme irrationality" to stem from "the logical paradigm that Kuhn accepts" (*ibid.*, p. 238) and this paradigm (sic!) is "the algorithmic paradigm of logic" (*ibid.*, pp. 238-239).

The manner in which Brown derives Kuhnian irrationality from his premise of the "logical paradigm" he attributes to Kuhn, may be schematized as follows: (i) there is no effective decision procedure for choice between paradigms; (ii) logic is confined to algorithms; thus (iii) choice between paradigms is non-logical; further, (iv) ""logical" equals "rational""; therefore, finally (v) the process of choosing between paradigms is irrational (*ibid.*, p. 238).

Brown's argumentation calls for the following comments. In the first place logic, as is well known, is not confined to algorithms; on the contrary, for the greater part logic and mathematics proper fall beyond the scope of algorithmic computability - for which reason Brown's comment

> "I think that [my] discussion is sufficient to show that algorithmic computability does not adequately cover the range of processes that have a legitimite claim to the title "logical" and is thus not an adequate explication of this notion." (*Ibid.*, p. 239)

is extremely peculiar and quite unnecessary. Secondly, Kuhn has no such special "paradigm" of logic as Brown envisages, in which the area of logic and that of effective decision procedures are identified. Thirdly, in referring to the nature of logic and mathematics as the opposite of empirical science Kuhn - so we would read him - intends the following (logic and mathematics in particular also containing an area lying beyond that of "algorithmic computability"): Empirical science diverges crucially from the formal sciences (logic and mathematics), and this is reflected in paradigms. That is, it is not possible in paradigm choice to appeal to arguments which are compelling in the same manner as those of mathematical proofs. Furthermore, competing paradigmatic theories cannot, according to Kuhn, be compared on the basis of a neutral comparative language SUPER fixed to the situation at hand, since for Kuhn no such SUPER languages exist (see earlier parts of the present work). To avoid misunderstandings it must at once be stressed that the argumentation whereby he denies the existence of such SUPER neutrals is not derived from the assumption that a neutral RV-type observation language exists; on the contrary - thus Kuhn - philosophers today have abandoned the hope of finding an ideal neutral observation language of this type - but in spite of this "many of [these philosophers] continue to assume that theories can be compared by recourse to a basic vocabulary consisting entirely of words which are attached to nature in ways that are unproblematic and, to the *extent necessary*, independent of theory" ((1970c), p. 266; italics added). We need not revert to the argumentation connected with this point, since the matter has already been dealt with (see chaps II, IV and V); attention must on the other hand be drawn to the fact that what was said above contributes to distinguishing empirical theories from logic and mathematics. For example in the mathematical theory of generative languages and grammars comparison of different generative grammars approaching the same sphere takes place *per definitionem* by means of a terminal vocabulary - let us call it V_o - "neutral" with regard to these grammars, comparison being made in an appropriate subset L of $W(V_o)$ which is precisely analogical to the SUPERs of the empirical sciences; and precisely this basic analogy and basic doctrine of the traditional view of science (be it based on the pure neutral observation language of the positivists or for example on the SUPERs of Popper and Lakatos) is what Kuhn denies: Here is one meaning of that claim of Kuhn's that in choosing between paradigms in the empirical sciences (e.g. in exact physics) it is not in the last instance possible to resort to arguments of the same type as in logic and mathematics (which does not mean of course that logic and mathematics are not used in the empirical sciences wherever they are of avail). On the basis of the above (and this is also connected with the weighting of the values in the set V) there is not, for Kuhn, any appraisal function for competing paradigmatic theories by which these could be (mechanically) placed in order of preference (by which, for example, it might be sought to assign to theories a degree of confirmation or a "distance" from the truth - for Kuhn no such functions thus exist).

In general those critics who see in Kuhn's model implications of irrationalism are convinced that it is precisely this which reflects the presence of untenable points in his theory: science of all things cannot involve irrational elements. Here Brown - as we have just seen - diverges from the common run of Kuhn criticism (according to Brown Kuhn *has* shown science to contain non-rational factors). Kuhn's "proof of irrationality" has also impressed for example W. J. Gavin, for whom "Kuhn has shown that there exists an irrational dimension in science as well as in other disciplines" (Gavin (1980), p. 28). Analysing Kuhn's model on the basis of *The Structure of Scientific Revolutions* Gavin, too, ends up in line with the mainstream in the Kuhn controversy, asserting that investigations pursued within the framework of this model must lose in objectivity because normal science is ideological and scientific revolution, again, involves among other things an irrational dimension (*ibid.*, p. 16). Gavin does not, however, offer grounds for this claim of an ideological factor in normal science, so that it can hardly be taken up in detail - suffice it to note that at least from the standpoint of the present work it is difficult to see how normal science could be ideological. If Gavin means by 'ideology' for example an approach to reality which generates distorted knowledge and identifies normal science and ideology in this sense, it must naturally be asked how *true* knowledge, in contrast to distorted, could be attained *otherwise than* in the process of actualization, whose nature - if we have been correct so far - cannot be ideological. If, on the other hand, Gavin could offer us some other means of obtaining a picture of reality *without* the "mediation" of actualization, that means would surely have to be considered ideological.

In justification of his claim that scientific change involves an irrational dimension Gavin in the last analysis asserts that "the transition from one paradigm to another is not *deducible* in any sense" (*ibid.*, p. 17; italics added). But why should change of paradigm have to be "deducible" in any sense? Or, if we set out from the "fact" (*ibid.*, p. 16) that transition from one paradigm to another is not deducible, why should it be deduced from this that science involves an irrational element? It would seem that Gavin has here adopted the traditional view of science in an extremely traditional sense of its rationality precepts (cf. chapter I, above) and is measuring the rationality of empirical science with the yardstick of or the analogies afforded by mathematics. When Gavin then realizes that the empirical sciences do not fall in line with the traditional model, he concludes that Kuhn has shown them to contain an "irrational dimension". The conclusion is rendered unnecessary and erroneous, however, when we recall that Kuhn's theory imbues the rationale of choice of theories itself in the empirical sciences with new content (as we have sought throughout to make clear). Without dwelling further on the sponsors of the "irrational dimension" we may briefly note that J. W. Meiland, for example, likewise perceives this factor in science, but "if one starts with the notion of anomaly, then one sees that irrationality in science is confined [sic!] to the adoption of paradigms" (Meiland (1974), p. 187). Satosi Watanabe, for his part, remarks in connection with his severe criticism of Kuhn that he *himself* has defended the view that "in matters of theory-choice, arational elements *have to* intervene" (Watanabe (1975), p. 116; italics added).

