EMPIRICAL LAWS, REGULARITY AND NECESSITY

H. KONINGSVELD

STELLINGEN

I

Austin's analyse van de sense-datum theorie leidt tot een verwerping van deze theorie, mits de analyse wordt doorgezet naar het niveau van de begripsvorming en geen halt houdt bij een beroep op het gewone taalgebruik.

J. L. AUSTIN, *Sense and Sensibilia*, Oxford, 1962.
A. J. AYER, 'Has Austin refuted the sense-datum theory?', in *Synthese*, 17, (1967).

II

Ryle's onderscheid tussen het denkproces en de verbale descriptie van gedachten heeft hem onbewust tot de conclusie gevoerd, dat situaties, waarin mensen rationeel handelen, kunnen worden beschreven zonder dat het noodzakelijk is te verwijzen naar verbaal gedrag. Dit wordt duidelijk, zodra we opmerken, dat verbaal gedrag binnen Ryle's behaviouristische visie verbonden is met spreken of pogingen om te spreken.

G. RYLE, *The concept of mind*, London, 1949.
G. RYLE, 'Thinking and language', 'Thinking and reflecting', 'The thinking of thoughts', in zijn *Collected Papers*, vol. 2, London, 1971.

III

Het is onjuist om, zoals dat bijvoorbeeld door de linker- en rechtervleugel van de Wiener Kreis is gedaan, de correspondentie- en coherentietheorie als alternatieve waarheidstheorieën te typeren. De correspondentietheorie vooronderstelt een coherentietheorie.

IV

Bekendheid met kennistheoretische en methodologische grondslagen van het door hem onderwezen leervak moet met name voor de leraar bij het V.W.O. van belang worden geacht.

V

De discussie over de methode van de sociale wetenschappen tussen subjectivisten en naturalisten heeft in het licht van een post-positivistische wetenschapsfilosofie aanmerkelijk aan betekenis ingeboet.

P. C. SEDERBERG, 'Subjectivity and Typification: A note on Method in the Social Sciences', in *Philosophy of the Social Sciences*, 2, (1972).
M. NATANSON (ed.), *Philosophy of the Social Sciences: A Reader*, Part III, New York, 1963.

VI

De pogingen van Thomas Graham (1805–1869) om een verklaring te geven van het verschijnsel der kolloïde oplossingen illustreren de in dit proefschrift ontwikkelde theorie van de empirische wet.

H. R. KRUYT en J. TH. G. OVERBEEK, *Inleiding tot de fysische chemie*, Amsterdam, 1958[15].
Dit proefschrift, ch. V.

VII

Het deductief-nomologische verklaringsmodel van Hempel is juist daarom ontoereikend, omdat de term 'nomologisch' hier verwijst naar de regelmaatsopvatting van een empirische wet.

C. G. HEMPEL, 'Studies in the logic of explanation', in zijn *Aspects of scientific explanation and other essays in the philosophy of science*, London, New York, 1965.

VIII

Veel hedendaagse beoefenaren van de natuurwetenschappen geven dezelfde kennistheoretische interpretatie van hun theorieën als de theologen uit Galilei's tijd van Copernicus' theorie.

K. R. POPPER, 'Three views concerning human knowledge', in zijn *Conjectures and Refutations*, London, 1963.

IX

Logisch gezien is de sterkte van het geluid, dat een mondelinge redenering vergezelt, volledig irrelevant. Empirisch gezien helaas niet.

X

Het verwerpelijke van uitspraken van politici is vaak, dat ze veel te vaag geformuleerd zijn om verworpen te kunnen worden.

XI

Als empirisch onderzoek tot een zuivere abstractietheorie van de begripsvorming zou leiden, dan moet het kennistheoretisch uitgangspunt van dit proefschrift als onhoudbaar worden gekwalificeerd.

Proefschrift van H. KONINGSVELD
Wageningen, 13 april 1973.

EMPIRICAL LAWS, REGULARITY AND NECESSITY

Dit proefschrift met stellingen van

HERMAN KONINGSVELD,

doctorandus in de wijsbegeerte, geboren te Helmond op 15 oktober 1939, is goedgekeurd door de promotor Dr. A. M. T. Meyer, hoogleraar in de Wijsbegeerte.

De Rector Magnificus van de Landbouwhogeschool,
H. A. LENIGER

Wageningen, 20 februari 1973.

H. KONINGSVELD

EMPIRICAL LAWS, REGULARITY AND NECESSITY

(met een samenvatting in het Nederlands)

PROEFSCHRIFT

TER VERKRIJGING VAN DE GRAAD
VAN DOCTOR IN DE LANDBOUWWETENSCHAPPEN,
OP GEZAG VAN DE RECTOR MAGNIFICUS,
PROF. DR. IR. H. A. LENIGER, HOOGLERAAR IN DE TECHNOLOGIE,
IN HET OPENBAAR TE VERDEDIGEN OP VRIJDAG 13 APRIL 1973
DES NAMIDDAGS TE VIER UUR IN DE AULA VAN DE
LANDBOUWHOGESCHOOL TE WAGENINGEN

H. VEENMAN & ZONEN B.V. – WAGENINGEN 1973

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Aan mijn Moeder,
Aan Ina, Haye, en Sophia

CONTENTS

PREFACE

In this book I have tried to develop an analysis of the concept of an empirical law, an analysis that differs in many ways from the alternative analyses found in contemporary literature dealing with the subject.

I am referring especially to two well-known views, viz. the regularity and necessity views, which have given rise to many interesting papers and books within the philosophy of science.[1]*

In developing my own views, it very soon became clear to me that the mere restatement of these alternative views, followed by a discussion of their defects and an explanation of my own view, would not suffice to show what I regard as basically unsound in these views.

If we seriously consider the well-known difficulties facing the regularity view, we have to consider the possibility that the time has arrived to stop our attempts to solve, by means of patch-work additions, the fundamental problem of empirical laws within the traditional context of logical positivist doctrines.

I have tried to find a solution which is based on a different philosophical context, the main incentive being that the discouraging results of the customary attempts to solve the problem might have been the outcome of the fundamental philosophical setting within which the problem had been formulated and within which everyone looked for a solution. The problem of empirical laws is not a problem existing by its own rights – it is shaped by an underlying point of view or theoretical framework.

For this reason I have started with a brief sketch of the logical positivist context. However, I want to stress the fact that my intention was not to offer an historical exposition, but an exposition which becomes meaningful when related to the subsequent arguments concerning empirical laws.

The notion of direct or theory-free observation has been taken as the fundamental characteristic of the logical positivist point of view. This view has been further characterised by means of five theses, among which is the thesis that observational terms are isolated or theory-independent units of extensional meaning. These terms are then supposed to be the starting-points of meaning and the end-points of confirmation, and these very characteristics mark the privileged position such terms have in a logical positivist philosophy.

The post-positivist view[2], on the other hand, is characterised by the opposing notion of theory-loaded observation. This notion leads to other conclusions which radically oppose some of the theses of logical positivism. The notion of theory-loaded observation itself, although often clearly stated, has not yet been systematically elucidated, and I have made an attempt to do this in ch. I after my sketch of the logical positivist context. This seemed to me to be necessary

* See NOTES, p. 109.

in order to make my post-positivist point of view as clear as possible, before tackling the problem of empirical law itself.

I have paid special attention to the lawlikeness of concepts and their confirmation and falsification.

These epistemological issues are followed by a separate examination of Goodman's riddle in ch. II, because it lies somewhere between the lawlikeness of concepts and the lawlikeness of laws. If Goodman's riddle is taken as a serious problem, as I think it should, it is primarily a problem of concept or theory formation and not a problem of induction.

In ch. III the regularity view has been examined and the well-known difficulties related to such a view are considered, assuming the knowledge gained in ch. I. This has led to some interesting results, especially in connection with the analysis of counterfactuals and the confirmation of 'normal' singular conditionals, to which I return in the last chapter.

Three different views of the necessity view are briefly stated and investigated in ch. IV. RESCHER's view[3] is, I think, close to mine, which has been developed in ch. V. However, there are fundamental epistemological differences between our views.

In the last chapter I have given my own analysis of the concept of an empirical law, which should not be taken as a synthesis of a regularity and a necessity view, but as opposed to both. It is, as far as particulars may count, the result of an analysis of the concepts 'regularity' and 'necessity'. I have tried to argue that the greatest shortcoming of the regularity view lies in the fact that the proponents of such a view have not been really concerned with the concept of 'regularity' itself. They usually take this concept to be intuitively clear enough to serve as basis for their analysis. Their attacks upon the necessity view are based on the reproach that 'necessity' is a very obscure concept, and that it should, therefore, be avoided in the philosophy of science. We could, however, reproach them in a similar way, since the concept of 'regularity' is anything but clear.

One may say that an empirical law formulates a 'regularity in nature', but we can only do justice to the function a law has in science, if we are ready to view this regularity as a necessary connection, which does not exist in an observer-independent reality, but in a theoretically co-constituted empirical world.

This investigation does not pretend to answer all questions concerning empirical laws. It should, therefore, not be regarded as complete, but neither should the arguments and suggestions be regarded as completely free from ambiguity.

Much remains to be done in order to offer a generally acceptable theory and better understanding.

I. THE EPISTEMOLOGICAL CONTEXT

1. THE LOGICAL POSITIVIST CONTEXT

It is, of course, rather hazardous to speak of 'the' logical positivist theory, but with the following I do not try to give an accurate description of the view of any logical positivist. I rather want to give a model or a framework in this section, which may help the systematic analysis I want to offer.

According to the positivist view, the ultimate link between our knowledge of the world and the world itself is *direct observation.* This direct observation provides one with a special kind of knowledge expressed in so-called observational language, and however liberal the reductionism that one likes to claim and however pragmatically one wants to draw the distinction between observational and theoretical terms, I hold it to be fundamental to logical positivism that empirical knowledge gained from direct observation has an epistemologically privileged status.[4]

The pre-eminent characteristic of this kind of observation is its *theory-independence* or its *theory-freedom.* The word 'direct' in the above context indicates that no theory or no other knowledge intervenes anywhere. The naked eye or ear, etc., meets with brute facts or raw data. Assuming for the moment that there is something like direct observation and that there are observational terms and statements, let us systematically investigate what this assumption entails if we are to make sense of it. I think it will be agreed that at least the following five, mutually dependent presuppositions, must then be valid: two ontological (i and ii), two epistemological (iii and iv), and one methodological (v).

i. *The reality (or the world or nature) of which we have knowledge in common sense and science is observer-independent.*

This thesis is not exclusive to logical positivism, but that is of no importance here. It states that nature is what it is, and would be what it is, even if there were no observers[5] and the term 'direct' in 'direct observation' serves to indicate that it is of such an observer-independent world that we acquire knowledge of in direct observation. In other words, if one claims the possibility of direct or theory-free observation, one must also claim the existence of an observer-independent reality of which one gains knowledge by means of such observations.

ii. *Nature or reality is populated by mutually independent data or brute facts.* (I do not, at this stage, make a distinction between data, brute facts, events or phenomena and I shall almost always use the term 'data', when referring to this particular type of context).

With this thesis I particularly allude to the negatively formulated idea that there are no necessary connections in nature tying data together. A datum is an isolated unit, it is literally self-supporting. It is observer-independent by being an element of the independent reality, but it is also datum-independent or

independent of other data for its being that very datum. But 'as a matter of contingent fact', as one is likely to say, a datum can, spatio-temporally, be related to (e.g. 'go together with') one or more other data. So we can speak of connections between data based on such contingent relations in space-time. This is the only way in which we may legitimately say that there are empirical connections between data, and it is very important to realize that they leave the separate facts or data untouched. There is no place for other, empirical connections within the positivist context.

There is one special kind of the 'going together in space-time', namely the constantly 'going together' or the *constant conjunction* of a datum of a certain kind, A, with one of another kind, B. Such a constant conjunction is (part of) a *regularity in nature*, but again this is a purely contingent matter. Secret forces, as they are sometimes derogatively called, are dismissed and there is only the constant conjunction and the regularity without exceptions and these concepts apparently are taken to be completely transparent.[6] And like the separate or single data, a regularity also is what it is, independent of any observer. Such regularities can be known by induction, which is taken to be the pre-eminent characteristic of empirical science, and they are formulated in universal statements. This is a first-order approximation of the logical positivist view of empirical laws, the regularity view.[7]

iii. *There are observational terms which are isolated or theory-independent units of meaning.*

In direct or theory-free observation we acquire, in one way or another, knowledge of the mutually independent and observer-independent data referred to above and this knowledge is laid down in observational terms. At best these terms are the names of the data we are aware of in direct observation, which is pure, not mixed up with theoretical or subjective interference. Somehow the same data are given the same name. (I think it is in principle impossible to give a satisfactory explanation of this 'somehow' as long as one presupposes theory-free observation. However, I must pospone this point till later – see section 3). These names can only be *theory-independent units of meaning*, because theorising does not enter into direct observation at all and consequently they acquire the exact role they are preordained to play in a logical positivist methodology. The observational terms are the constants in the 'Aufbau' of empirical knowledge, having a fixed meaning, as NAGEL calls it. They are isolated or theory-independent in the sense of not referring to other terms; they have the meaning they have, independently of the meaning of other terms. In particular, they constitute the end-points of confirmation, which becomes possible just by their not referring to other terms, but I shall come back to this in section 2. At this moment suffice it to say that the meaning of observational terms should be characterised as purely extensional: there is no place for intentions*, because there is no empirical basis for such things in a positivist ontology. As

* The term 'intention' is used here in the sense of 'meaning' and not in the sense of 'purpose'. It is synonymous to what e.g. QUINE and GOODMAN call 'intension'.

there are no necessary connections tying things together in nature, similarly there are no obscure entities or mystic forces or intentions tying terms together in our language. QUINE and GOODMAN are consistent with respect to this point, being allergic to intentions, which, in their view, can only be obscure intermediary entities. CARNAP, HEMPEL, NAGEL and others are not consistent if they appeal to 'the' meaning of a term in cases where the extensional approach seems to fail.*

iv. *Observational statements are isolated or theory-independent.*

Observational statements, i.e. statements containing only observational terms as non-logical terms, are theory-independent firstly because the observational terms are theory-independent and secondly, because the 'synthetic operation', to borrow Reichenbach's term, does not introduce any theory-dependence at all.

We may elucidate this by means of an example. If we take 'raven' and 'black' as observational terms, then 'all ravens are black' is an observational statement. It is based on a directly observed constant conjunction:

$Ra_1.Ra_1$, $Ra_2.Ba_2$, ..., $Ra_n.Ba_n$ (R = raven, B = black)

and it is brought about by induction. The atomic observational statements like 'Ra_k' and 'Ba_m' are isolated or theory-independent, because 'R' and 'B' are; the conjunctions are theory-independent, because the synthesis, by being based upon a directly observable 'going together', does not introduce such a dependence and the separate data, e.g. 'Ra_k' and 'Ba_k', remain what they were before the synthesis; and the universal statement is also an isolated or theory-independent unit, because induction does not create any theoretical connections either. In particular, induction does not introduce any obscure, *stronger than extensional* connections. The separate data again remain what they were before. Thanks to this theory-independence, observational statements, especially the so-called experimental (NAGEL) or empirical (CARNAP) laws, may survive the replacement by an alternative of a theoretical framework, in which they have been taken up originally.[8]

v. *Extensional logic is an adequate framework for scientific as well as epistemological and methodological investigation.*

The logical framework, laid down by propositional and (first-order) predicate logic has been used in nearly all investigations by philosophers such as CARNAP, HEMPEL, NAGEL, QUINE, GOODMAN and many others. This framework has become a very reliable ingredient of the method of philosophical inquiry. It is taken to be adequate for the analysis of concepts like 'confirmation', 'lawlikeness', 'theory', 'explanation', 'counterfactual', etc., and it is also used as an analytical tool with respect to the way of reasoning in daily life and science.