As regards the accusations of irrationality levelled at Kuhn - and for the most part they rest in an extreme interpretation, and one which diverges at least from the conception adopted in the present work, of the incommensurability thesis, together with application of some model of rationality characteristic of the traditional view of science - we shall in what follows content ourselves with a brief examination of a number of conceptions propounded by Frank Cunningham and J. W. Hattiangadi. (As other representatives of the irrationality claim not dealt with in the present work, see for example Fine (1975), p. 18, Flonta (1978), p.

402, Kmita (1975), pp. 68, 70, 73, 75, Meynell (1975), p. 88, (1978), p. 249).

Cunningham proceeds to a criticism of Kuhn and the non-rationality of paradigm change from the following basis (see Cunningham (1973) and (1978) *passim*). He holds that Kuhn's theory of paradigm change, which makes of the debate between competing paradigms a circular process, is founded on the same argumentation which sceptics down the pages of history ever since Sextus Empiricus have exploited:

> "The feature of rationally motivated changes in thought focussed on by sceptics is that these changes result themselves in part from prior thought. This thought, explicitly or otherwise, employs certain principles of what constitutes good grounds for rational acceptance or rejection of something. The sceptic then asks how at any one time these principles are themselves justified and argues that in order to avoid an infinite regress of justificatory principles, circular argumentation must enter someplace." (Cunningham (1978), p. 74)

But here, to our mind, Cunningham mistakenly sees Kuhn as embracing the traditional picture of science: a given theory is for Cunningham's Kuhn a set of statements whose members one should justify in accordance with the criteria of rationality adopted by Cunningham. But the facts of the matter are otherwise: the concept of theory, the "justification" of a theory and rationality itself diverge for Kuhn from what Cunningham proposes, as we have indeed brought out in the course of our discussion (see chapters IV and V). Particularly we may note that Cunningham overlooks Kuhn's insistence that scientific breakthrough entails a great deal more besides argumentation; and that a certain "circularity" of normal science, which Kuhn *does* acknowledge, is not of the same nature as the circularity of argument or proof meant by Cunningham: the actualization process is simply *not to be described* in the terms of Cunningham's "sceptic model".

Hattiangadi seeks in his analysis to show how the rationality of science can be "rescued" from the adverse comment Kuhn's theory has inspired ((1978), p. 4). This end he thinks he can attain by taking the history of science to be a series of intellectual debates (a view we have already had occasion to criticize in the foregoing). Here Hattiangadi must of course assume that no incommensurability obtains between the theories involved in the debate. In order to demonstrate the untenability of the incommensurability thesis he employs the following strategy:

> "As a clear refutation of the view that such theories as Einstein's and Newton's are incommensurable, let me take two theories which by any standard must be candidates for incommensurability." ((1974), p. 119)

His choice of examples is this: "[O]ne of them is Darwin's theory of evolution of species, and the other is Lord Kelvin's physical theory of heat. *What could be incommensurable if these are not?*" (*ibid.*, pp. 119-120; italics added).

Hereafter Hattiangadi sets out to show that Darwin and Kelvin "found themselves disagreeing over facts" (*ibid.*, p. 120), so that the theories in question are not incommensurable, so that the theories of Newton and Einstein likewise cannot be incommensurable! We need not, however, consider the "crucial test" between Hattiangadi's example theories, for we may answer his above exclamation directly and at the same time take the ground from under his feet. The theories he cites as examples are *not* "candidates for incommensurability" - contrary to what he assumes - because they are not theories claiming something with respect to the same sphere of phenomena and its nature. And indeed, a suitable answer to Hattiangadi's "What could be incommensurable if [Darwin's and Kelvin's theories] are not?" is this: These two theories do not represent the situation envisaged in the incommensurability thesis; this situation obtains for example in the case of Newton's and Einstein's theories, which propose incommensurable conceptions *partly* of one and the same sphere of nature and its basic constitution (see chapters IV and V, above), (It does not, however, follow from incommensurability that theories are not to be compared: the possibility of comparison is manifest precisely from the fact that they are incompatible. The paradox between the incommensurability and incompatibility theses is for its part possible to overcome (see esp. § 11.4, above).)

3. With the above in mind we must consider erroneous those conceptions propounded in the literature (and especially in the later Kuhn controversy) to the effect that Kuhn's model contains a non-rational, irrational (or even political) element. Such interpretations of Kuhn are associated in one form or another with an inadequate reading of the incommensurability thesis and an untenable construal of the function of the set of values V (or oblivion of its function). Almost invariably the irrationality critique involves claims of the presence of sociological (or similar) factors (see above). (See in addition to the accusations of the critics discussed in chapter III and in the present chapter, also for example Kmita (1975), pp. 67, 69–70, McMullin (1974), p. 673, Meynell (1975), p. 88, (1978), p. 249. Watanabe is not the only one of these critics to give a nihilistic reading of Kuhn (see also e.g. Meynell (1975), p. 88), but his presentation is eminently representative of this false interpretation: "[F]or Kuhn a revolution means not much more than a battle between two feudal lords, or an interminable chain of revolutions and counter-revolutions in some part of the Western Hemisphere" (Watanabe (1975), p. 115).)

4. We have in the foregoing rejected the irrationalistic-sociological mode of reading Kuhn as erroneous. Kuhn himself, however, stresses that the basis of his position is sociological (see for example (1970c) *passim* and p. 253). Regarded from the standpoint of the present work the matter is to be understood as follows. The achievement of normal-scientific conceptual change and precipitation of a scientific revolution presupposes a certain "pragmatic" activity (actualization), which proceeds in a "circular" manner from the conditions imposed by the prevailing paradigm and in which, on the basis of those very conditions, a step is taken beyond the bounds of knowledge hitherto actualized, that is, into the unknown. For such an activity there is, however, no set of rules which would make it possible to compute *in advance* for every situation the procedure ensuring a successful step beyond the existing knowledge. But there *are*, on the other hand, norms applying to scientific communities as communities *per se*, whose observance can ensure the preservation of such communities which

sincerely and responsibly do what *can* be done for the acquisition of knowledge in the situation at hand (and what can be done may constitute a revolution). These norms are, *for example*, the following (see e.g. Kuhn (STR), pp. 168–169): (a) It is the duty of a scientist to study nature (not to propound visions). (b) Research must confine itself to clearcut problems (as opposed to disputations). (c) The results obtained by a member of a scientific community must enjoy acceptance *within* that community: the community is the only and the ultimate judge. (d) A member of a scientific community must not, in matters touching his profession, appeal any more to political decision-makers than to the public at large: in matters concerning the results of science there are no outside referees.