In my opinion, this confidence in the adequacy of extensional logic can be understood in the light of the four theses mentioned before: *extensional logic fits*, so to speak, *the ontological and epistemological facts*, briefly sketched in i to iv above. Observational terms as theory-independent units of extensional

* We shall meet with examples of this in ch. III.

meaning and observational statements as isolated units of empirical knowledge constitute a very good interpretation basis for first-order predicate calculus: the universe of discourse is formed by the isolated data, named by individual constants; groups of the same data (a class if you like) are named by predicates[9]; and the adequacy of the truth-functional composition of statements is guaranteed by the isolatedness of observational statements (they can only be bound extensionally or truth-functionally). This adequacy of extensional logic constitutes a part of the logical positivist philosophical point of view and I think it is an important part, because many contemporary problems in the philosophy of science are at least partially created by this 'faith in extensional logic'. But this point, like all others mentioned above, will become clear in the course of this study.

2. The post-positivist context

In section 1 we started with the notion of direct observation. Knowledge gained by such observation is taken as the starting-point of all other knowledge and as the end-point of confirmation[10] by the logical positivists. And observational terms are basic in this sense that they do not refer to any other terms or do not entail any meaning-connections; they only and immediately, i.e. not through the medium of any theory, refer to data.

In this section we shall start with the opposite notion of *theory-loaded observation*. Shapere has given the following characterisation of this view: "there is one by now familiar objection (against the theoretical-observational distinction, H.K.) (that) marks out a transition to a view of science which stands in radical opposition to that of the empiricist-positivist tradition and has, both by its own freshness and its own failures, helped to bring about the shift of emphasis in the problems of the philosophy of science. (. .) according to this criticism, not only is the *relevance* of observations at least partly dependent on theory; even *what counts* as an observation, and the *interpretation* or *meaning* of observational terms, is at least partly so dependent. All 'observation terms' in science are, in this view at least to some extent 'theory dependent' or 'theory-laden' in a sense which is passed over by the usual ways of making the distinction. Data are not 'raw'; there are no 'brute facts'."[11]

This view can be recognised in the work of many authors, but the notion of theory-loaded observation itself has not yet been analysed very thoroughly. One may say that a theory is a "conceptual network through which scientists view the world", like Kuhn[12]; or that "scientific theories are ways of looking at the world", like Feyerabend[13]; and one may agree with the early post-positivist Popper, that "there is no such thing as 'pure experience' (compare: 'direct observation', H.K.), but only experience interpreted in the light of expectations or theories which are 'transcendent'."[14] or with Hanson, when he says "Seeing is not only the having of a visual experience; it is also the way in which the visual experience is had" and "seeing is a 'theory-laden' undertaking"[15]; and at last we may refer to Mary Hesse, who gave the example of a

layman and a physicist in an atomic physics laboratory and concluded that "Each entity is observed in the ways appropriate to it"[16] where this appropriateness is determined by the theory 'through which' observation takes place (compare Kuhn's expression quoted above). But all these remarks only describe what one takes to be a fact, namely that observation is always theory-loaded, and a systematic explanation of this fact, if it is a fact, has never been given, except in one case[17], to which I shall come back in section 3. There I shall consider questions like "How is it possible that all observation is theory-loaded if one does not presuppose an innate theory, which can load observation from the beginning?" and "Is it not much more 'natural' to presuppose theory-free observation as the starting-point of empirical knowledge, followed by the formation of a theory, which can then load further observation?"

For the moment, however, we can use this somewhat vague notion of theory-loaded observation in its opposition to direct observation, and investigate what its consequences are for some of the points mentioned in the theses of section 1.

i. *Empirical data, and therefore empirical reality, are not observer-independent.*

If all observation is theory-loaded, executed from a certain point of view (POPPER), if there is always observation through a certain theory (KUHN, FEYERABEND) or if observation is the having of a certain experience *in a certain way* (HANSON), then *what* we observe, the empirical datum, is *co-constituted as that datum by the theory or the concepts through which we observe.* Thus, empirical data are, for the very fact of their being those data, essentially observer-dependent; they are those data only by being observed in the *appropriate way.* Consequently there cannot be as far as empirical science is concerned, any raw or pure or theory-independent data or brute facts, free from theoretical slants, but only data "analysed, modeled and manufactured according to some theory".[18]

A metaphysician may, perhaps, lay claim to a special faculty which enables him to distance himself sufficiently from reality so as to see it as it really is (although, in my opinion, he shall see nothing at all), but a scientist can only have knowledge of a world which is co-constituted by theory-loaded observation and consequently cannot speak of an observer-independent world. 'Nature' as we know it in daily life, and science, is co-constituted by our theories or conceptual systems through which observation becomes at all possible. And if RESCHER says (compare note 5) that it would take a bold act of rashness to deny that such an observer-independent Nature is regular in various respects, I would say that the assertion, as well as the denial of such a claim seems to be meaningless because it would presuppose the existence of a rather mysterious Nature.

This should, I think, be taken as the post-positivist alternative to thesis i of section 1.

ii. *There are no terms or statements with a fixed or theory-independent meaning.*

This is the post-positivist alternative to theses iii and iv of section 1. Observational terms with a privileged epistemological status do not exist, therefore there can be no isolated observational statements either: "The meaning of

every term we use depends upon the theoretical context in which it occurs. Words do not 'mean' something in isolation; they obtain their meaning by being part of a theoretical system".[19] The meaning of any term whatsoever, and thereby of any statement, is determined by the *meaning-connections* it has with other terms within a particular conceptual system. Of course the term 'meaning-connection' needs to be clarified, but it should be clear by now, that this theory-dependence of every empirical concept and every empirical statement, however simple it may seem, stands in direct opposition to the extensional approach to the problem of meaning within the precincts of logical positivism, which resulted in theses iii and iv of the previous section. If all empirical concepts have a meaning by their being connected with other terms, then there are no concepts which do not refer to other concepts. But then, there can be no observational terms either, because these terms must be characterised by the very fact that they do not refer to other concepts (compare note 10). Only terms which do not refer to other terms can be used as the end-points of confirmation and the starting-points of meaning. And when the possibility of such terms is denied, as it is by post-positivism, then a fresh answer is required for both the problem of meaning and the problem of confirmation (one should e.g. offer an analysis of the term 'meaning-connection', mentioned above). With respect to confirmation it is illustrative to compare Carnap's view with Mary Hesse's and Hanson's. CARNAP states that, if confirmation is to be feasible at all, we need theory-independent end-points (i.e. observational terms). MARY HESSE and HANSON on the contrary, state that, if confirmation is to be feasible at all, there must be meaning-connections between 'observational terms' and theories, so that these terms cannot be taken as isolated units.[20]

Enough has been said for the moment to formulate briefly the post-positivist alternative to theses iii and iv. It will be clear, I think, that this alternative has consequences for the concept of law, which is the aim of this study. The discussion about this concept mainly centers around the question what *experimental laws* (NAGEL) (or empirical laws; CARNAP) are. In other words, the problem of law is nearly always reduced to the question "What makes a universal conditional, which contains only observational terms, lawlike; or what is the criterion for distinguishing between accidental and lawlike universals about observables?".[21] And these experimental laws were taken to be isolated or theory-independent from a logical positivist point of view (compare thesis iv of section 1). They were taken to be directly, that is theory-independently, confirmable by their instances, while a theory, or a theoretical law in Carnap's words, "cannot be put to a direct test".[22] But as soon as observational terms are essentially theory-dependent one can no longer claim that there are isolated experimental laws and much of the discussion about these 'laws' becomes senseless within a post-positivist context. But we shall come back to this later.

I will not formulate a post-positivist alternative of theses ii and v at this stage, because the concept of theory-loaded observation itself should first be further elucidated (sections 3 to 6). From this analysis we may perhaps formulate an alternative view concerning 'connections in nature', which cannot be connec-

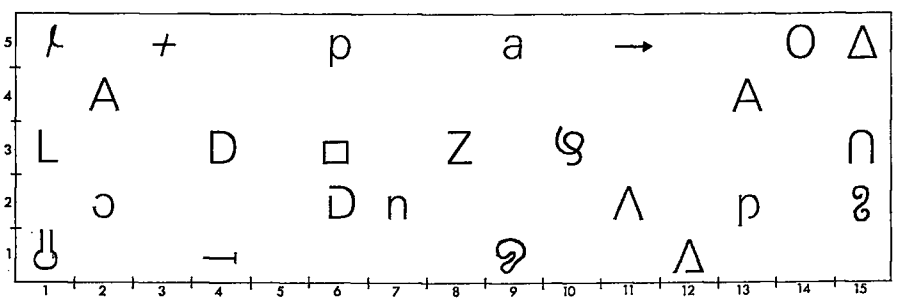
tions in an observer-independent reality, and we may then be able to review the adequacy of extensional logic.

3. Theory-loaded observation

My intention with the following example[23] is to stimulate the formation of some new empirical concepts, i.e. concepts not previously formed. This has the disadvantage of artificiality, but it has the advantage of elucidating what the essential impact of empirical concept formation is. Afterwards I hope to lessen the disadvantage of the example by a switch to 'normal' concepts.

Being obliged to explore the example in a written text some complications arise that do not normally arise in practise. I shall refer to these as 'example-complication' and I trust the reader will be able to imagine the factual process.

In the rectangle below I have given a number of objects:



The numbers only serve to refer to the objects in the rectangle: e.g. (5,15) is the object in the top right-hand corner. This method of referring to any particular object is an example-complication, as in practise I should merely point it out with my forefinger. This would mean that such a pointing-out could already be understood and, as is well known, one has to learn this. A baby looks e.g. at the tip of your forefinger when you point to a certain object. Likewise the reader is supposed to be able to observe the objects as separate objects.

If necessary, I can add more objects.

Now if I do something with some of these objects, I shall have to be explicit about this, while in practise no words would be needed (example-complication). The success of the example will depend on my description being followed step by step. The reader who only skims through what follows will fail to grasp the points I wish to stress.

First I put (5,14) in the first row below (I), second (1,4) in row II, then (3,4) in row I, (2,11) in row II, (5,9) in row I and (3,1) in row II. The result can be sketched as follows:

I could go on with my investigation of the objects within the rectangle and continue to put them in one of the two rows, on the basis of this inquiry, but that would not be very interesting. I may, however, ask someone else to go on. Could he? If he claims that this 'going on' would be purely a haphazard operation of arbitrarily distributing the objects between rows I and II, I shall take his answer to be in the negative. By admitting this, e.g. when he stipulates in which row a particular object belongs, he would be admitting that he does not *understand* what I did, because this was not a matter of mere arbitrary selection. *He cannot observe in the way I can observe and for him the empirical data, that I observe, do not even exist.* I must come back to this remark, but let us first continue the investigation, assuming that, if an answer is given, it would not be that the process is haphazard. Now where would one put (5,11)? In row I? No, that would be wrong, it belongs in row II, but it is an *understandable* or *intelligible* answer, it is not a gamble. From what I have done up till now one could perhaps have got the idea that I put the objects from the rectangle alternately in rows I and II: the first thing in row I, the second in row II, the third in row I again, and so on. This would at least render the above answer intelligible. *The trial*, briefly expressed by the concept 'alternately' *was, however, an error* and this became clear as soon as I explained that (5,11) should be put in the second row. *Observation had not yet occurred in the right way*, although something had definitely been observed (the trial made this possible!) and the answer based upon this observation, but *without any understanding of what I had done.*

If someone answered my question of where to put (5,11) with: "In row II" that would be correct, but whether he observed in the right way, or not, and whether he already understood what I did, or not, remains to be examined.

We would now have:

Where shall we put (4,2)? In row I? Yes, that is correct, but the answer "In row II" would again have been intelligible. For one might have reasoned that all objects built up of straight lines belong in the second row, while all objects which are also constituted by bent lines belong in row II. Up till now this conjecture or trial has always been confirmed, but now, with (4,2), we have met

a trial-falsifying instance and the trial is again unsuccessful. According to this trial, (3,15) would also have been a falsifying instance, for it belongs in row II, while one would have put it in row I on the basis of this trial. On the other hand, (3,10) would have confirmed it, because it belongs in row I, where it also belongs according to this trial. In the same way (5,6), which belongs in row I, would also have been a confirming instance. And again we must conclude that anyone who observed the objects within the rectangle *through this conjecture*, i.e. who observed those objects *as* being bent or not-bent, had not yet observed from the right point of view (POPPER) or in the right way (HANSON). And he who directly gave the correct answer may have observed in the right way, but might also be in error, even if he has put all the objects, referred to up till now, in the right row.

In the meantime we have now got:

row I	O D a A ϑ p
row II	⊣ Λ L → ∩

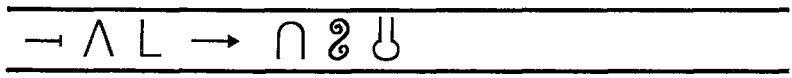
Where shall we put (5,15)? I expect that many readers will answer: "In row II". This answer is completely understandable and I trust everybody sees why. It is very simple: in row I all letters within the rectangle we have met with so far are taken up and in row II all (logical and geometrical) symbols. Now (5,15) being the geometrical symbol for a triangle, should be put in row II. This trial or conjecture has been confirmed by all instances investigated up till now. But in saying this an interesting point is raised. If we observe the objects in the rectangle through the conjecture 'all letters in I – all symbols in II' and say that this has always been confirmed up till now, then apparently (3,1) has been observed not as the 12th letter of the alphabet, but as the geometrical symbol for 'angle' (angle A of triangle ABC is often symbolised by ∟A, just as triangle ABC is symbolised by △ ABC, using (5,15)). Object (3,1) might at first sight have appeared to be an exception to the conjectured rule, because it could have been observed as the 12th letter of the alphabet. But it is not a trial-falsifying instance after all. It has become an understandable or explainable exception, which turns out to be a nice confirmation: (3,1) is not the 12th letter, but a geometrical symbol and therefore belongs in row II. This clearly illustrates, I think, that *what* we observe, the empirical datum, is co-constituted by *how* we observe; in the given example (3,1) could be observed through the concept 'letter' or through the concept 'symbol' and in both cases we observe a different empirical datum.

But let us now return to (5,15). I must disappoint those who believed it to belong in row II, as it belongs in row I, although it is not a letter of the alphabet and although it is a geometrical symbol. And (1,9) which is neither a letter nor a formal symbol, also belongs to row I. We cannot give an explanation of these

facts leaving the trial 'all letters in I–all symbols in II' intact, as we could in the case of (3,1). Objects (5,15) and (1,9) are not explainable exceptions, but falsifications of this trial, which must now be accepted as an error, and the observer should try again. He has not yet observed in the right way, he does not yet know the data I know.

Let me add some extra information: (2,15) and (1,1) belong in row II and (3,6) and (4,13) belong in row I. Then we have got:

row I 

row II 

Can the observer go on now? (5,3)? In row II? Yes, that's correct. And (3,8) in row II, yes. (2,6) in row I? No, have another look, it belongs in row II, as does (1,12) and (2,13). And (2,2) must indeed be put in row II.

At this stage, most students attending my lectures, where I actually went through this process, succeeded in understanding me. This involves their being able to continue my investigation as I would have done it. They went through many different conjectures and at last, some sooner, some later, 'saw' it or had an 'Aha Erlebniss', if you like. At a certain moment they understood what I was doing and how I was doing it. They could, from that moment on, observe what I already could observe and they gained knowledge of empirical data which they had not even known to exist.

Now I can only hope that the reader also had such an 'Aha Erlebniss' on the basis of the evidence just described. I shall assume for the moment that the reader has been successful and shall say that *he has formed concepts.*

Now, once the concepts have been formed, we can give names to them. The giving of a name to a concept is, as such, very important for communication, but which name, i.e. which letter-sequence or word we shall use as a name for a certain concept is, at least in our culture, a highly conventional matter. Let us use the letter-sequences desolc and nepo, or the words desolc and nepo, as names for the concepts just formed: the objects in row I are desolc, those in row II are nepo. I think, that letter-sequences become words just by acquiring this function of naming a concept; otherwise such a sequence remains a meaningless string of letters.

Before going on I must be sure (and perhaps you also want to be sure), that you have indeed formed the concepts 'desolc' and 'nepo'. Therefore I am obliged to make use of an artifice which is not essential to what follows (example-complication). This artifice consists in the betrayal of a secret, which is possible in this example and which is essentially dependent upon the fact that you have already formed many other concepts to which I can appeal. The secret is simply this: if an object in the rectangle completely encloses a certain area it is put in row I (desolc is the reverse of closed; the name is not purely conventional after

all) and if an object has an open area it is put in row II (nepo – open). But again, the possibility of betraying the secret, present in this example, should not be taken to be a conditio sine qua non for concept formation. On the contrary, at the most fundamental level of concept formation we have no possibility of betraying a secret. In the case of the colour concepts, for example, we cannot say what red things 'have in common', what the characteristic of green things is, etc. In such a case the question is simply whether one sees it or not or whether one has formed these concepts or not. Thus, the betrayal of the secret in our example should not be taken as essential: with enough patience, nearly everybody would, sooner or later, be able to form these concepts in the way briefly sketched. I shall come back to the 'nearly' mentioned in the last sentence in section 5.

Let us now review this process. A certain activity was deployed therein, one tried and erred and at last understood, one gained certain knowledge in the form of the concepts 'desolc' and 'nepo'. This activity is, I think, a very important kind of empirical inquiry. As a matter of fact, I do not think it is normal science in Kuhn's sense[24], but it need not be revolutionary science either. For there need not be a rival or conflicting or paradigmatic concept of the concept which is formed in such an investigation. We could call it evolutionary science, but perhaps these remarks are rather premature here and I shall have the opportunity to come back to this point later. The point I wish to stress here is that a certain activity was deployed which I have called an important kind of empirical inquiry.

Before one started this investigation, before one formed the concepts 'nepo' and 'desolc', one did not know anything about nepo and desolc things. One could not observe them and one did not even know of the existence of empirical data such as '(5,15) is desolc'. And now, afterwards, one can observe such things and have knowledge of such data as the one mentioned. *Concept formation means precisely this learning to observe in the appropriate way.* And therefore: *the empirical concept formed entails a method of observation.*

Having a concept is being able to do something, namely to observe in a certain directed way or from a certain point of view. Once one has learned to observe as one should, one has formed concepts. This 'as one should', this appropriateness, constitutes the meaning of the concepts, its intention if you like, and in the following sections of this chapter I shall attempt to analyse this further. I intend to show that the concept is itself a minute theory, complete with lawlike connections, however rudimentary one may wish to call it. The empirical concept, as a minute theory, lays down the appropriateness of a method of observation. This is the primary ingredient of the meaning of an empirical concept. Once one has formed the concepts or minute theories 'nepo' and 'desolc', one can practise normal science with them. One can now investigate the 11th letter of p. 36 of Kuhn's 'The structure of scientific revolutions' and establish whether it is nepo or desolc by observing it through the concepts 'nepo' and 'desolc'. This was not possible before, one was blind with one's eyes open, one saw but did not observe anything of this kind. Of course, one observed letters and other

symbols, but this only stresses the point: one could observe these things just because one had a method of observation laid down in the concepts 'letter' and 'symbol'. And this observation in normal science should now be characterized as *theory-loaded observation* in this sense, that *we observe through the minute theories or concepts* 'nepo' and 'desolc'. In this very elementary sense all observation is theory-loaded; all observation is observation from a certain point of view (POPPER) or through a conceptual scheme (KUHN). It is always the having of a certain experience in a certain way and not simply an exposure of the senses to the world (HANSON). And it is not only the having of a visual experience but also the way in which it is had (HANSON again). Without such a theory-loadedness there can be no observation and it is precisely this theory, that makes observation in a certain way possible, which we come to know in the trial and error investigation called concept formation. And then, if all observation is theory-loaded, the empirical data we become able to observe, cannot be called 'raw data' or 'brute facts.' *Empirical data are always conceptually or theoretically co-constituted just because they are those data by being observed theory-loadedly.* The empirical datum of science is essentially theory-dependent or concept-dependent for its being that datum. Theory-free or directly observable data are not the things we have knowledge of in empirical science.

Before finishing this section we must reconsider the question how it is possible that *all* observation is theory-loaded: must not the process of concept formation have a theory-free starting point, so that, basically, logical positivism is right in this respect? Must not we suppose that there once was a theory-free observer facing theory-independent data? Is not every little baby an example or a confirming instance of this supposition? Does not every human being start as a theory-free observer meeting with naked data, forming concepts as isolated units of extensional meaning, which then may load his further observation? It is precisely this problem of *where to find a fixed and stable, i.e. theory-independent, starting point* of empirical knowledge which, according to MARY HESSE, may explain why the distinction between an observational and a theoretical language lives such a long life[25], because this distinction provides an answer to this problem by providing a theory-independent observational basis. I think we can give a much more satisfactory answer by now.

A human being does not start as a theory-free observer and a baby is not a theory-free observer facing raw or theory-independent data or brute facts. Not because of the fact that a human being is born with some theory in mind, but simply because he does not start as an observer at all. A baby is not an observer at birth, just as it is not a biped at birth. It must *learn to observe* and as a rule, but not always, it has the ability to learn to observe in a directed or theory-loaded way (if I had omitted the last six words of this sentence, I would have said the same thing; for clarity I have added them). It has, in other words, the ability to form empirical concepts or the ability to learn how to load its observation appropriately. A baby can learn to handle things, to manipulate them, to move them and to play with them as we, at least, often call it. In such a process it forms the very wide and unspecific concept 'thing' and *at the same*

time it constitutes things as empirical data. *Concept formation, or the learning how to observe appropriately, and datum-constitution are closely interrelated: they are brought about at the same time in one and the same process.* In growing up, the baby learns to handle and thus to observe things in many other ways, forming new concepts and at the same time constituting new empirical data by trial and error. Once we have recognised the fact that a human being must learn to observe in order to be an observer at all, we must also recognise all observation to be theory-loaded, because this 'learning to observe' means learning to observe theory-loadedly or the formation of the concepts through which observation becomes possible. And the problem of the starting-point has then disappeared: we do not start theory-free and we do not form our first concepts by theory-free observations. We learn to observe by trial and error and this is the same as the formation of empirical concepts as minute theories in a sense to be explained in the following three sections.

I doubt whether the proponents of theory-loaded observation had this radical alternative to direct observation in mind. Many of their remarks suggest that there still are two stages in observation, a 'pure stage' followed by an interpretive stage, but this, I think, would be pointless, because it would reintroduce theory-free observation through the back-door. The difficulty here is one of formulation as is also clear from what I have said in this section. I said e.g. that concept formation is the learning how to load one's observation appropriately. This also suggests that there is some measure of theory-free observation, which must be loaded with theory, but this is not what is meant at all: the expression 'theory-loaded observation' is a pleonasm. We can only separate the concept as an isolated unit from the theory it implies, if we sacrifice the concept. Such a separation destroys the concept, it simply vanishes and we deprive ourselves of a method of observation, which might have been important. The same holds for the data: we can only separate the datum as an independent or self-supporting datum from the concept or theory through which we can observe it, if we destroy it.[26] This does not imply, that e.g. atoms did not exist before our modern concept 'atom', which provides us with a very complicated method of observing atoms (which, among many other things, means that we cannot meet them in the street and that we cannot observe them in the way we can observe the telephone on my desk) was formed. Of course atoms existed before modern physics, but this only means, that if human beings in 1500 A.D. had known our modern theory and had formed the concepts in which this theory is focussed, then they would have observed atoms. They could e.g. have observed sodium and chlorine ions, arranged in the cubic crystal structure of NaCl, in an X-ray analysis. But then the datum 'that thing there is a sodium atom' is a theoretically or conceptually co-constituted datum, as it is for a modern chemist.

In what sense is our answer to the starting-point question more satisfactory than the logical positivist answer? In section 1 under thesis iii I stated that, presupposing theory-free observation, it is impossible to explain how 'the same data are given the same name' or how certain data are brought together in the same group or class of objects. If one does not know the concept 'red', then

how can it ever be explained, presupposing theory-free observation, that one, at a certain moment, succeeds in bringing together in one group such different things as a ripe tomato, a bottle of blood and that chair over there? One might answer that in all these cases we have the same sensory impression, but how do we know this? How can we judge impressions as being 'the same' if we have not yet formed the concept 'red'? Once we have formed this concept it is easy: we observe that tomato, that bottle of blood and that chair through that concept or minute theory so that we can know an order in a multitude of impressions. But we can only answer the starting-point question if we say that the formation of a concept is primarily the learning to observe through that concept. Presupposing theory-free observation, however, an answer like this is prohibited.[27]

One final remark should be made in connection with this point. POPPER also considered the question of the starting-point, while not presupposing theory-free observation. In his view all observation is theory-loaded and therefore theory must precede observation; but this theory is in turn preceded by other observations, which "in their turn, presupposed the adoption of a frame of reference: a frame of expectations: a frame of theories. (. .) There is no danger of an infinite regress. Going back to more and more primitive theories and myths we shall in the end find unconscious, *inborn* expectations".[28] Of course this is also an answer to the starting-point question and apparently we must not take too seriously his remark that "The question how it happens that a new idea occurs to a man (. .) may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge".[29] I do not like this answer, not because it implies a psychological statement (I think my answer also does and one should welcome such a scientific risk in epistemology), but because it is an answer to a wrong question. The question "Is not there a danger of an infinite regress?" is wrong because it presupposes that theory *precedes* observation, which implies a separation that I warned against earlier. The way in which one observes and the theory or concept which loads one's observation come to be known at the same time, in one and the same inquiry, namely concept formation.

4. THE EMPIRICAL CONCEPT AS A MINUTE THEORY

In this section I shall not refer only to the nepo-desolc example of section 3, but mainly to the colour concepts. Without this switch which, in my opinion, will justify itself in due course, I should be obliged to develop the nepo-desolc case artificially further and further while the colour concepts have, as a matter of fact, gone through just such a development and it will, therefore, be easier to use these as our example.

The formation of 'red', 'green', 'blue', etc., is also primarily a case of learning to observe in a certain way, appropriate to certain data: all data are observed in the ways appropriate to them (MARY HESSE) and this also holds for red, green, blue, etc., things; otherwise, we may add, there would not even be such

empirical data as 'this thing here is blue'. It is this appropriateness, this 'certain way' or this 'how' of our observation, which makes observation possible. It directs or loads our observation, which cannot thus be imagined as being theory-free. In this section, as well as in the next two, I shall try to analyse this 'how' and this analysis should make it clear in what sense an empirical concept is a minute theory. In other words, this analysis should explain what we mean by the meaning or intention of an empirical concept.

As in the case of 'nepo' and 'desolc' the formation of the colour concepts has been successful only if one discovers, at last, after many trials and errors and some sooner, some later, *the trial that holds*, the way in which we should indeed observe, the 'how' of observation. We should e.g. not observe things as bigger or smaller, as square or round, as beautiful or ugly, as nepo or desolc, etc. etc., until at last one grasps the correct trial and this cannot be further explained as in the case of 'nepo' and 'desolc'; the secret cannot be betrayed. One grasps the correct trial at a certain moment or not; one has the mentioned 'Aha Erlebniss' or not; one succeeds at a given moment to make the required 'creative jump' or not. In the next section we shall investigate the problems connected with this inexplicability, such as the role of the teacher, the possibility of different people having the same concepts and the question whether everybody is able, in principle, to make the same creative jump or not. Now this inexplicable 'how' of observation, that trial that holds, is the primary meaning-ingredient of the concepts which are formed: 'red', 'green', 'blue', etc., all mean the same in this respect. In what sense they are different concepts will be explained later. We said, we could not explain this 'how' any further, but we can give it a name, i.e. colour. This 'how', shared by 'red', 'green', 'blue', etc. constitutes the meaning of the concept 'colour' and we may now answer the question "How do we learn to observe when we form the concepts 'red', 'green', 'blue', etc.?" with "We learn to observe things as coloured things or, *what boils down to the same*, we learn to constitute certain data, namely coloured things, by observing appropriately". But notice, that this is only a naming of the 'how' and not its explication. In the case of 'nepo' and 'desolc' we could have named the how of observation, which is present in both 'nepo' and 'desolc', e.g. by using the letter sequence f o l o r, which becomes the word folor just by acquiring this function of naming the 'how' of observation. But in this case we could betray the secret, as we did.

Let me repeat the warning of section 3 here: we should by now realise the error of saying that there are folored and coloured things in an observer-independent reality, which come to be known as soon as we appropriately observe them, i.e. through the concepts 'folor' and 'colour' respectively; we cannot say this, precisely because these data are only those data within that appropriate method of observation by which they are co-constituted; the 'how' of our observation and the data for which this 'how' is appropriate are brought about in one and the same process of concept formation. I will now first say something more about such concepts as 'folor' and 'colour' and then I will come back to 'red', 'green', etc.