Conditions (a) - (d) - and the list might of course be extended - constitute, together with the set of values V, the "sociological" basis in Kuhn's theory: their function, however, is a striking one, namely to counter the influence of genuinely *sociological* (and for example political) factors on the course of science and to ensure the preservation of professionalism among scientists. (An unfortunate weighting of values will lead swiftly to a halt in progress, as we have seen.) This is no insignificant task, and its accomplishment is not possible without the "sociological" dimension described; by reason of the "pragmatics" of actualization the abstract objectivity of the traditional view of science cannot give a correct course of science, because in that view values and norms do not have an "objective" position.

According to Kuhn's critics observance of his model would allow of the making of revolutions in science at the behest of a political instance. It must be admitted that such a train of events would be (and has been) quite possible in the world of reality; only what is involved would not be (and has not been) - in contrast to what the sociological reading of Kuhn implies - in any sense a Kuhnian scientific revolution, for it would contravene the above conditions (a) - (d) (and has in fact done so). (In fact we are here appealing to sociological criteria to acknowledge that a given set of events is not a scientific revolution but a sequence adulterated by sociological factors and threatening to halt the progress of science!) Although Kuhn's theory is thus irrevocably "sociological" in the sense described, it is not sociological in the sense intended by his critics, for the Kuhnian "sociological" dimension is designed to *prevent* the entry of sociological and similar factors into the development of science as far as this is possible. To summarize, Kuhn's basis is sociological in a sense: progress in science is possible so long as certain sociological conditions guarantee the preservation of a certain tradition of "pragmatics". This also has an emancipatory function, seeking for example to keep natural scientific research apart, or to free it, from technical interests.

5. One mode of approach to Kuhn and to the course of science is a kind of psychological-educational appraisal; for example Popp writes:

> "Kuhn once remarked that "the talking-through-each-other that regularly characterizes discourse between participants in incommensurable points of view requires much further study". This inquiry is, it seems, within the domain of educational research. ... No doubt the free movement from framework to framework mentioned by Popper is not a characteristic of many [sic!] researches ... . But to what extent is it learned? That is, does one's *educational* experience render him more or less able to take the other person's view?" ((1975), p. 39; italics added)

Though Popp is here cautious in his wording, he apparently believes that the problem area in question can be approached by psychological-educational means. Likewise Swartz seems to perceive the nature of normal science as a matter of human idiosyncracies:

> "Unfortunately, scientists are not always as open-minded as we have been lead to believe. However, even if Kuhn's historical thesis has some verisimilitude, this does *not* mean that academic communities should continue to be intolerant to the nonconformist." ((1976), p. 174)

Meynell would seem to understand the matter in like terms:

> "That some such exercise of authority is necessary, I would agree; but that it may be exercised otherwise than in uncritical deference to the prevailing paradigms seems a consequence of what I have already argued." ((1975), p. 89)

This psychological aspect finds especially clear expression in Quay:

> "Kuhn's revolutions" are to be found more in *scientists* that in *science*, and result from the same factors which cause similar upsets in students, artists, lawyers or philosophers." ((1974b), p. 351; italics added)

Behind the argumentation of these writers lies the following view. Paradigmatic boundaries are merely psychological (arising for example from idiosyncrasies, training or the like); man is after all capable of learning to understand other points of view, so that - by education and good training - the course of development of science can be rendered other than what Kuhn's model envisages (if indeed the development of science ever corresponded to his model in the first place). The extreme in this "psychology of learning" approach to Kuhn is the view of Koyrany:

> "... if what Kuhn maintains be true - that is, if in the process of learning a theory ... scientists come to see the world of their research-engagement organized in ways corresponding to the language of that theory, and thus, come to use the language of that theory to report what they see, to state what they take to be "the facts" - then ... the mere learning of any theory by scientists will effect their acceptance of it ... and scientists will never know how to apply more than one set of alternative theories ...
>
> Thus, scientists who have learned one theory will experience a "gestalt-switch" when they attempt, successfully, to *learn* an alternative theory, and as a consequence, will no longer know how to apply the language of the first theory ..." ((1979), p. 55; italics added)

According to what has been proposed in the present work, the (erroneous) view of the above-mentioned writers arises from their ignoring the basic function of normal science, namely actualization. We might express the matter briefly thus. It is of course possible to learn other theories besides "one's own", theories which are in competition with it (for example a historiographer of science may find himself having to learn the stands adopted in a number of successive theories). But the fact that someone may have swotted up - as is quite conceivable - the languages of, say, two rival paradigms, is *not* in conflict with the incommensurability thesis: the actualization *processes* of competing paradigms are incommensurable, but this does not prevent one from learning to understand the results achieved within the framework of *each* (see Chapters IV and V, above). By means of good and comprehensive training persons of more expansive understanding can be produced, it is true, but - if Kuhn is right - this will in no way affect the basic structure of scientific progress. We shall conclude with a reference to Blackmore, whose stand in this context is illuminating:

> "Furthermore, in the light of Kuhn's theory of 'scientific revolutions' and 'incommensurable paradigms', it is interesting to point out that Planck had the strength of character [sic!] not only to admit he was wrong in opposing Boltzman but to *start* the quantum tack of 'modern physics' and become the first *major supporter* of Einstein's special theory. He bridged the 'incommensurabilities' of 'classical' and 'modern' physics, contributed to the old and the new, supported the soundest features of both, and perhaps most important vis-à-vis Kuhn, *understood both*." ((1978), p. 347)

Blackmore thus feels he has shown the untenability of the incommensurability thesis on the basis of a single individual's having "*understood both*" (italics Blackmore's) competing viewpoints. But - in our own reading of Kuhn - the incommensurability thesis does *not* apply where Blackmore would have it and that is why Blackmore tumbles down with his impossibility proof of incommensurability: the thesis means - to put the matter metaphorically - that nobody (not Planck any more than Blackmore) can put together a jigsaw puzzle with pieces picked from the boxes of two different puzzles, no matter how cleverly he may have put together each of them separately.

KEY TO THE SYMBOLS USED

We assume in the reader a general background in the philosophy of science and a sufficient knowledge of mathematical set theory and mathematical logic together with the symbols they employ. Mention may, however, be made of a number of definitions involved in the present work.

We mean by *vocabulary* V a finite, non-empty set whose elements we call *terms (symbols)*. We get a *formula* over vocabulary V by placing a finite number of its elements in succession. Let W(V) be the set formed by all the formulas over V. We then call each subset of W(V) a *language over vocabulary V* (or a *V language*).

Let X be a formula over vocabulary V. If $F_1, \ldots, F_n \in V$, $F_i \neq F_j$ for every $i \neq j$, when $i, j = 1, \ldots, n$ and $f_1, \ldots, f_n \in V$, then

$$S^{F_1 \ldots F_n}_{f_1 \ldots f_n} X|$$

means the formula X* over V we get when we write in X simultaneously in place of symbols $F_1, \ldots, F_n$ in corresponding order $f_1, \ldots, f_n$.

In the case of functional calculus we observe the customary modes of designation throughout. For the sake of clarity a few examples may be brought out here. Let Γ be a set of well-formed formulas and B a well-formed formula. In this case the expression $\Gamma \vdash B$ means that there is a finite subset of Γ, $\{A_1, \ldots, A_n\}$ such that $A_1, \ldots, A_n \vdash B$. Further, the set $\{A | \Gamma \vdash A\}$ we designate $D(\Gamma)$. It is easily proved that $D(D(\Gamma)) = D(\Gamma)$. Further, let Γ be a set of well-formed formulas. By $CON(\Gamma)$ we then designate the conjunction of the well-formed formulas in Γ. Finally, the set of singular statements, fixed in a given context, is abbreviated as SING.

Let Γ be a set. Here we write the complement and the cardinal number of Γ, in that order, $C(\Gamma)$ and card (Γ). Let $R_1, \ldots, R_n$ and

$S_1, \ldots, S_n$ be sets. Now the inclusion of an ordered n-tuple $R = [R_1, \ldots, R_n]$ in the ordered n-tuple $S = [S_1, \ldots, S_n]$ is defined in the familiar manner and designated $R \sqsubseteq S$. The corresponding genuine inclusion we write $R \sqsubset S$.

NOTES

* Sometimes in the text concepts are first used without explication or definition, thus assuming in the reader a general background in the philosophy of science, then those concepts are later clarified, and perhaps clarified again.

** Since our approach to Kuhn is immanent, formal reconstructions of Kuhn's theory, for instance the set-theoretical reconstruction which Stegmüller (1976) bases on the work of Sneed (1971), are excluded from consideration.

1. The division here is based partly on that of Lakatos (1970).

2. We wish to point out that test theory does not fall within the scope of the present work. We shall therefore not pass an opinion as to the various logical forms the instances supporting the given generalization may have.

3. The proving untrue or alternatively the falsification of theories is of course also involved in other branches of justificationism. In this context note the following. When we say for example that a theory is *falsified* by *modus tollens*, we mean this: Let $A_1, \ldots, A_n$ be the theoretical points of departure in question, the theory $T = \{A_1, \ldots, A_n\}$ and $I_1, \ldots, I_m$ certain additional conditions. If now there is a B such that the conditions

   (i) $(A_1 \wedge \ldots\ A_n) \wedge (I_1 \wedge \ldots \wedge I_m) \rightarrow B$

   and

   (ii) $\sim B$

   hold, we may have, assuming that the additional conditions are established, the conclusion $\sim (A_1 \wedge \ldots \wedge A_n)$ and *say* now that the theory T is falsified. The expression 'the theory T is falsified' thus means that the *conjunction* of the components of the set T is falsified.

4. On the development of positivism, see e.g. Feigl (1956), Krohn (1949), (1950), and von Wright (1945).

5. We would like to emphasize that the *phenomenalism* - *physicalism* controversy cannot be taken up within the framework of the present work.

6. See however Suppe (1974), p. 16, note 32 and pp. 45-49 and

66-85 *passim*. We pass over in this context certain divergent views such as have been expressed by Otto Neurath, among others.

7. Be it noted that according to Carnap the inapplicability of inductive logic in these cases arises mainly from *practical* reasons ((1971), p. 243). It is to our mind, however, "more likely" to be a matter of principle (see later parts of this work).

8. Note that Carnap feels here that *intuition* gives the term's cognitive significance with such certainty that the demand for explicit definition has been waived. In this context one may consider the following comment of Hempel:

> "But it is now rather generally agreed that theoretical terms do not usually admit of such definitions [the form of definitional biconditional statement]. In the nature of the case, *there can be no conclusive proof of this view*: strict proofs concerning definability can be given at best for expressions of a precisely formalized language." (Hempel (1971), p. 373)

9. I.B. Cohen gives a particularly vivid account of Carnap's markedly ahistorical view of the relationship between the philosophy and the history of science (see Cohen (1974), p. 310, note 10).

10. On the *nature* of methodological rules see esp. Johansson (1975), pp. 1-14, and on methodological rules as such pp. 15-200. According to Wolfgang Stegmüller Popper's theory (at least as expressed in *Logik der Forschung*) may be interpreted in three ways:

> "Popper's discourses in [Popper (1968)], as well as in his other epistemological writings, can be interpreted [1] first as *historical observations* or the rational reconstruction of such. [2] Second, they can be regarded as the intuitive spadework for metascientific analysis, or for the reconstruction and explication of certain concepts of the metatheory of the empirical sciences. ... [3] A third interpretation is the *normative methodological*. ((1976), pp. 249-250; numbering in squared brackets added)

Our emphasis in the present work is on interpretations [2] and [3]. Loose (1972) stresses interpretation [3], likewise Johansson (1975). Johansson also makes a comparison of Popper and Kuhn (pp. 121-135), but fails, in our opinion, to construe correctly the relationship between the two.