A concept like 'colour' comes to be known by trial and error: one must try *and* err in order to grasp at last the correct trial; one also learns how to observe in 'incorrect' ways. In the nepo-desolc case the observer tried to understand how I observed in terms of a putting of the objects from the rectangle alternately into two groups, he tried to understand it as an observation of bent and straight things or as a putting of linguistic symbols in one row and of formal symbols in the other. In all these cases he failed because he had not yet observed appropriately, he had not yet observed folored things, which did not yet exist for him. But it is very important to err in such ways, because *it provides us with a first kind of order in a chaotic multitude of possible ways of observation.* The concept 'folor' comes to be known as *distinct from* the concepts 'form' or 'symbol' or 'size' etc., and analogously the concept 'colour' comes to be known as distinct from 'size', 'folor', 'taste', 'form', 'smell', etc. In this way these concepts, as methods of observation, are ordered amongst a great many other concepts or methods of observation. They get a position in the conceptual scheme, part of which may be present beforehand, but part of which may also be formed in the same process of concept formation. The order in the conceptual system is brought about by the relation 'distinct from'. Of course, being routineers in concept formation, and most ten-year old people are, these relations are brought about rather automatically and they may therefore seem trivial, but they are not unimportant, because they constitute part of our methods of observation: learning how not to observe contributes to learning to observe from the right point of view. Once we have formed the concept 'colour' and thereby the colour concepts (see below), we know that in order to establish the colour of a thing, e.g. to test 'that thing over there is red', it is not necessary to observe its form, its size, its weight, its being ugly or not, etc. And this knowledge is stored, so to speak, in the concept 'colour' and rests upon the relations 'distinct from'. That it is trivial knowledge, if it is, does not imply that it is epistemologically unimportant to explain where it comes from.

Observation of coloured things, then, is observation loaded with, or through, the concept 'colour'. This concept directs our observation (there would not even be observation without a direction!) and this direction is partially determined by the relations, 'distinct from', that this concept has with a great many other concepts. The concept 'colour' thereby enables us to bring about an order in a multitude of sensory impressions, which implies the constitution of the class of coloured things. The extension of a concept is, in this view, indissolubly bound to its intention. The concept as a method of observation or as a point of view, to borrow Popper's term[30], enables us to speak of its extension. 'Colour' is a minute theory in that it focusses these 'distinct from' relations by which it is taken up into the conceptual scheme. It is a minute theory in that it focusses this conceptual scheme and it has the meaning it has by being taken up in this scheme, which is not only built up of such 'distinct from' relations as we shall see.

We must now return to the fact that, in learning how to observe, we form different concepts (e.g. 'red', 'green', 'blue', etc.). This is a rather simple point:

once the 'how' (e.g. 'colour') of an observation is understood or grasped, other concepts (e.g. 'red', 'green', 'blue', etc.) have more or less also been formed, because that 'how' or that method of observation is learned or discovered *in an ordering of things*, which could not be ordered in that way before: *the learning to observe in a certain directed way comes down to the same as the learning to order things in a certain specific way.* If we learn to observe coloured things, we learn to order them according to their colour.[31] But here we must also take care not to separate what cannot be separated: there is not first the method of observation and then, as a consequence, the order. No, we learn how to observe in learning how to order and vice versa. These are two aspects of one and the same process of concept formation and we cannot understand the one without the other. The ordering should be understood as a special kind of manipulation of things, to which we referred before in connection with the baby's formation of the concept 'thing' (section 3).

Of course not all concept formation can be said to be a matter of learning to order in as simple a manner as in the cases of the colour and folor concepts (these are at least simple in our very 'theoretically loaded eyes'), but we are still at the very elementary starting-point level. Later on we shall see (first in section 5), that the formation of 'more advanced' concepts takes place in a different way. Also in those cases we may say, I think, that a concept is primarily a method of observation. But returning to the colour concepts, we may say that the 'how' of observation, laid down in the concept 'colour', is at the same time the 'how' of the order which is brought about. And if someone would ask what this 'how' is, I could only answer "Colour". It cannot be further explicated, it can only be named, as I said before. And via the concept 'colour', being part of the meaning of 'red', 'green', 'blue', etc. these concepts are taken up in the conceptual scheme. But they have another *meaning-ingredient, constituted by their having a specific place in an order*, a place which can be filled by the appropriate data[32] and this aspect of their meaning distinguishes the colour concepts from each other.

Let us now consider in a little more detail what it means to say that, e.g. 'red', is a minute theory. First, of course, 'red' contains the 'how' of the observation or ordening, by which it gets a position within the conceptual scheme via 'colour'. And part of the meaning of 'red' may be formulated by 'red is a colour' or by 'if a thing is red then it is coloured'. Both these statements formulate the same *necessary connection* between 'red' and 'colour' and *thus* also between the empirical data known by means of, observable through or co-constituted by, these concepts, e.g. 'this is a red thing' and 'this is a coloured thing'. But notice that we do not mean a necessary connection between facts in an observer-independent reality, nor a necessary connection between linguistic entities, which has no empirical foundation at all. It is impossible to apply the positivist analytical tool of the factual-linguistic dichotomy in such cases (compare Quine's criticism in his 'Two Dogma's', to which I shall come back later). It is much more as Hanson stated: once a concept or minute theory has been formed "scientists will cease distinguishing between its (i.e. the concept's, H.K) structure and that of the phenomena themselves."[33] But this 'fusion' can only be

justified within an epistemology, marked by theory-loaded observation. Within such a context one may speak of necessary connections in a theory-dependent world (compare thesis ii of the logical positivist context, sketched in section 1). Connections such as the one between 'red' and 'colour' are necessary in a very simple sense: part of the meaning of 'red' is 'colour', which contains the 'how' of observing coloured things. The negation of the statement 'red is a colour' would come down to the same as "I don't know what 'red' and 'colour' mean". Such a necessary connection is *empirically founded as a necessary connection*, but not in the inductivist sense of a constantly going together of a thing's being red and its being coloured. It is empirically founded in the following sense: in concept formation the 'how' of observation is, finally, discovered. This 'how' or this trial that stands the test is discovered in empirical inquiry, it is not known a priori, and then, only afterwards, we can recognise in such empirically confirmed trials such necessary connections as the one between 'red' and 'colour' and between 'folor' and e.g. 'nepo'. But the necessity of such a connection does not imply an 'irrevisability' or a 'true, come what may' (Quine). On the contrary, what may come may indeed force us to break such connections open, which would then imply the destruction of our colour concepts and we would be deprived of a possible method of observation. This stresses the empirical character of such necessary connections. There is no guarantee whatsoever that the trial we at last discover in concept formation will continue to stand the test to all eternity. Thus, if we speak of necessity, this is always a necessity relative to our conceptual scheme.[34]

And how else could it be: concepts are always concepts within such a scheme and data are always theoretically or conceptually co-constituted data. Thus, if we speak of a necessary connection between concepts or data, it can only exist relative to a conceptual scheme. But this possibility of its being refuted emphasises the fact that the necessary connection as such is empirically founded. And the question whether we can imagine such future evidence, which would force us to break such a connection open and therefore to reject our method of observation or, in other words, which would lead us into a conceptual crisis, is very interesting because of the answer we must give to it: we cannot imagine such data because of the very fact that they are excluded by our conceptual scheme or our theories. If we could understand such data we would have a concept through which we could observe them, at least in principle, and in that case the necessary connection would already have been broken open. We can imagine or understand a red thing to be square or round only because, with the concept 'red', we know that the form of a thing is irrelevant to its being red. This goes back to the relation 'distinct from' between 'colour' and 'form' and thereby between 'red' and a certain form. Our ability to understand this rests upon the fact that it constitutes a possibility within our conceptual scheme. Analogously, our not being able to understand a red thing not to be coloured as well, rests upon the fact that it is impossible within our conceptual scheme, that has been formed in empirical inquiry, namely concept formation. But we shall meet with this point, namely the possibility of refutation, several times again and I shall then go

further into the matter. We are, after all, able to make such conceptual revisions; concepts like 'particle', 'energy', 'simultaneity', 'mass', etc., from physics, and concepts like 'individual', 'state', 'social change', etc., from social science[35] have been revised precisely because of the necessary connections they contained within the 'old' conceptual schemes or theories.

But the concept 'red' still contains other connections as a part of its meaning, it is a still more extensive minute theory. By having a place in an order, 'red', is connected with the other places in the order: it has *that* place by its not occupying the other places. We may formulate this simply by 'red differs from green, blue, etc.' or by 'if a thing is red then it is not green, not blue, etc.',

Again these connections are necessary in the same sense as the one discussed before: they are not 'necessities in nature', nor are they purely linguistic truths, but they are relative to the conceptual scheme in which they acquire meaning and they may be falsified, which would cause a conceptual crisis. That red differs from green, blue, etc., is necessary for the formation or the having of those concepts. Otherwise there would be no order nor a 'how' of observation: there would be no colour concepts at all. Of course we can only say this in retrospect; we can only recognise these necessary connections with retrospective effect. And if we do form these concepts then the necessary connections are there. They are established in the trial and error investigation which is called concept formation.

It can now be seen more clearly that concepts, even if they would be called observational predicates by a positivist, are not isolated units of meaning. Concepts are only concepts in relation to many other concepts; among them are the 'distinct from' relations and the necessary connections. By focussing these relations an empirical concept is a minute theory. It may also be more clear by now that an empirical datum is neither observer-independent nor independent of other data for its being that datum. An empirical datum is not an isolated and raw fact. The empirical datum 'this is blue' is only that datum thanks to the existence of many other data in respect of which it can be ordered and thereby constituted as that datum.

There are other theory-ingredients of an empirical concept like 'red', that are more important than the ones mentioned in this section, especially in connection with the analysis of the concept of empirical law, which is the aim of this study. These ingredients will be called the *lawlike traits* of a concept or its *lawlike character*. They will be investigated in the next two sections. POPPER[36] has frequently referred to the lawlikeness of a concept, but the idea has never been extensively elucidated.

5. THE LAWLIKENESS OF AN EMPIRICAL CONCEPT – I

Up till now we have tried to elucidate some implications of concept formation (sections 3 and 4), but we relegated certain points to this section: from section 3 the question that *nearly* everybody can form certain empirical concepts in

the way sketched by means of the nepo-desolc example and the point of the 'Aha Erlebniss' or 'creative jump'; from section 4 the inexplicability of 'the trial that stands the test', which is suddenly discovered by making that creative jump, the role of the teacher in concept formation and the problem of how we can ever say that different people have the same concepts.

These problems are all closely interrelated and we cannot try to answer them separately.

Let us start with the following question: "How can a teacher know that his pupil has, as a matter of fact, formed the colour concepts at a certain moment?" In other words: "How can he know that his pupil has grasped the correct trial?" He can have no recourse to a 'betrayal of the secret' as I could in the nepo-desolc case. Of the second letter of this page we can say that it is nepo because it has an open surface, but we can only say that it is black because it is black. What could be a basis for the teacher's judgement that his pupil now has succeeded in the formation of the colour concepts? Of course, we are here assuming the infallibility of the teacher or, better, the presence of a universally accepted authority, by which we do not mean a person, but rather an objective rule. But we shall come back to this. There is a twofold basis I think. First, there is the fact that, from a certain moment on, the pupil 'always' correctly describes the colour of the things he is asked to describe. This may be taken as confirmatory evidence for the teacher's judgement. And this constitutes 'confirmation by positive instances' for the trial and thus also for the concepts in question, in the eyes of the pupil (remember the nepo-desolc case). Second, the teacher may ask 'crucial questions', e.g. "Can a thing be red and blue at the same time?" A few months ago I asked my four-year old son for the colour of the tomato he was eating. He promptly answered "Red" (a confirmation of the first kind). Then I said that it was also blue and he rejoined with "Oh no, that is impossible, if something is red it cannot also be blue, can it?" (a confirmation of the second kind). The teacher could e.g. also ask "Must a thing be red and square at the same time?", checking whether or not his pupil has gained knowledge of the relevant 'distinct from' relations (compare section 4). As a matter of fact, about a year ago I said to my son (the same as the one mentioned before) that the table he was sitting at was white. "No", he said, "it is round". The answers to such questions or remarks may indicate whether the pupil has formed the minute theory stored in the colour concepts or not. On such a twofold basis the teacher may soon be pretty sure that his pupil has really made the required jump, i.e. that he really can observe appropriately now. And he may reason that as long as his pupil displays exceptions or as long as he gives the wrong answers to 'crucial questions', he does not yet observe in the right way or that he has not yet formed the colour concepts.

If this were all, the situation would be rather uncomplicated, in my view. Of course, we should never know for certain whether a person really had formed certain concepts, because there is always the possibility that he grasped a trial that parallelled the correct one in all cases up till now (compare the 'letter vs. formal symbol-trial' that parallelled the correct one, i.e. 'folor', for a great

many things; section 3). But most of us would be satisfied with the practical certainty we may get on such a twofold basis, perhaps fearing getting entangled in metaphysical speculation about 'obscure intermediary entities' which should be present in a person's mental equipment when he has grasped the correct trial. But the mentioned uncomplicatedness would be marked in particular by the fact that we always have *an answer to exceptions*: even when a person who has up till now always called nepo things nepo and desolc things desolc, red things red, brown things brown, etc. we should know in advance the explanation of his behaving irregularly, e.g. by suddenly calling a red thing green. This explanation would simply be, that he has not yet observed appropriately or that he has not yet grasped the correct trial or that he has not yet formed the concepts in question.

However, what has been said is not all. If it were, the minute theory laid down in, e.g. the colour concepts, would constitute *a barren bit of knowledge because it would not leave room for genuinely new exceptions, for which an explanation could not (yet) be given*; it would be a minute theory in the sense of section 4 and that would be that. Fortunately we can say much more and this will make it clear that even such simple concepts as the colour concepts are very *fertile* or have an important *heuristic value.*

Let me first say that I do not have in mind a further analysis of the 'creative jump'. We have now reached a point at which I can agree with POPPER, who relegated such an analysis to empirical psychology.[37] What can be said in addition comes down to this: not the regularity in the behaviour of a person who apparently has formed certain concepts nor the irregularities explicable in the way just mentioned, but *the exceptions in the behaviour of a person, who should, normally, have formed those concepts are of primary importance.* With this I allude to the point that nearly everybody can form certain simple concepts in the way mentioned in section 3, be it the one sooner the other later. And of the fact that not everybody can form those concepts, I say, that it is of far greater importance, epistemologically, than the fact that the vast majority of people can form them. That it is a fact and how it can be a fact, will be shown in a moment. It is this fact, that both makes the matter more complicated and makes our minute theories fertile.

I have stressed the fact of there being such exceptions, because *exceptions have received a stepmotherly treatment in epistemology and methodology*, even in Popper's philosophy, I think. 'Regularity without exceptions' (compare the quotations in note 7) constitutes the fundamental notion of lawlikeness, but the function of exceptions will appear to be of the utmost importance in the establishing of empirical laws, as it is in empirical concept formation.

Let us come to the point now. Thus far our analysis of concept formation was related to the teacher-pupil situation. We must now imagine a different situation. Suppose that a long time ago, when no physical theory of light or a physiological theory of sense organs was at hand, two gentleman, Mr. A and Mr. B, who grew up in the same 'cultural circle', walked in a garden on a sunny day, both eating a ripe tomato. Mr. A, not knowing what else to say, remarks:

"This is a delicious red tomato". He does not expect any reaction, but Mr. B appears to be surprised and asks: "You called this tomato red?" "Well, yes, look here" and A shows him his tomato. "But it is green, can't you see?" Mr. B answers.

In brief: Mr. A says of the tomato that it is red and Mr. B says that it is green.