11. A basic statement claims that an observable event takes place in a certain "sufficiently" concisely demarcated area, the demarcation given by certain fixed space-time coordinates k (Popper (1968), pp. 88-89, 102-103). As regards basic statements note also this:

> "The 'empirical basis' consists largely of a mixture of *theories* of lower degree of universality (of 'reproducible effects')" (Popper (1969), note 8 beginning on p. 41)

At the same time note on the other hand that for Popper every statement - be it as simple as 'Here is a glass of water' - is of the nature of a theory (hypothesis) ((1968), pp. 94-95).

12. In this connection it is unnecessary to ponder whether this interpretation of Wittgenstein is correct; on this point consult for example Griffin (1964), p. 4 and *passim*.

13. A brief comment may be appropriate on one of the interpretations of the demarcation criterion to be found in the literature. This interpretation is part of the so-called *Popper legend,* whereby the falsifiability condition is a criterion of meaning which - at the same time as it distinguishes science and metaphysics from each other - excludes metaphysics in its entirety from the scope of meaningful discussion.

    Now according to Popper metaphysical ideas (e.g. atomism) have exerted an influence on the development of science. This would be difficult to understand if such metaphysical ideas were entirely devoid of meaning.

    Popper's demarcation criterion is then *not* intended - despite the assertion of the Popper legend to the contrary - as a definition of the concept of meaning. Nor is the demarcation criterion intended to *eliminate* metaphysics - as if it could do so. Popper in many contexts stresses the significance of metaphysics for science. (On the foregoing see e.g. Popper (1968), p. 38, (1969), pp. 184-200, (1974b), pp. 963-974.)

    Let also the following be noted. A statement bearing an unrestricted existential quantifier is according to the demarcation criterion metaphysical. On the other hand such a statement may be verified. If the Popper legend held true we might construct a set of statements whose elements were at one and the same time verifiable and meaningless - which of course would be nonsensical (see Popper (1974b), p. 964).

14. Note that the demarcation criterion itself cannot be grounded otherwise than by appeal to its fruitfulness (see Popper (1968), pp. 38, 55). Popper also had an autobiographical motivation for the formation of his demarcation criterion (see (1969), pp. 33-65, (1974b), p. 976).

15. Be it noted that, according to Popper,

    > "... a theory makes assertions only about its potential falsifiers. (It asserts their falsity.) About the 'permitted' basic statements it says nothing. In particular, it does not say that they are true." ((1968), p. 86)

16. Popper eliminates classical conventionalism by the introduction of a set of methodological rules, thus:

    Let T be a theory. Now the falsification of T can be avoided for example by the following "conventionalist stratagems": (1) the addition of *ad hoc* hypotheses, (2) the alteration of definitions, and/or (3) by ignoring the falsifying observations (Popper (1968), pp. 41-42).

A fourth means of evading falsification is *implicit definition*. If undefined terms in axiomatic theory T are so interpreted that the system of axioms in question implicitly defines them, then the theory cannot be falsified by means of any empirical material, for in the sense of the implicit definition it applies exclusively to those phenomena "for which it holds".

In the classical conventionalist view certain fundamental laws of nature cannot be falsified by observation, since it is precisely these laws which determine what observation and measurement are (Popper (1968), p. 79). In the background here is a mode of thought based on the fourth conventionalist type of stratagem, which Popper holds can hardly *as such* be shown untenable (*ibid.*, pp. 79-80). If thus the criterion of demarcation were applied to sets of statements as such, it would be possible to show almost every theory to be unscientific in the light of that criterion. But, Popper feels, this shows only that

> "... [the] criterion of demarcation cannot be applied immediately to a *system of statements* ... The question whether a given *system* should as such be regarded as a conventionalist or an empirical one is misconceived. *Only with reference to the methods applied* to a theoretical system is it at all possible to ask whether we are dealing with a conventionalist or an empirical theory. The only way to avoid conventionalism is by taking a *decision*: the decision not to apply its methods." (*ibid.*, p. 82).

Classical conventionalism is thus eliminated by methodological rules (norms) whereby (for example) the above four stratagems are prohibited:

(1') Auxiliary hypotheses may be added to a theory only where this does not diminish the testability of the theory.

(2') Changes of definition must be defined.

(3') If the Popper procedure is repeatable and each time leads to negative results, the theory is to be abandoned.

(4') A theory must not be rendered immune to falsification by means of implicit definition. (On the foregoing see *ibid.*, pp. 41-42, 72-84.)

Be it noted, then, that although Popper's own theory is imbued with a conventionalistic element, it tends to diverge radically from classical conventionalism: a universal statement cannot in Popper's view ever be endorsed by convention (nor, therefore, can natural laws be conventions).

17. Popper has sought to formulate a variety of theory appraisals whereby rival theories might be set in order of preference. Of these appraisals (at least) corroboration is in fact "inductive" and is associated with the extent and the nature to which acceptable basic statements may be derived from a theory. The

corroborative instances for the theory a T with initial conditions I are those deductive consequences of T and I in the empirical basis EB which are accepted and are not derivable solely from I. Popper has sought to define corroboration preferences in different ways by means of corroborative instances. He points out: "The degree of corroboration of two statements may not be comparable in all cases, any more than the degree of falsifiability: we cannot define a numerically calculable degree of corroboration, but can speak only roughly in terms of positive degrees of corroboration, negative degrees of corroboration, and so forth" (Popper (1968), p. 268). Nevertheless it is possible according to Popper to give methodological rules as to degree of corroboration. In connection with degree of corroboration (or preferences associated with corroboration) there arises the following problem: Can the degree of corroboration of a given theory be now raised, now lowered (and to what extent) over a period of time? This problem is "solved" by the following methodological rules (*ibid.*, p. 268):

(i) If a theory is falsified, it loses whatever degree of positive corroboration it may have had.

(ii) Because falsification is (generally) final, positive corroboration may be replaced by negative but not vice versa.

Judging by these two methodological rules Popper would appear to fall outside the camp of theory reductionism.

18. Note that Popper has written to the new edition of Popper (1972) an appendix (Appendix 2, Supplementary Remarks (1978), pp. 363-375), where he represents some scattered remarks on the claim, voiced in these days by many, that the development of science is not cumulative. These remarks of Popper seem, however, to reach no conclusive point and are in the present work left aside. If a new Popper began to grow up from the appendix referred to; we have overlooked this and are dealing with that Popper who is usually confronted with Kuhn.