Suppose further that this difference is not caused by the fact that Mr. B has given a different name, i. c. 'green', to the concept which Mr. A named 'red', and vice versa. In other words suppose, that the difference is not caused by the fact, that A and B gave different names (in the sense mentioned before in connection with the nepo-desolc example) to the same concept. In that case they could easily reach agreement by conventionally adjusting their naming.

Suppose, finally, that the disagreement is also not caused by the fact, that A observed the tomato through the colour concepts (or: as a coloured thing) and B through shape concepts (or: as a thing with a certain shape), or vice versa. In other words, it is not the case that what B describes as 'green' would be described by A as e.g. 'bulgy' and neither is it the case that what A described as 'red' would be described by B as 'cubic'. If, upon further investigation, such a cause of the trouble would become manifest, the difference could also be removed by conventional adjustment.

Both A and B pretend to describe the colour of the tomato in question. And it may be that they have always, up till the time of their conversation in the garden, given the same description when describing the colour of things. And, as a matter of fact, they give the same description of the colour of the grass they are walking on. But now Mr. A is really surprised and says: "If you say that this tomato and the grass over here is green, then you do not observe a difference in colour in these two cases. That is very strange and I cannot understand it". "But it is completely clear to me and I wonder why you use two different names for the same concept. That is rather confusing".

Both are honourable men and they are not satisfied with this situation of disagreement. They could, for example, have reacted with resignation: "Well, we ought to be tolerant, so for A this tomato is red and for me it is green; full stop". Why do they not reason this way? Because both took it for granted that they had made the 'correct creative jump' or that they had grasped 'the trial that stands the test' or that they could observe in the appropriate way in order to observe coloured things. They both thought that they understood the minute colour theory. This understanding presupposed intersubjective agreement, because they took it to be understanding by the very fact that it enabled them to communicate with other people. If they should reason in the way just mentioned, with resignation and with an appeal to pseudo-tolerance, they would, in principle, give up their concepts as a means of intersubjective understanding. Mutual understanding would be an inexplicable miracle and there would be no room left for discussion about the truth of their statements. But if they want to maintain understanding, they *must* go on with their discussion and investigation in order *to reach better understanding.*

First Mr. A might suggest that B has not yet grasped the correct trial or that he has not yet formed the colour concepts, but Mr. B cannot accept this. There is no reason at all why B should be the pupil and A the teacher. Why should it not be the other way round? The situation is completely symmetrical in this respect. The result of such a move would again be a mutual non-understanding. Moreover, it would be a completely ad hoc escape, because it can, a priori, be said to be applicable in all future cases of disagreement. This holds, of course, for both sides and the best they could say would be that both had a piece of private knowledge, relinquishing any intersubjective character of their minute theories. Second, Mr. A cannot simply accept Mr. B's description of the tomato as a *falsification of his colour concepts.* For it would not at all be clear why his own observation should not be taken as a falsification of B's concepts instead, and it would therefore again be a completely ad hoc manoeuvre, the situation again being completely symmetrical. Moreover, Mr. A and B could not even consider this possibility seriously. If e.g. Mr. A really took B's description as a falsification of a part of his colour theory, this would imply that he admitted that he can no longer observe in a way, laid down in the concepts 'red' and 'green', that he could do until a moment ago. But if one has learned to do something, i.c. to observe in a certain way, one cannot suddenly decree that one cannot do it any longer. Such a step would not solve any problem and no better understanding would be gained, neither by A nor by B.

The most important reason for these failures can be found in the *static approach* to the matter. I will make this clear by continuing the story. Mr. A does not take B's description as a falsification of his colour concepts, nor does he qualify it as a sign of B's failure to have formed these concepts up till now. He takes it as *an exception to his colour theory that requires an explanation.* But taking it as an exception implies that he made his colour concepts *normative* or *lawlike.* He can only meaningfully speak of an exception when he has some norm to judge what is normal. And this, of course, is also an ad hoc manoeuvre, but when he adds that the exception requires *an explanation,* he has an explanation in mind *which removes the ad hocness.* And to do this, Mr. A formulates a completely new conjecture: "I think you are blind in a certain sense. You cannot observe a difference in colour, when you observe this tomato and the grass we are walking on. Your blindness could be called 'colour blindness'". And Mr. B may reason in a completely analogous way: "From your point of view (sic!), you may say that, but it does not solve any problem. You only gave the problem a name, as I could give it a name e.g. 'colour obsession', suggesting that you observe differences in colour where I observe none".

And Mr. B is right; A only made his concepts 'red' and 'green' lawlike in an ad hoc way and deduced on this basis that B is ab-normal in the literal sense of the word, i.e. deviating from the norm 'red', and he named this abnormality 'colour blindness'. But now Mr. A retires in his laboratory and after a long, long time he meets Mr. B again. A tremendous advance has been made. A physical theory of light and a physiological theory of the structure and the functioning of the human retina have been developed. Now Mr. A can say to

Mr. B: "I think you are colour blind. There is a certain defect in the structure of your retina. Visit an oculist and he will confirm my prediction". But B has already visited an oculist and he knows that he is colour blind: "I think there is no further disagreement. We have both reached better understanding and therefore restored intersubjective agreement. Both our conjectures, yours of colour blindness and mine of colour obsession, have lost their ad hoc character".

In my opinion, we can learn many things from this example, which is of course not meant to describe the factual history of the colour concepts. (I could also have chosen the folor concepts as my example; some people cannot observe a very small 'e' to be desolc; they need a magnifying glass because they are short-sighted; compare also the use of a letter-chart by an oculist).

In the first place the example illustrates the fundamental role exceptions play in concept and theory formation. And we mean those exceptions for which no acceptable explanation can be given at the moment of their occurrence; we shall, call them *genuine exceptions*. Mr. B's observation that this tomato is green constituted a genuine exception to A's concept 'red' (and 'green' of course) because A could not explain it by saying that B had not yet formed the colour concepts or that he had not yet observed in the appropriate way but that he would, sooner or later. So there is, at that moment, a genuine exception, in particular to 'red differs from green' or 'a thing cannot be red and green at the same time'. Therefore, the on-going process of concept formation and thus theory formation becomes possible thanks to such genuine exceptions. If there were no exceptions of this kind empirical knowledge would be restricted to a collection of such minute theories as the one sketched in section 4. And these theories would indeed be barren and static pieces of knowledge. But, fortunately, genuine exceptions do occur and they make our concepts fertile by initiating further investigation, thereby determining *empirical inquiry as a dynamic undertaking*. And we should honour them for this. We can only understand science as a dynamic concern if we give genuine exceptions the place they deserve in our epistemology: *they make further concept and theory formation possible and necessary and they may thereby lead to better understanding*. They do not get a fair chance in a logical positivist methodology, because 'regularity without exceptions' is preached there; and it must indeed be preached there in order to uphold the inductivist view of empirical knowledge.

Secondly it will be clear that there would be no exceptions without lawlike concepts. Mr. A must claim e.g. that a red thing cannot possibly be also green in order to qualify B's observational result as a genuine exception. Otherwise he could only accept it as a *falsification* of his concepts in the following sense: the concept 'red' cannot be said to be intersubjectively valid, it can at best be taken as a piece of private knowledge. Thus, if we are not prepared to acknowledge such a lawlike character or such necessary connections as 'red differs from green', we can do no justice to the fact that scientific investigation is an on-going process of concept and theory formation.

In the third place, *this lawlikeness of concepts must be empirically confirmed*, it is not justified *per se*. At first it is a matter of imputation and it therefore

has a purely ad hoc status. But the concept as a lawlike concept comes to be *confirmed by the result of on-going concept or theory formation.* At first the term 'colour blind' is only a name of the exceptional datum, which could only be observed by observing it through the lawlike concept 'red', but later on the concept of colour blindness is formed within a physiological theory, resulting from further investigation to which the exception was the incitement. And this confirmed 'red' as a lawlike concept. B's description is no longer a genuine exception, but it became an *explicable exception* or even stronger, within the new conceptual system, B's observational result became a 'normal case'. The meaning of 'red' has now been extended with an empirically confirmed lawlike trait through which it is connected with the concept 'colour blind' and with the theory by which this phenomenon can be explained or understood. The meaning of 'red' is determined by its being taken up into this theory and we cannot view 'red' as an isolated or theory-independent unit of meaning. This extended theory enables us to observe a datum like John's colour blindness. In other words the formation of new concepts, i.c. 'colour blind', implies the co-constitution of data which could not be observed before. Concept formation and datum constitution again go hand in hand, but we should notice the fact that the concept 'colour blind' has been formed in a way different from the way in which we formed e.g. 'nepo' and 'desolc'. In science most concept formation takes place in the way of the concept 'colour blind', but this way always presupposes the presence of other empirical concepts, which can be used as a norm in further investigation, in which new concepts may then be formed.

Let us now return to the teacher-pupil situation and the question how a teacher can ever know whether or not his pupil has made the correct creative jump or whether or not he has grasped the correct trial, which cannot be further explained. The role of the teacher seemed very important: the teacher determined whether or not his pupil had erred and whether or not his pupil's description was the correct one. But the role of a teacher is overestimated if we should say that it is a condito sine qua non for concept formation. A teacher can never *force* his pupil to form certain concepts. The decisive point lies in the pupil's ability to grasp the correct trial, he must take the jump by himself as his teacher cannot make him do so. This implies that concept formation is in principle possible without a teacher. A child is much like Hanson's *paradigm observer* who "is not the man who sees and reports what all normal observers see and report, but the man who sees in familiar objects what no one else has seen before".[38] Of course, a child does not learn to observe what no one else has seen before, but he must learn to observe and describe what he could not see and describe before and this activity does not differ principally from that of the paradigm observer. If a pupil from a certain moment on observes and describes in the same way as his teacher, we may say that he has become a *normal observer* having the same concepts as his teacher and able to cooperate with his teacher in doing *normal science*, in Kuhn's sense. But the justification of the pupil's concept does not lie in the fact that he grasped the correct trial, which stood the tests brought about by the teacher. Nor can the justification of the teacher's

concepts be found in the mere fact that he is the teacher. The justification of their concepts is obtained by an empirical confirmation of their concepts as being lawlike. Their concepts come to be justified once they have stood an empirical test in the sense of the example described above. In other words, their concepts come to be justified once they have proven to be fertile by enabling them to reach better understanding by on-going concept formation. This is clearly illustrated by a 'good teacher' at an elementary level: when his pupil, after many trials, nevertheless continues to err, a good teacher will stop using the explanation that his pupil has not yet grasped the correct trial. Such a teacher will see that this 'explanation' leads to *a piling-up of ad hoc theses*: every time a pupil makes a mistake in the eyes of his teacher, that teacher might say that he has not yet observed from the right point of view. But being a good teacher, he will realise that such an explanation could be used ad infinitum and he will therefore question his own concepts. His being a teacher cannot be taken as a justification any longer and he will face his pupil's observations as genuine exceptions, which need to be explained. He is then no longer a teacher, but he and his pupil become co-operators. And only if they succeed in giving the required explanation through further concept formation, will their concepts have stood the test: they have then been confirmed as being lawlike. What constituted a genuine exception before can now be observed as a normal datum.

The 'bad teacher', on the other hand, will continue to use the ad hoc explanation: he is the authority taking it for granted that there will eventually come a time when his pupil will succeed in learning to observe in the correct way. He may thereby temporarily block further investigation which could *restore mutual understanding*. He does not leave room for genuine exceptions, which might lead to a falsification of his concepts (see below for this possibility) and he acts very unscientifically because he does not seem to reckon with the fact that science aims at an empirical foundation of all knowledge – even of simple concepts – which implies intersubjective understanding, as we said before. As a matter of fact, he frustrates all future inquiry, because he deprives science of its dynamic character: he already has the answer to all future exceptions. In our example Mr. A acted in a scientific spirit by giving up his role as a teacher.

We have given an example in which a genuine exception led to a confirmation, but it may also turn out to be a falsification upon further inquiry. Notice the proviso 'upon further inquiry': as we said at the beginning of the example, Mr. A cannot simply qualify Mr. B's observational result as a falsification of his colour concepts, because this would also block the way to better and intersubjective understanding of the empirical phenomena. On-going investigation may, however, lead to nothing. More precisely, it may happen that we can only explain the genuine exceptions in question by adding new ad hoc statements to the ad hoc imputed lawlikeness of the concept, which made the exception an exception. We may, after some time, find ourselves confronted with a piling-up of ad hoc trials, not being able to extend and to refine our original concepts. In such a case some will go on and try again, but others will lose confidence in the concepts in question. In our example, further investigation led to an extension

and refinement (eventually a restriction) of the original concepts – new concepts were formed and new data co-constituted – and an explanation of the exception became possible. Afterwards, we may say that the exception caused a 'minor conceptual crisis', i.e. a crisis that could be warded off by an extension, refinement, etc., of the conceptual system at hand.

There is, however, no guarantee that future exceptions will also turn out to be explicable in this way, i.e. by a further extension, amending or refinement of the conceptual system. And when a result of such evolutionary science is long in coming, when it leads only to a piling up of ad hoc theses, some people may give up hope that continuing the investigation on the basis of the conceptual system at hand will ever be successful. They think it better to face the possibility of the exception being a falsification of the 'theory at hand', as well as the underlying inexplicable trials, grasped in a creative jump. These people find themselves confronted with a much graver conceptual crisis. They no longer believe in the possibility of evolutionary science, but think it necessary to reach better understanding by the formation of an alternative to the very theory evolutionary science started from. They commit themselves to do revolutionary science, facing 'a big crisis'. They have chosen the most difficult way because they can only prove their position by providing an alternative conceptual system (e.g. an alternative colour theory) and they must play the role of Hanson's paradigm observer, who can see in familiar things what no one else has seen before. The alternative theory must constitute an alternative method of observation so that the empirical data which could be observed and understood through the 'old' theory can also be understood and observed through the new one and the new theory must also enable us to observe and understand the genuine exceptions in respect to which the 'old' theory is taken to fail, as normal or explicable cases. Only when they succeed in doing this do they prove their point, because they cannot confine themselves to the remark that the 'old' theory has been falsified. A theory is not a coat that can be taken off at will. Therefore HANSON is right when he says: "Should someone claim he has a good reason for abandoning a theory T, but can suggest no alternative to T, *no other way to form concepts about the phenomena T covers* (my italics, H.K.), I deny that he has good reasons for abandoning T!".[39]

But there will also be those who stick to the 'old' theory, relying on the possibility of getting rid of the piled-up ad hoc trials by extending, etc., this theory. They face the situation as a minor crisis.

Nothing can be said in advance about the possible success of these two approaches. The exceptional orbit of Uranus could be explained by evolutionary science: the discovery of Neptune fitted very well in the conceptual system at hand. An analogous attempt to account for Mercury's exceptional behaviour failed in this sense, that the 'Vulcan-trial' remained purely ad hoc. Revolutionary science had to be done (by Einstein) in order to reach better understanding. The ether-theory was, for a long time, defended by scientists, who thought it possible to get rid of the ad hoc additions by doing evolutionary science. But revolutionary science settled the debate, at last.

It could, however, not be said in advance which way would lead to success, i.e. to better understanding.