19. See however Johansson (1975), pp. 23-24.

20. On the history of the concept of scientific revolution see Cohen (1976b).

21. See also Lakatos (1970), pp. 107-108 and note 1 on p. 108. The argumentation introduced in the text we have proposed on an earlier occasion (see Verronen (1979), § 3 *passim*). At that time we were not familiar with Brown (1977b), where some of the same problems inherent in Popper's theory are stressed as in the present work.

22. See Miller (1974), Niiniluoto (1978).

23. Popper has sought to rid the concept of corroboration of justificationist and inductivist nuances. However, it is scarcely possible to overcome the inductivistic nature of the corroboration relation (between a given theory and accepted basic statements) (see also e.g. Niiniluoto (1975), p. 114).

24. We pass over for example the methodological rules pertaining to probability statements, because this problem area does not fall within the scope of our work.

25. See also Johansson (1975), p. 41.

26. A probabilist theory cannot *in itself* be in conflict with the evidence, so that certain methodological rules are needed to ensure its empiricity. As we have pointed out above, problems associated with probability are passed over in this work.

27. To Lakatos' doubts as to the possibility of refuting Newton's theory according to Popper's methodology, Popper replies:

> "There *are an infinity of possibilities,* and the realization of *any* of them would simply refute Newton's theory ... This theory would ... be contradicted if the apples from one of my, or Newton's, apple trees were to rise up from the ground (without there being a whirlwind about), and begin to dance round the branches of the apple tree from which they had fallen ..." (Popper (1974b), p. 1005; italics added)

Popper's attitude to the *ceteris paribus* condition is likewise one of outright rejection:

> "No *ceteris paribus* clause is necessary. I do not want to get involved in the morass of a discussion of these clauses; but I must say that I hold that most of the discussion of *ceteris paribus* clauses and all of the appeals to them are misleading. As a condition or antecedent, the clause "*ceteris paribus*", ... is, of course, never satisfied in this world. Such an antecedent would therefore empty a theory to which it was attached of any empirical content ... I ... suggest that *ceteris paribus* clauses should be avoided and, more especially, that they should not be imported into the discussion of the methodology of the natural sciences." (*Ibid.*, p. 1186, note 75)

As to Popper's *own* theory, on the other hand, it may not after all be quite superfluous to add that his passage on the behaviour of apples (see above) in (1974b) continues thus:

> "... and if all this were to happen, *without* any other *very obvious* changes in our environment." (P. 1005; italics added)

Popper's theory does not, however, afford an answer to the question of when changes in our environment might be said to be "very obvious", when, for example, in the case of experiments concerning the microcosmos such changes might be regarded as "very obvious" and when again as *not*. What is the relationship between Popper's 'without' and the *ceteris paribus* clause? Correspondingly the argumentation with which Popper rejects use of the *ceteris paribus* condition seems more like a norm subjected to the demarcation criterion - whose purpose is in the manner of an *ad hoc* hypothesis to rescue this criterion - than proper grounds in the context.

28. Note in this connection Lakatos (1974), p. 244, note 1.

29. Lakatos' understanding of himself is inconstant, see e.g. Lakatos (1970) and (1974) *passim.*

30. We assume the reader to be familiar with the intuitive use of these terms. We shall also revert to them in § 6 and make a detailed analysis of them in Chapter IV.

31. See in this connection esp. also Masterman (1970).

32. Cf. Worrall (1978).

33. At least *geometric optics* would appear to constitute an exception to this rule (see Kuhn (STR), p. 79).

34. This does not exclude the possibility of using different paradigms in the solution of routine problems; on the contrary: for example the theory of Ptolemy (which has been rejected) is still applied as a calculator in such situations. However, to use a *rejected* theory as a calculator is not the same thing as to actualize a theory, that is, to practise normal science: see Chapter IV of the present work.

35. It is possible to make a retort to Kuhn's theory and to our interpretation of it in the light of the example here adduced (or of other corresponding examples). One might namely ask: Is not the statement regarding a weight swinging on a string and eventually coming to rest neutral with respect to both Aristotle and Galileo? And is not therefore a neutral observation language possible? In reply to this we would point out that since the statement in question merely *localizes* the phenomenon, and a localizing statement has *nothing to say* of that phenomenon as part of for example Galileo's theory or Aristotelian mode of thought, this is not the case. (Pursued far enough, the critical remark in question would lead to a situation where, having described the operation of an electric switch, one might claim to have participated in the development of electronics.)

36. Kuhn's citation, see (STR), p. 151.

37. The researcher is *not,* of course, bound to a single paradigm unless it is assumed that he is faced with two paradigms of the *same* dicipline. Since it is not clear whether Watkins is aware of this, we make this assumption on his behalf (otherwise his argumentation would be nonsensical).

38. We assume Popper's theory of worlds I, II and III to be familiar (see Popper (1972)).

39. Kuhn does not hold that rejected elements are necessarily

erroneous in the usual sense - such rejections of errors even the text-book tradition admits to.

40. If the distortion is conscious and deliberate, the sociological aspect of the problem of knowledge becomes even more interesting.

41. and humane.

42. Text-books influence the image of science, which influences text-books and so on.

43. With this we would not, however, wish to make of Koyré a representative of "repeated Kuhnian revolutions".

44. Cited by Cohen, see (1974), p. 319, note 37.

45. As to Galileo himself, it is problematic to associate even the law of inertia unequivocally with his name - a point taken up in later sections; see esp. Cohen (1974), and also Koyré (1968), esp. e.g. p. 2:

    > "And it is only [Galileo's] reluctance to draw ... the ultimate consequences ... of his own conception of movement ... that prevented him from making the last step on the road which leads from *the finite Cosmos of the Greeks to the infinite Universe of the Moderns.*" (Italics added)

46. 'Per se' in this special meaning is in contrast to the Aristotelian mode of thought.

47. Cited by Cohen after the translation by Andrew Motte, see Cohen (1974), p. 329.

48. Be it noted for example here that (GN1) is not in keeping with Galileo's own conception (see note 45, above).

49. Note, however, Kuhn (STR), p. 124: "Contemplating a falling stone, Aristotle saw a change of state rather than a process."