When the 'progressives' have, in their own view, succeeded in the formation of an alternative theory, then the battle has just begun. The 'conservatives' (without any pejorative connotation, unless they start to behave like the aforementioned bad teacher!) defending the 'old' theory, cannot be easily convinced and I think this is perfectly understandable after what has been said about concept formation up till now. They are asked to give up their familiar conceptual system and to learn to observe in a completely new way, incompatible with the way in which they observed through the 'old' theory. They are asked to become paradigm observers and a severe hindrance is formed just by the familiar conceptual system, which enabled them to observe and to explain a great many empirical data. (This is a difference with the child, whom I compared with Hanson's paradigm observer a moment ago: the child is not hindered by an empirically well-confirmed alternative system of concepts).

PLANCK has given a rather negative judgment of such a battle: "Eine neue wissenschaftliche Wahrheit pflegt sich nicht in der Weise durchzusetzen, dass ihre Gegner überzeugt werden und sich als belehrt erklären, sondern vielmehr dadurch, dass die Gegner allmählich aussterben und dass die heranwachsende Generation von vornherein mit der Wahrheit vertraut gemacht ist" [40], and of course some 'conservatives' may start to act like Authorities, like Ostwald, Mach and Boltzmann in Planck's view. But this is, at least, a onesided appreciation of such battles. They also play an important role in the confirmation of the newcomer. A new theory gains its empirical confirmation from the fact that it leads to better understanding of certain empirical data and this implies intersubjective agreement. Once the battle is over and it appears that the new theory has beaten its rival (which is of course not the only possible result), it has proven that it leads to better intersubjective understanding: we have all learned to observe in a new way and we have thereby reached better understanding. We have formed new concepts and new data. Going through such a battle the new theory is subjected to a very severe test. The death of the (authoritarian) opponents, the 'old and honourable men', cannot explain the victory of a new theory, because these men also had their pupils from 'die heranwachsende Generation'. And these pupils, educated in terms of the 'old' theory, may nevertheless reject this theory and adopt the new one. They do so because of its greater explanatory power (in the sense frequently indicated). With the new theory one can observe and explain what could not be observed, explained or derived before. When the authorities or bad teachers die, a practical obstacle may be removed, but this does not guarantee the victory of the theory they opposed.

In this way, the confirmation of a concept as lawlike or as implying a necessary connection and also its falsification become possible thanks to genuine exceptions. And genuine exceptions only exist if we acknowledge this lawlikeness, which cannot possibly be the result of an observed regularity, simply because an

irregularity (i.e. a genuine exception) is needed to establish it. Confirmation and falsification are both the result of on-going concept formation.

We must add two remarks at the end of this section. The first is this: neither a minor crisis nor a big crisis can be solved by convention. Such a manoeuvre would again block the road to better understanding. It would again frustrate the dynamic character of empirical science and misapprehend the function of exceptions. A colour-blind person such as Mr. B is not someone who refuses to join a convention. On the contrary, he could not join it even if he wanted to. And the battles fought to defend old theories and to have a new one accepted would be completely irrational if it were a matter of convention – why make so much fuss about a convention? Later on, when we consider the concept of empirical law, we shall come back to this point.

New genuine exceptions, and this is our second remark, always remain possible. They may incite evolutionary science, leading to new lawlike traits of the concepts at hand and their confirmation. But they may also initiate revolutionary science, they may force us to consider the possibilty of our conceptual system, including its lawlike traits or necessary connections, being falsified. It may, for example, happen that we must consider the possiblity of *dissolving the empirically confirmed necessary connections* present in the concept 'red', in order to make room for an alternative connection, which could lead to an explanation of the exception within a new colour theory. Of course we cannot imagine such an alternative because our imagination of colour phenomena is bound to our present colour theory, but this does not exclude the possibility of this theory being falsified (compare my remark in section 4 with respect to the statement 'red is a colour'). So the lawlike traits or necessary connections are not irrevisable, come what may. If one interprets the term 'necessary' in this way, as QUINE did, and then rejects it, one is right, but at the same time one makes a rather gratuitous remark about empirical knowledge. It is gratuitous because it opposes a necessity in an observer-independent reality, but in empirical science we get knowledge of an empirical reality, which is co-constituted by the conceptual system or theory which makes observation possible and this reality changes when our theories change. And from the point of view of theory-loaded observation, exceptions are a definite necessity for an empirical science and hence necessary connections are, far more than 'a constantly going-together' or a 'regularity without exceptions'.

6. THE LAWLIKENESS OF AN EMPIRICAL CONCEPT-II

I now wish to draw attention to another kind of lawlikeness present in empirical concepts, which may, once more, make it clear in what sense a concept is a minute theory or essentially theory-dependent. Much of what has been said in section 5 is also valid here.

The lawlikeness I have in mind *consists of a norm (or norms) determining certain circumstances, under which a thing may be observed through a concept, to*

be relevant for the applicability of that concept. The problem of relevant conditions or circumstances is frequently mentioned in the literature, but has never been systematically analysed. We may illustrate this lawlikeness by an example: part of the meaning of 'red' (and the other colour concepts) is that the intensity of the irradiating light is relevant to the applicability of this concept. Likewise the kind of light is a relevant circumstance. In other words, we can only get confirming or falsifying evidence for the statement 'this is red' when we observe it in the presence of 'sufficient light', as CARNAP once put it[41] and when it is irradiated by white light and not e.g. by a sodium vapour lamp.

This lawlikeness determines the *dispositional character* of such concepts (cf. POPPER in note 36). The concept, entailing such lawlike traits, enabling us to observe certain data, determines a lawlike behaviour of those data. On the basis of such a lawlikeness, an observation of a red tomato in the evening, resulting in its being grey, does not constitute evidence against its being red, nor for its being grey. The observation was not performed under *normal circumstances.* The colour 'behaviour' of that tomato is governed by the lawlike character of the colour concepts. It is normal, given the concept 'red', that a red thing appears to be grey in the evening, when the light intensity has become too small to observe coloured things. The colour change can be explained by the conceptual system at hand. (cf. Ayer's discussion of the shape of a coin).

But in order to see what this lawlikeness means, we must go back to a situation in which the conceptual system has not yet sufficed to explain this phenomenon of what we now call an apparent colour change. After all, 'red', as a lawlike concept, must once have been formed.

When, in this situation, Mr. A observes the behaviour of the tomato, he cannot explain it. He observes a change of colour, but he is not content with the 'explanation' that apparently things may be red at one time and grey at another time. He does not take things 'as they are', but he wants *to restore understanding.* This introduces two questions: first "Why is he not content with a description of 'the facts'?" and second "What is meant by understanding and its restoration?" The first question might be answered with: "Because a description of a change does not make him understand why it happened. A description, in this case, is not an explanation, but simply a restatement of the fact to be explained". This leads us to the second question, which may be answered with: "If A wants to understand or to explain a change, this means that he wants to be able to observe it as a normal phenomenon". But this is either tautologous or very cryptical and it must be elucidated. Mr. A is supposed to have *the classical concept of change*, which implies that every change has a cause. A change without a cause would not be a normal change or it would not even be understandable as a change within his concept of change. Let us assume that he uses this concept in its secularised sense of a methodological rule.[42] A change always has a cause in his eyes, and it becomes a normal phenomenon when he has traced its cause or when he has discovered the circumstance which is responsible for the change and which thereby has become relevant. He has then restored the possibility of normal science in that he can explain it within his conceptual scheme.

(The first question, mentioned above, could now be substituted by "Why does he have this concept of change or why does he demand understanding in causal terms?", but I cannot go into this question here).

On the basis of his concept of change, Mr. A says:

> if a thing is red at t_1, then it is red at any other time, t_n, if all relevant circumstances are the same at t_n.

This is simply an application of his methodological rule to the concept 'red': it excludes the possibility of a red thing changing its colour without any cause, because in that case it could not even be conceived by A as a change. In this way, Mr. A formulates a very elementary *lawlike trait* of 'red', *determining a lawlike constancy behaviour* and it will be clear, I hope, that this does not mean a constancy in an observer-independent nature, but a constancy which is conceptually co-constituted.

This classical concept of change is also used to mark the difference between classical statistical mechanics and quantum mechanics as conceived by BOHR. Quantum mechanics describes and explains phenomena which have a principally statistical character and A's concept of change can no longer be applied. But classical statistical mechanics is statistical for practical reasons and A's concept of change is applicable in principle to the phenomena this theory explains [43] – it is even used as a norm here to be able to speak of the practical statistical character of classical mechanics. When BOHR frequently stresses the fact that he concept of causality cannot be applied to quantum phenomena, this means that the classical concept of change cannot be applied either.[44] But Mr. A cannot but understand a change through his concept of change: he could not meet quantum processes – he could not understand them, because he has not formed the appropriate concepts through which such an understanding would become possible. So, if he wants to understand a change, he first tries to reach his goal by evolutionary science, i.e. by a further formation of the concepts he has already formed. And when such attempts do not succeed – *and they may fail, they may lead to a piling-up of ad hoc additions*, as we know – a big conceptual crisis may arise and then A's concept of change is no longer sacrosanct.

But now, meeting with a factual change, that evening in the garden, Mr. A must trace the relevant circumstance, he predicted to exist. His first conjecture is:

> when it gets cold a red thing turns grey.

It was indeed becoming cold that evening and he proposes its being cold or warm to be a relevant circumstance. This conjecture has an ad hoc character and as long as this has not been removed, Mr. A has not succeeded in explaining the change of colour or in restoring understanding or in forming his colour concepts so that he can observe the change as normal. He may adduce many positive instances, but being a good scientist, he does not accept a mere cumulation of positive instances as a good ground for the removal of the ad hoc character. If a mere description of a change does not explain it (see above) then why should an infinite number of descriptions of analogous changes do so?

As the conjecture is ad hoc as a universal statement, the inductively established universal statement would be ad hoc and we cannot get rid of this by an appeal to a great number of positive instances, but we shall come back to this in a moment.

Mr. A, as a good scientist, no longer looks for positive instances. He waits until it has become winter, when it is much colder then that evening in the garden, and he observes that things, which were red on a sunny day in the summer, are also red on a sunny day in the winter. So he meets with negative instances, but, again as a good scientist, he does not take these as falsifications of his conjecture. *He realises that he will always need negative instances as genuine exceptions and not as falsifications, if he wants to get rid of the ad hoc character of his conjecture, if this is possible at all.* He therefore takes them as genuine exceptions, which implies the making of his conjecture lawlike or normative in an ad hoc way (compare section 5). But he should then formulate a new conjecture enabling him to reduce the genuine exceptions to normal phenomena or explicable exceptions.

We shall not continue the description of these attempts and assume, that A is forced to add ad hoc conjectures again and again. Instead of getting rid of the ad hoc character or instead of succeeding to confirm the conjectured lawlike trait by on-going theory formation, a piling-up of ad hoc additions occurs. He then loses his confidence in his conjecture, considers it as erroneous and has to start anew. The points to be noticed here are, that first, cumulation of positive instances alone is not enough for the confirmation of a concept as lawlike or for the getting rid of its being ad hoc (see also below); second, the appearance of negative instances alone is not a good ground for the falsification of the lawlikeness of a concept; and third, the piling-up of ad hoc additions to which the negative instances, as genuine exceptions, give rise may form good ground for giving up the conjecture and only then negative instances may be called falsifications. *Negative instances never declare themselves to be falsifications.* Mr. A is doing evolutionary science and this often implies a temporal ad hoc status of a concept. Moreover, the one will lose confidence sooner than the other (EINSTEIN never lost confidence in the classical concept of change, through which Mr. A performs his investigations[4,5]), but *the falsification of a conjecture is only definitively established once the unbeliever has established an alternative conjecture.* In other words, a conjecture can only be taken as rejected, when the ad hoc character of an alternative conjecture has been removed by theory formation on the basis of on-going investigation.

Now this is exactly what Mr. A tries to do. His second conjecture is:

when it gets dark a red thing turns grey.

Again A may present an overwhelming number of positive instances, as in the first case. But this alone does not remove the ad hoc character of A's second conjecture. One might propose that the fact, assuming it to be a fact for the moment, that no negative instances have been observed is a ground for taking the conjecture as established or well-confirmed and, therefore, as no longer ad hoc. So, one might argue, a cumulation of positive instances of a conjecture

removes its ad hoc character, when no negative instances are found. But this is very ambiguous.

First, one may intend to say that negative instances remain possible in the future or that negative instances may have occured in the past but that they remained unnoticed. Second, one may intend to say that it is impossible that negative instances will turn up. With respect to this second interpretation we can be very brief: in that case one makes the conjecture lawlike (or normative) and the cumulation of positive instances in the absence of negative instances does not confirm this; or, the repeated observation of a red thing turning grey upon darkening is not a good ground for the thesis that a red thing *must* turn grey when it is getting dark, even if it has never been observed that a red thing did not turn grey when it was getting dark. Upon this second interpretation, no confirmation or removal of ad hoc character has taken place; *the cumulation of positive instances in combination with the absence of negative instances does not confirm a necessary connection.*

But the first interpretation, which leaves negative instances possible and which may be called the positivist (or: inductivist) view, leads directly to the question what will be done when a negative instance does, as a matter of fact, occur. A consistent positivist should, I think, answer that in that case the conjecture in question is falsified. But fortunately, positivists are not very consistent in this respect. If they were, they would alienate themselves and their methodology completely from scientific practice. The correct answer would be, that a negative instance is taken as a genuine exception, which implies making the conjecture lawlike. But if the status of the conjecture is so determined that negative instances alone, be it that they have not been observed up till now, cannot falsify it, but constitute genuine exceptions, then one has also made the conjecture lawlike or normative or the connection necessary. So, also upon the the first interpretation, the cumulation of positive instances in the absence of negative ones does not confirm the conjecture in question or remove its ad hoc character. One might object to this analysis of the first interpretation, that it suddenly introduces scientific practice. Now, I think, the practice of science is often a good guide in the solving of methodological problems, but I also think that it is possible to give the ratio of the practice we referred to (this also explains why we called Mr. A a good scientist in connection with his first conjecture). Even if it were a fact that there are, omnitemporally, no negative instances of the conjecture, our knowledge of this fact would not enable us to give a non-ad hoc explanation of the change of colour in question. For in that case, i.e. if omnitemporally there were only positive instances, there would be no difference between such an 'explanation' and e.g. the one we can give of the redness of an apple from Hempel's well-known basket[46] in which all apples are red. No more than the redness of all apples in that basket can explain the fact that a particular apple in that basket is red, can the turning grey of all red things upon darkening explain the fact that this tomato turned grey yesterday-evening. If there is only accumulating positive evidence and no negative instances, and if this is, as a matter of fact, always so, the

best we can say is that the conjecture formulates an 'historical accident on a cosmic scale', as KNEALE called it.[47] And the fact that a scientist *wants to understand* the phenomena he can observe through his concepts, which means that he wants to get rid of the ad hoc character, results in his wanting more than a mere formulation of a regularity without negative instances or of an historical accident on a cosmic scale. Therefore, scientific practice, searching negative instances and taking them as genuine exceptions, is completely understandable.

We needed this lengthy excursion to make it clear that no matter how many instances Mr. A may adduce in the absence of negative instances, this alone will not enable him to achieve the required understanding.

Fortunately, Mr. A is still a good scientist and he is not fascinated or fooled by the large number of positive instances: *positive instances alone do not confirm in any way, just as negative instances alone do not falsify in any way.* Mr. A realises that, if he really wants to restore understanding within his concept of change, he must confront his conjecture with negative instances. He knows that even the formulation of an historical accident on a cosmic scale cannot help him and that he needs a lawlike or necessary connection for the explanation he is after. He needs extra evidence to establish this lawlikeness. And indeed negative instances turned up: Mr. A makes his conjecture lawlike:

when it gets dark a red thing *must* turn grey

and the negative instances are genuine exceptions. He gives them a name: 'phosphorescence phenomena'. But this manoeuvre of naming does not, of course, by itself constitute a confirmation of his lawlike conjecture (compare the naming of Mr. B's behaviour in section 5 with 'colour-blind'). Only when he has formed the concept 'phosphorescence' by on-going investigation does he succeed in getting rid of the ad hoc character of his lawlike conjecture. Once he has formed a theory of light in which the concept of intensity is taken up and in which different emission phenomena are explained, once he has formed a theory of the retina and in particular of the functioning of the cones and rods in the retina, he has succeeded in reducing the genuine exceptions to normal cases and gained an empirical confirmation of his lawlike or necessary connection.

The genuine exception has then revealed itself as a new datum, called 'phosphorescence', which could not be observed before and which is co-constituted by the conceptual system now at hand. A new concept has been formed in a way different from the nepo-desolc example, but comparable to the formation of the concept 'colour-blind'. I think this idea of confirmation by on-going concept or theory formation may be compared with what HARRÉ called 'an ontological experiment'[48], which in his view, could also remove the ad hoc character of a theory.

A thing's being phosphorescent is now as normal a datum as a thing's being red and *thus* its turning grey upon darkening. Moreover, Mr. A can now explain why there were so many positive instances of his first conjecture and only now he may take the negative instances of this first conjecture as falsification.

Mr. A's attempt to confirm his conjecture or to get rid of its ad hoc character or to arrive at an explanation of the observed phenomena, came down to the

attempt to reduce (possible or actual) negative instances, viewed as genuine exceptions, to normal or explicable phenomena. It came down to the formation of a theory or conceptual system through which the exceptions could be observed as phenomena which should indeed be observed, if that theory or conceptual system were a reliable method of observation. The concept 'red' now implies the lawlike connections as a part of its meaning, determining a lawlike behaviour of red things. But this concept cannot be viewed as an isolated unit of meaning, because it is connected with a very extensive conceptual system by means of these lawlike traits: *it has the meaning it has by being taken up in that system.* But there is not first the system in which 'red' is then taken up. The formation of this conceptual system implied the formation of 'red' as a part of it: the system and the concept cannot be separated without destroying them both.

We restricted ourselves to one specific lawlike trait, but 'red' entails other lawlike connections as well, e.g. about the kind of light that must irradiate a thing in order to be able to verify of falsify wether or not a thing is red.

Two questions deserve to be considered in concluding this section: first, one might ask whether negative instances, as genuine exceptions, are really needed, as I have said several times; second, one may remark, that on-going theory formation never completely removes the ad hoc character, so that the required confirmation cannot be gained.

Let us consider the first of these two questions. With our example we did not intend to say that the phosphorescense phenomena, referred to in this example, are indispensable for the confirmation of this particular lawlike conjecture. Other phenomena might have been considered in our example, e.g. a red-hot piece of iron that remains red upon darkening (but not upon cooling!). But a confirmation of a lawlike connection may also take place indirectly: *it may be armed against negative instances* by its being deducible from other connections which have been directly confronted with negative instances, viewed as genuine exceptions and which came to be explicable by on-going theory formation. In other words, a lawlike connection may be confirmed as lawlike by its being deducible from other connections, which have been confirmed as lawlike before. I do not think it is important *where* exactly genuine exceptions start to operate, but I think it is important for the confirmation of lawlike traits *that* they operate at some level.

The second question comes down to this: is it not true that only a shift of the ad hoc character from the original conjecture to 'later' conjectures takes place? In that case it can never be completely removed.

The answer to this question should start, I think, with the making of a distinction between two attitudes towards a theory or a conceptual system. First, one may view a conceptual system in the flux of on-going investigation. In that case, in the external sense, so to speak, one cannot speak of necessary connections at all. For this means that one views a theory as a member of a series of possible successors which have come to be accepted as the alternatives of their predecessors. And externally, as we also remarked in section 5, the necessary connections do not exist: new empirical evidence may come and may in-

deed force us to break the *internally necessary connections* (see below) open. This may occur on the basis of new exceptions for which the attempts to reduce them to normal cases or explicable exceptions leads to a piling-up of ad hoc additions. Such exceptions may occur at all levels. They may occur at the 'simple' level, e.g. a red thing remaining red upon darkening when it is neither red-hot nor phosphorescent, and at the 'advanced' level, e.g. light not only being a wave phenomenon. When such exceptions occur, normal science may no longer be possible; revolutionary science, leading to an alternative theory, may then be necessary to restore understanding and thereby to restore the possibility of normal science.

Second, one may view a conceptual system as enabling us to understand or explain the phenomena, which can only be observed through this theory. It is then taken as a relatively closed system enabling us to do normal science. It is, so to speak, separated from the abovementioned flux. In this case – and we may call this case *internal – a theory is a system of necessary connections, determining the meaning or intention of the concepts in it.* The 'simple' as well as the 'advanced' connections are necessary for, if they were not, the understanding the theory offers us would slip from our grasp. Normal science is normal just because there is a conceptual system which consitutes its norm as laid down in the necessary connections.

As the concepts of necessity or lawlikeness can only be meaningfully applied internally (externally it is neither applicable in a true nor in a false way to anything and therefore Quine's attack is spurious), the concepts 'true' and 'false' can also only be applied internally or given a conceptual context. If we say of a thing that it is red, we can verify this by observing it through the concept 'red' and such a verification implies, in principle, that all relevant circumstances are of the required kind (i.e. normal) and that the instruments used are the suitable (or normal) ones. The concept through which we observe makes it possible to judge circumstances and instruments. When such an observation shows that the thing is red, then the statement 'this is red' is true. *This truth is internally indubitable*: if it should be doubted this would mean that the concept 'red', as a confirmed method of observation, should be doubted. And internally this is not the case. But the statement is of course not indubitable externally. Just as there is no place for necessary connections in the sense of 'remaining necessary come what may', there is no true statement in the sense of 'remaining true come what may'. The concepts of truth and necessity may be meaningfully applied internally, so that an empirical truth or a necessity in nature is always a truth or a necessity in an observer-dependent empirical reality. I have not the faintest idea what these concepts could mean externally or independent of a conceptual scheme or relative to an observer-independent reality, because we can have no knowledge of such a reality in empirical science.

It may clarify matters to add a remark about this dependency. In section 3 I already stated, that atoms existed before our modern concept 'atom' or our atomic theory was formed. I think we can now make this a little more precise.

Every empirical concept implies lawlike traits determining a lawlike beha-

viour, including the so-called lawlike constancy behaviour of the data which can be observed through that concept or which are co-constituted as those data by that concept. Thus, once a concept, as a lawlike concept, has been empirically confirmed, we may say, *on this basis*, that those data exist when we do not de facto observe them (e.g. this book is still here when you have turned around and no longer de facto observe it), or that they existed a long time ago, when nobody could possibly observe them, because the appropriate concepts had not yet been formed (the example of the atoms), or that they will continue to exist. But all these cases of existence are cases of an existence in an observer-dependent, i.e. conceptually co-constituted, reality. And the lawlike traits of our concepts form the basis of these statements. This means, that the concept of existence (atoms existed 1.000.000 years ago, the earth existed long before there were human beings, this book continues to exist if I do not de facto observe it, stones will still exist in A.D. 2050) can also only be meaningfully applied internally. But the above-made distinction between internal and external should not, I think, be identified with Carnap's distinction between internal and external questions, when he considers the concept of existence.[49]

In this chapter I have tried to analyse the notion of theory-loaded observation as opposed to theory-free or direct observation. Theory-loaded observation is observation through a concept and therefore through a conceptual system, because a concept is always a concept within a theory. We tried to express this by saying that each empirical concept is a minute theory: it focusses the system in which it gets the meaning it has. This meaning is built up from (a) what we called the distinct-from relations, (b) the lawlike traits determining certain instruments to be appropriate for the verification of 'a full sentence' (to borrow Carnap's term) of that concept, and (c) the lawlike traits determining the behaviour of the data observable through that concept under relevant circumstances. From this epistemological context we shall now first analyse Goodman's riddle and then two views concerning the concept of empirical law, namely the regularity view and the necessity view. That we treat Goodman's riddle first, in a separate chapter, and not as a part of the problem of lawlikeness, will be justified by showing that it is primarily a problem of concept formation.

II. GOODMAN'S RIDDLE

1. THE FORMULATION OF THE PROBLEM

In a first approximation the story seems very simple. Up till now (the 16th August, 1972) all examined emeralds have been green. Let us suppose that on this basis, i.e. a regularity without exceptions up till now, the universal statement

all emeralds are green (I)

is formulated. All examined emeralds have been positive instances and they are taken to confirm (I), if we use Nicod's conception of confirmation, as it was formulated by HEMPEL. (I) can be equivalently reformulated as a universal conditional hypothesis and: "an object confirms a universal conditional hypothesis if and only if it satisfies both the antecedent and the consequent of the conditional; it disconfirms the hypothesis if and only if it satisfies the antecedent, but not the consequent of the conditional".[50] It is this conception of confirmation that is used by GOODMAN in the exposition of his new riddle.

GOODMAN then introduces his famous predicate in the following way: "Now let me introduce another predicate less familiar than 'green'. It is the predicate 'grue' and it applies to all things examined before t_0 just in case they are green but to other things just in case they are blue".[51]* GOODMAN has taken t_0 as the present time t, but we shall pin it down to the first September of 1984 (the Orwellian time).

Upon this definition of 'grue', the positive instances of (I) are likewise positive instances of

all emeralds are grue (II)

Both (I) and (II) are equally well confirmed by the same evidence. Or, in Goodman's own words, "the prediction that all emeralds subsequently examined will be green and the prediction that all will be grue are alike confirmed by evidence statements describing the same observations".[52] But with (I) we predict an emerald to be green in the Orwellian era and with (II) we predict that it then will be grue and thus blue and not green. So, we arrive at contradictory predictions about the colour of an emerald after t_0 on the basis of exactly the same evidence.

Intuitively we should take (I) as confirmable by the mentioned positive instan-

* I take Goodman's explication to imply the following:

$t < t_0$: examined:	green	– grue
	not green, e.g. blue	– not grue
not examined:	blue	– grue
	not blue, e.g. green	– not grue
$t \geqslant t_0$: examined:	blue	– grue
	not blue, e.g. green	– not grue
not examined:	blue	– grue
	not blue, e.g. green	– not grue

ces and (II) as non-confirmable and it is precisely the problem of finding a criterion for distinguishing between confirmable (or lawlike, according to GOODMAN) and non-confirmable (or accidental) universal statements that constitutes the new riddle of induction.[53] In Goodman's view the *only* way out of this dilemma can be found in the history or biography of the rival predicates 'green' and 'grue': entrenchment constitutes the only possible criterion to establish an *asymmetry* between (I) and (II) via the predicates 'green' and 'grue'. In all other respects these predicates are *symmetrical*. What can be said of 'green' can completely analogously be said of 'grue'. In particular the one is not 'primitive' and the other 'derived', or the one is not 'purely qualitative' and the other not, or the one is not 'non-positional' and the other 'positional'.[54] And we must be careful not to destroy this symmetry in a way forbidden by the very formulation of the problem.

I do not agree with Goodman's *solution* of the problem. I think, for example, that Slote's question "why should a predicate with which inductive generalisations have been more *frequently* made necessarily be a predicate with which it is *preferable*, more *reasonable*, to make such generalisations?"[55] is a good question.[56]

But neither do I agree with the *problem itself* and it is the main aim of this chapter to make this clear, although I shall incidentally insert a remark concerning the inadequacy of Goodman's solution if it were a genuine problem. Before going on, however, we must formulate Goodman's problem more precisely and in my opinion Mary Hesse's article 'Ramifications of 'grue'' is a good guide to this purpose.

Following MARY HESSE, we introduce two gentlemen. First, Mr. Green, who speaks the language L_1, in which 'green' and 'blue' are primitive concepts, namely colour concepts. The predicates 'grue' and 'bleen' are introduced in L_1 by means of the following definitions:

$$(x)\,(t)\{(x \text{ is grue at } t) \equiv (t<t_0 \rightarrow x \text{ is green}).(t \geqslant t_0 \rightarrow x \text{ is blue})\} \qquad (L_1\text{–}1)$$

$$(x)\,(t)\{(x \text{ is bleen at } t) \equiv (t<t_0 \rightarrow x \text{ is blue}).(t \geqslant t_0 \rightarrow x \text{ is green})\} \qquad (L_1\text{–}2)$$

Second, Mr. Grue, a speaker of the language L_2, in which 'grue' and 'bleen' are primitive and colour concepts; 'green' and 'blue' are introduced by means of the definitions:

$$(x)(t)\{(x \text{ is green at } t) \equiv (t<t_0 \rightarrow x \text{ is grue}).(t \geqslant t_0 \rightarrow x \text{ is bleen})\} \qquad (L_2\text{–}1)$$

$$(x)\,(t)\{(x \text{ is blue at } t) \equiv (t<t_0 \rightarrow x \text{ is bleen}).(t \geqslant t_0 \rightarrow x \text{ is grue})\} \qquad (L_2\text{–}2)$$

There has been much debate about the definition of a predicate like 'grue' (a Goodmannian predicate), but I think it is safe to make use of the kind of definition originally formulated by BARKER and ACHINSTEIN and (equivalently) reformulated by BLACKBURN.[57]

Furthermore, we should avoid the introduction of "needless asymmetries into the definition or interpretation of the problematic predicates", as MARY HESSE warns us[58] and she makes three assumptions which should prevent us from doing so:

first, L_1 and L_2 differ only in two descriptive predicates; 'grue' replaces 'green' and 'bleen' replaces 'blue' wherever they occur in a description;

second, Green and Grue accept all four predicates as colour predicates;
third, both Green and Grue commit themselves to "emeralds remain the same colour after t_0" in their respective predictions.[59]

(The last assumption is a consequence of the second: Grue, for example, describes the colour of an emerald before t_0 as 'that emerald is grue' and he predicts of an emerald after t_0 that it will be grue; so if 'grue' is a colour concept, he implicitly states that there is no colour change at t_0).