50. Quoted by Duhem, see (1954), p. 232.

51. Quoted by Cohen, see (1974), p. 338.

52. Acceptance of Duhem on this point does not however mean acceptance of cumulativity.

53. As Kuhn points out: "Those who rejected Newtonianism proclaimed that its reliance upon innate forces return science to the Dark Ages" ((STR), p. 163).

54. The present author is aware of few who have addressed attention to the analysis of actualization and/or articulation in Kuhn's theory. *M. Martin* considers the aspect of articulation, but notes only one of its less essential

functions; the process of actualization is ignored by him (see Martin (1970) and (1973)). *J.C. Pitt* (1978) is more expansive on the subject of actualization, but in our view fails to reach the heart of the matter.

55. Be it noted that the concept of theory need *not* here be explicitly defined.

56. Since measuring operations are in principle theory-laden, the distinction made here can be problematized.

57. A paradigm is not strictly speaking unchangeable, since its cognitive content is partly given only in the course of actualization. Also exemplars may change in a certain sense of the word (see Kuhn (POST) p. 175)

58. The function of exemplars is an almost untouched area of study in the philosophy of science - on the problematic nature of exemplars see esp. Kuhn (POST), (1974a) and (1974b). Note particularly that exemplars are in a sense open to correction and thus subject to change:

> "Though such knowledge [knowledge that Kuhn has described as tacitly embedded in shared examples] is not, without essential change, subject to paraphrase in terms of rules and criteria, it is nevertheless systematic, time-tested, and in some sense *corrigible*" (Kuhn (POST), p. 175; italics added).

59. The dilemma arising (that there are statements which are neither definitions nor neutrally testable) has thus evoked the solution whereby there is a "third category of statements" (on criticism of this solution see e.g. Feigl (1956)). One suggestion as to the nature of this "third type" comes from McGuinness, who speaks of statements which

> "... are not things put forward which we acknowledge but things which all our actions in a certain way show that we take for granted. I question ... therefore, von Wright's remark that they have "a peculiar logical role." This seems to suggest that they are a *third class of propositions alongside the a priori and the a posteriori* [italics added]; in reality, however, ... they have not an intrinsic logical character but a special relation to us, which Wittgenstein often describes in terms of the way of life, the fundamental decisions, the *faith* of a community. ... these certainties give us the framework both of a priori and of a posteriori knowledge [italics added]." ((1972), p. 64).

McGuinness refers in this context to Kuhn's paradigms (p. 65; see also von Wright (1972), pp. 59-60 and Wittgenstein (1974), particularly §§ 94-99, esp. 94, which von Wright selects as exemplars). Can McGuinness' suggested localization of "third propositions" be of any use in the solution of the dilemma raised by paradigmatic points of departure? In our opinion the answer is negative, because paradigmatic points of departure have the following properties:

(i) Let P and Q be successive paradigms. The scientific revolution P/Q means that the theory $G(P)$ was replaced by theory $G(Q)$.

Let us now consider a given paradigm R. Here a part of the paradigmatic starting-points of R, particularly elements in the set U, are not *a priori*, and there is to our mind no reason to say that they are not *a posteriori* (localization of the third category from the standpoint of this work does not take place at this point), since the connection of the paradigmatic starting-points (esp. of the symbolic generalizations of U) with experience appears as follows: (a) the series of successive theories ... $G(P_i)$, $G(P_{i+1})$, ... displays the compelling force of *empiria* to change points of departure; (b) success in the formation of a series of actualization of one and the same paradigm R

(*) ... $G_i(R)$, $G_{i+1}(R)$, ...

renders the empiricity of the points of departure obvious.

(ii) Of the paradigmatic starting-points the elements in the set U particularly clearly do not represent what in the normal sense of the word one might call "the *faith* of a community": the sequence of actualization (*) is surely not the outcome of faith.

(iii) Paradigmatic starting-points can in no way provide us with "the framework both of a priori and of a posteriori knowledge": in the sphere of empirical theories the development of science is not cumulative, but on the other hand the progress of science which is independent of experience (mathematics) *is* cumulative.

60. The paradox inherent in Kuhn's theory was perhaps first noticed and formulated by Scheffler (see (1967), p. 82).

61. If the source in question had actually appeared even earlier, this would not invalidate our remarks on Toulmin but strengthen our position.

62. In fact Kuhn (1961) [1961] is a polished version of an earlier talk (see Kuhn (1961), p. 161, n 2).

63. See e.g. Kuhn (1963b), p. 347, n 1.

64. See the introductory part of the book (edited by Crombie) of which Kuhn (1963b) and (1963c) form a part.

65. Kuhn (STR), pp. i-xii.

66. Kuhn (POST), p. 174.

67. See p. vii of the book (edited by Lakatos & Musgrave) of which Kuhn (1970b) forms a part.

68. See p. vii of the book (edited by Suppe) of which Kuhn

(1974a) and (1974b) form a part.

69. Our expression here is misleading in that it may give the impression that we accept Toulmin's statement that Kuhn's theory is explanatory: if explanation means what it means in empirical science, Kuhn's theory is *not* explanatory.

FINNISH SUMMARY - TIIVISTELMÄ
TIEDON KASVUN ONGELMA: TUTKIMUS KUHNIN PARADIGMA-TEORIASTA

Käsillä olevassa työssä olen tutkinut Kuhnin paradigma-teoriaa, jolle olen antanut oman tulkintani. Tuon tulkinnan puitteissa tarkastelen tieteellisten teorioiden rakennetta ja luonnetta sekä tieteen kehitystä.

Saavuttaakseni tavoitteeni olen menetellyt seuraavasti. Aluksi luvussa I karakterisoin sitä käsitystä empiiristen teorioiden rakenteesta ja luonteesta, jota kutsun traditionaaliseksi. Valaisen traditionaalisen tiedekuvan eri haarojen - esim. ns. ortodoksisen tiedekuvan - käsitystä tieteen edistymisestä ja toisaalta seuraan niitä askelia, joita ottamalla siirrytään traditionaalisen tiedekuvan sisällä yhdestä positiosta toiseen, edellisen kanssa kontroversiaalisessa suhteessa olevaan. Luku I on työni johdatteleva osa, jossa systematisoin ja nostan esille sen taustan, jota vasten Kuhnin teoria mielestäni parhaiten on ymmärrettävissä. Luvun I rajaus seuraa tästä tavoitteesta, joten en ole voinut traditionaalisen tiedekuvan tarkastelussa mennä edemmäs kuin olen mennyt.

Kuhnin paradigma-teoriasta on esitetty toisistaan suuresti poikkeavia käsityksiä siinä massiivisessa kirjallisuudessa, jonka tuo teoria on katalysoinut. Traditionaalisen tiedekuvan karakterisoinnin (luku I) jälkeen olen konstruoinut Kuhnin teorian esityksen, eräänlaisen Kuhnin teorian standardi-version (luku II). Seuraavassa luvussa III olen analysoinut osaa siitä ankarasta kritiikistä, jota paradigma-teoria on synnyttänyt.

Juuri mainitut luvut - traditionaalisen tiedekuvan esitys, Kuhnin teorian esitys ja paradigma-teoriaan kohdistetun kritiikin esitys - antavat sen resonanssipohjan, jota vasten sitten luvussa IV olen rekonstruoinut oman tulkintani Kuhnin teoriasta. Tässä tulkinnassa eräs keskeinen sija on normaalitieteen analyysillä. Normaalitiede on paradigman aktuaalistamista, joka ilmenee monen muassa uusien, paradigman puitteisiin kuuluvien teorioiden syntymisenä ja entisten kehittymisenä yhä tarkemmiksi. Normaalitiedettä ajatellen aktualisaation käsite on paradigma-teorian ydinkohta - ja aktualisaation käsite osoittautuu tutkimukseni valossa odottamattamalla tavalla keskeiseksi myös teorioiden yhteismitattomuutta ajatellen. Näin ollen teorioiden aktualisaatioprosessi ei ole ratkaisevaa vain normaalitieteen kannal-

ta, vaan myös tieteellisiä vallankumouksia ajatellen. Siksi on ollut tutkimukseni edistyessä yllättävää havaita, että Kuhnia käsittelevässä kirjallisuudessa aktualisaation käsite kutakuinkin kokonaan sivuutetaan.

Tieteenkehityksen tarkastelussa on tärkeätä ottaa huomioon suksessiivisten paradigmojen yhteensopimattomuus ja yhteismitattomuus, joiden seurauksena tieteen edistymisessä on murroksia - ja ne ovat "parantumattomia", jos Kuhn on oikeassa. Teesi suksessiivisten paradigmojen yhteensopimattomuudesta ja *samanaikaisesta* yhteismitattomuudesta aiheuttaa kuitenkin paradoksaalisen tilanteen. Jos suksessiiviset paradigmat ovat yhteensopimattomia, niiden on oltava vertailtavissa: Kuinka ne siis voivat olla yhteismitattomia? Jos taas paradigmat ovat yhteismitattomia, niin kuinka ne voivat olla niin hyvin vertailtavissa, että ne voidaan todeta yhteensopimattomiksi! Tämän paradoksin olen pyrkinyt työssäni ratkaisemaan.

Tieteen kehityksen murroskohtien sekä teorioiden yhteensopimattomuuden ja yhteismitattomuuden tarkastelemiseksi olen introdusoinut jäädytetyn teorian ja kalkylaattorin käsitteet vastakohtana sille, mitä tarkoitan aidolla teorialla, tieteen kehityksen perusyksiköllä. Tältä pojalta päädyn tulokseen, jonka mukaan tieteelliset murrokset sekä teorioiden yhteensopimattomuus ja samanaikainen yhteismitattomuus ovat tieteen edistymisen aitoja seuralaisia.

Työni loppupuolella luvuissa V ja VI olen tutkinut paradigmateoriaan kohdistettua kritiikkiä, johon olen osin jo luvussa IV puuttunut. Näin artikuloin edelleen omaa tulkintaani paradigmateoriasta.

Tieteellisten vallankumousten osalta paradigma-teorian vastaisen kritiikin eräs keskeisväittämä on, että kahden suksessiivisen paradigman edustajat eivät ollenkaan voi ymmärtää toisiaan, seikka, joka ilman muuta merkitsisi tieteellistä anarkiaa. Tässä yhteydessä otan esille myös tieteellisen kriittisyyden, jolle aktualisaatioprosessin luonteen pohjalta annan disputatiivisesta kriittisyydestä poikkeavan sisällön. Korostamalla eräänlaista pragmaattista kriittisyyttä päädyn käsitykseen, jonka mukaan paradigmat ovat vertailtavissa tietyn tieteen arvojärjestelmän säilyessä tieteen historian läpi; tästä huolimatta paradigmojen yhteismitattomuus ei ole eliminoitavissa (kuten työssäni tarkemmin olen esittänyt).

Muuan paradigma-teoriaan kohdistetun kritiikin keskeisväittämä on se, että normaalitiede ja rutiini ovat sama asia ja että normaalitiede on vaara koko tieteelle. Tähän sisältyy esim. se Feyerabendin huomautus, että jos normaalitieteen kuhnilainen kriteeristö otetaan vakavasti, niin mitä omituisimmat seikat - esim. kassakaappimurtoihin erikoistunut järjestäytynyt rikollisuus - lankeavat tieteellisen toiminnan piiriin. Tämän *Dillinger*-dilemman olen työssäni ratkaissut: Normaalitiede ei ole sama asia kuin rutiini eikä normaalitieteilijä ole säälittävä dogmaatikko, vaikka Kuhnin oppineet kriitikot sellaista väittävät. Normaalitiede on osoittautunut suorittamani tutkimuksen valossa pitkälle juuri perustutkimukseksi ja sen edustajia eräässä normaalitieteellisessä traditiossa olivat esim. Euler, Lagrange, Laplace ja Gauss, joista käsitykseni mukaan kukaan ei onnistunut täyttämään Poppperin käsityksiä normaalitieteilijästä, nimittäin, että olisi ollut "huonosti koulutettu" tai "indoktrinaation uhri" - ja joiden edustama normaalitiede Newtonin jalanjäljissä ei, jos olen asian oikein ymmärtänyt, ollut sitä, mitä Popper väittää normaalitieteen aina olevan, nimittäin "vaara tieteelle ja todella koko sivilisaatiollemme".

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