Given these definitions and these assumptions, Green and Grue, *on the basis of the same observations*, as GOODMAN puts it, arrive at contradictory predictions about the colour of an emerald in the Orwellian era. And both can agree that there is such a contradiction between their respective predictions. In Green's view, such an emerald is green, while Grue says that it is grue and thus, via (L_1–1), blue and therefore not green. In Grue's view it is grue, while Green said that it was green and hence, via (L_2–1), bleen and thus not grue. Or, in L_1 Green predicts 'green' and Grue predicts 'blue' and in L_2 Grue predicts 'grue' and Green predicts 'bleen'. Neither of them predicts a colour change. Now intuitively we choose L_1 and not L_2, but, although intuition is not always a bad guide, the very problem is to show why our intuition is a good guide in this case. As long as we have not succeeded in doing this, then "if we simply choose an appropriate predicate then on the basis of these same observations we shall have equal confirmation, by our definition, for any prediction whatever about other emeralds – or indeed about anything else. We are left once again with the intolerable result that anything confirms anything".[60]

The problem has been stated precisely enough by now, I think, and in the next section I shall try to show that there is already a problem at the 'singular level', i.e. in calling the evidence statements like 'this emerald is green' or 'this emerald is grue' true. This point has been overlooked up till now, as far as I know, but it has far reaching consequences for the formulation of Goodman's problem.

2. THE TRUTH OF '*a* IS GREEN' AND '*a* IS GRUE'

Suppose Green and Grue examine emerald *a* at t_1 under the same relevant circumstances (compare ch. I, section 6).
Green says: *a* is green
Grue says: *a* is grue

It is always assumed that there exists no problem at this stage of the exposition of Goodman's riddle. Both singular statements are taken to be true descriptions of emerald *a* and both Green and Grue supposedly can agree upon this fact. Everyone followed GOODMAN with respect to this point. There is complete symmetry between 'green' and 'grue' and complete agreement between Messrs. Green and Grue about what they say at this 'singular level'. Disagreement can only arise at the 'universal level', which is reached by inductive generalisation or projection, for then they derive their contradictory predictions about the colour of an emerald after t_0.

But we should nevertheless stress the question: "Can Green (Grue) really agree with Grue (Green), that '*a* is grue' ('*a* is green') is true?"

Let us first examine the usual affirmative answer to this question for the case of Green: "Can Green really agree with Grue that '*a* is grue' is true?" (notice, that we could just as well have examined this question for the case of Grue and what follows would be exactly the same except for the replacement of 'Green' by 'Grue' and of 'green' by 'grue' and vice versa; we shall come back to this). The answer is never explicitly stated just because the question has never been viewed as a problem. But I think there could be no objection if we were to say that all authors let Green reason as follows:

Mr. Green: "When I heard Grue say '*a* is grue', I checked the time of observation, t_1, and established that $t_1 < t_0$. Of course, I did not need this information for my own investigation of the colour of *a*. By means of my definition of 'grue' (L_1–1), and this is the only thing I know about this concept, I know that Grue says:

($t_1 < t_0 \rightarrow a$ is green).($t_1 \geqslant t_0 \rightarrow a$ is blue).

This would be the translation in my language, L_1, of what Grue intends saying in his language, L_2.

Now, I can indeed accept this conjunction as true, because '$t_1 < t_0$' and '*a* is green' are both true, as I established myself, and therefore the first conjunct is true; and the second conjunct is also true, because '$t_1 \geqslant t_0$' is false. So, the conjunction is true. And Grue's description of emerald *a*, be it a strange one, is as true as my own description of that emerald".

But Green, and with him all those who let him argue in this way, shows himself to be a hardened *logical* positivist. He, and with him all those who let him argue in this way, seem to be blind to the facts. For it is a fact that *empirical reasoning and empirical inquiry is not governed by extensional logic in this way.* There is no scientist who will ever accept a singular empirical conditional as true just because its antecedent is false (this seems a platitude to me). No scientist will accept the empirical conditional '$t_1 \geqslant t_0 \rightarrow a$ is blue' as true just because t_1 as a matter of fact is smaller than t_0. If, for example, a physicist says of this particular piece of copper, which is not heated, 'if this piece of copper is heated, then it expands' and he takes this to be a true description of this piece of copper, as he certainly will, please do not say that he is right in doing so because this piece of copper is not heated. He will laugh at you or, if he can still remain serious, he will say that you do not understand what empirical science is. And if the scientist in question is very patient he will explain that he has quite different evidence as a basis for his description of this piece of copper and that the falsity of the antecedent of such a description *never* constitutes any evidence for its truth, let alone that it would be the only ground for it. Nevertheless this is exactly what Green (and with him all those...) does. It is rather difficult, if not impossible, to explain why this point has been systematically overlooked. Everybody acknowledges the discrepancy between the use of the empirical concept 'if...then', which I will call the empirical conditional, and the use of the formal logical concept which is generally called material implication, but it

seems to have been forgotten the very moment it becomes important. And everybody knows that a counterfactual or a contrary-to-fact or unfulfilled conditional cannot be called true solely on the ground that its antecedent is false: *other evidence is required to establish the truth of such a statement.* The analysis of this 'other evidence' is precisely the problem of counterfactuals. But Green (and with him all those...) fails to appreciate this fact.

(Later, in ch. III, I shall investigate the problem of counterfactuals. It may seem a little strange to call '$t_1 \geqslant t_0 \rightarrow a$ is blue' or 'if this piece of copper is heated then it expands' counterfactual. One might reserve this term for conditionals whose antecedents are not only false, but cannot be 'made true' either. The antecedent of the copper-statement, for example, can be made true simply by heating this piece of copper as yet, and the antecedent of the statement about the emerald can be made true simply by waiting until the time of observation, t_1, becomes equal to or greater than t_0. One might also reserve the term 'counterfactual' for conditionals in the subjunctive mood. The two conditionals mentioned above are in the indicative mood. Whether this makes sense or not will be considered in ch. III. For the moment suffice it to say that the conditionals, considered here, share an important characteristic with the suggested 'real' counterfactuals, namely that their antecedents are false. And therefore these conditionals are in the same boat with the 'real' counterfactuals with respect to the requirement of other evidence. Whether or not there is a separate problem of 'real' counterfactuals, marked by an 'unrealisable' antecedent, remains to be considered. For the present we shall use the term 'counterfactual' in the wider sense, including singular conditionals in the indicative mood whose antecedents are, de facto, false).

Now, let us suppose, that we succeeded in convincing Mr. Green (and with him all those...) of the unacceptability of the step in his argument in which he concluded that '$t_1 \geqslant t_0 \rightarrow a$ is blue' is true, because this step was solely based on the falsity of '$t_1 \geqslant t_0$'. This, of course, has far reaching consequences, because he now takes Grue to say that emerald *a* is green and that it is, or would be or would have been, blue if t_1 is, or would be or would have been, equal to or greater than t_0. Or, in other words, Green takes Grue to describe *two properties* of the emerald in question.

First he takes him to say that *a* is green, and with this he can agree (In ch. V I hope to show that Green's ground for this agreement is not sound either. His argument is purely extensional: from the fact that '$t_1 < t_0$' and '*a* is green' are true, he concludes that the first conjunct, i.e. '$t_1 < t_0 \rightarrow a$ is green', is true and I do not think that any serious empirical conditional is accepted as true solely on such an extensional basis. For the time being, however, I shall not complicate matters by introducing this point here).

Second, he takes him to describe *a dispositional property* of that emerald, viz. 'blueable', that is 'turning blue at t_0'. There is no mystery connected with such a description. The scientist, who takes 'if this piece of copper is heated then it expands' as a true description of this piece of copper, which is de facto not heated, simply says, that this piece of copper is expansible or that it has the

dispositional property of being expansible. (This scientist might as well have formulated his description by 'if this piece of copper were heated then it would expand' or by 'if this piece of copper had been heated then it would have expanded'). To take yet another example, if we say of a lump of sugar 'if it is immersed in water then it dissolves', we say that that lump of sugar is soluble. There is nothing strange about our ascribing a dispositional property to a thing, except that other than instantial evidence is required to confirm it. Nor is there anything strange about the ascribing of two properties at once to a thing, one of which is dispositional ('green' is also dispositional as we argued in ch. I, section 6, but we shall leave this out of the discussion for the moment, although we shall certainly come back to it). We can, for example, say (it is the 16th August in Holland), that that oak-leaf over there is green and 'brownable', i.e. 'turning brown in autumn'. We could express this by using a single word to indicate both predicates, e.g. 'that oak-leaf over there is grewn' (MARY HESSE referred to beech leaves and others to leaves in general).

These examples only serve to elucidate in what sense Mr. Green now understands what Grue says with his description of the emerald.

And Green cannot accept Grue's description as true as long as he is ignorant of the required other evidence, Grue must have in Green's eyes, in order to justify his ascription of a dispositional property to emerald a, as it is expressed in the counterfactual part of the L_1-translation of Grue's statement 'a is grue'. Therefore the generally accepted presupposition that there is complete agreement between Green and Grue at the 'singular level' about the truth of their singular descriptions of emerald *a* must be dropped. Green will ask Grue for the other evidence that he must have *from Green's point of view*. This point of view may be said to be determined by his language, L_1, but it would be more correct to say that it is determined by the conceptual system of Mr. Green, which can be expressed by L_1. This will become clear if we show another consequence of what has been said thus far. The presupposition that both, Green and Grue, agree that the predicates 'green', 'blue', 'grue', and 'bleen' are colour concepts (the second assumption formulated in section 1), cannot be upheld either. As far as Mr. Green knows the meaning of 'grue', and his knowledge of the meaning is completely exhausted by his definition (L_1–1), this concept has a dispositional meaning aspect which is not entailed by his own colour concepts. Within his conceptual system Green cannot understand 'grue' as a colour concept, because in that system a colour concept has no dispositional trait determined by the circumstance 'time', albeit that it has such traits determined by the circumstances 'sufficient light' and 'kind of light'. Even stronger, the factor 'time' can by itself never constitute a basis for a dispositional trait of a colour concept within Green's conceptual framework. Reference to a certain time would always be faced as a purely ad hoc escape, as was explained in ch. I, section 6, unless empirical evidence forced us to renounce the classical concept of change in connection with the colours of things, in which case the colour of a thing would be a 'fundamentally statistical property'. And also, in that case, it is Grue's other empirical evidence Green is waiting for. If Grue cannot reveal the

other evidence that he must have according to Green, Green cannot but suppose Grue to have the eyes of Big Brother through which he can observe how things are (will be, would be, would have been) in the Orwellian era.[61] Green cannot *understand* Grue when he says that he does not imply a colour change at t_0 (compare the third assumption of section 1). Within Green's conceptual system, expressed in L_1, Grue's singular statement '*a* is grue' *does* imply a colour change at t_0.

We started this analysis with the question: "Can Green really agree with Grue that '*a* is grue' is true?" and this analysis has shown, I think, that Green cannot simply do this. But, as we remarked when introducing this question, the matter is completely analogous for Grue judging the truth of '*a* is green'. Our analysis does not lead us to an asymmetry between 'green' and 'grue'; it does not make us choose between L_1 and L_2. This becomes clear when we hear Grue's response to the view Green arrived at after having been convinced of the untenability of his earlier, purely extensional view.

Grue's answer may be formulated as follows:

> "If you ask me for other evidence in order to show that '*a* is grue' is true, I have not the faintest idea what you could mean. It is all very simple: the colour of that emerald is grue, can't you see that? Moreover, I can only accept your description as true if you give me evidence for your statement that *a* is 'bleenable', which is, as far as I know, implied by your saying that this emerald is green. And I cannot possibly understand your concepts 'green' and 'blue' as colour concepts. If you cannot give me such other evidence, I must assume that you have the eyes of Big Brother".

In short, there is a serious problem at the 'singular level' and it seems to be *a conceptual problem rather than a problem of induction in Goodman's sense.* Green and Grue have arrived at *a rivalry of (part of) their conceptual systems,* it seems.

But before making this more precise in the next section, we must face two possible objections against our analysis.

First, it might be remarked that the definitions (L_1–1) through (L_2–2) are, after all, not of *a suitable form,* just because they lead to a problem about the truth of the singular descriptions of emerald *a*. In this case, my interpretation of these definitions, which amounts to a specific dispositional character of Goodmannian predicates (viz. 'grue' and 'bleen' in L_1 and 'green' and 'blue' in L_2), is not questioned. It is only said that they are not the correct definitions of the predicates GOODMAN intended.

I think that this is not a sound objection. For suppose we try to remain as close as possible to Goodman's own circumscription of 'grue' and that we define:

$(x)\{(x \text{ is grue}) \equiv (Ex.Gx) \vee (\sim Ex.Bx)\}$
in which E = examined before t_0
G = green
B = blue

We can then tell the same story as the one told above, using this definition

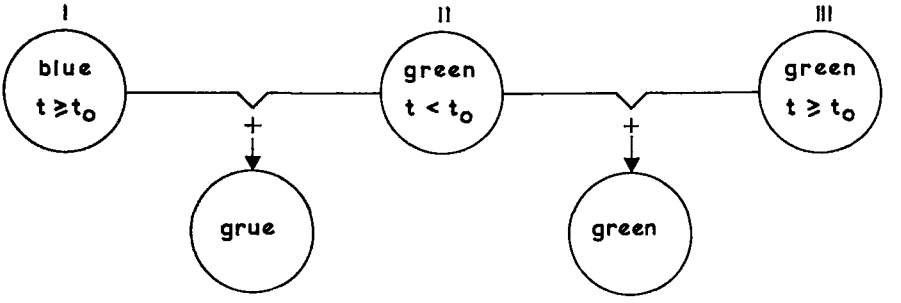
of 'grue' and I do not think that one could object to this definition. (Of course, this definition may be equivalently reformulated as

$(x)\{(x \text{ is grue}) \equiv (Ex \rightarrow Gx).(\sim Ex \rightarrow Bx)\}$

but this is completely inessential.) So, I do not think that it makes much sense to discuss the form of the definitions any further.

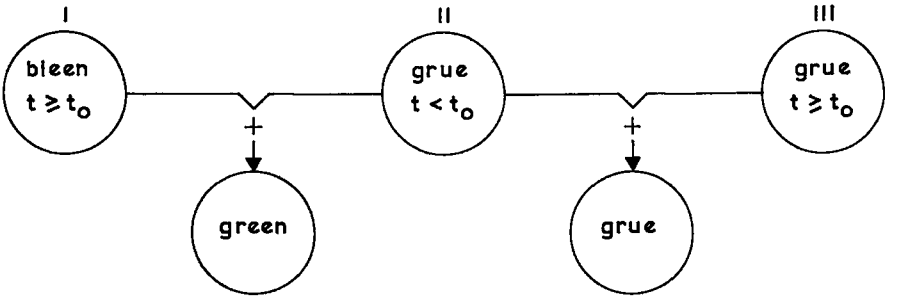
Second, one might object that my *interpretation* of the definitions is not correct and this is a much more important point. With ULLIAN, one could say that "One extension is as good as another for a class qua class, no matter how much (or how little) its description may cut across the boundaries of our ordinary classification. Unless we privilege special classes – and logic alone cannot allow us to do this – there is no hope of distinguishing those extensions which may be taken as belonging to bona fide predicates".[62]

In other words one must view e.g. 'grue' purely as the name of a class of things. 'grue' is the label of the collection of all those things which are green before t_0 and of all those things which are blue at t_0 or thereafter. And 'green' is the label of the collection of all those things which are green before t_0 and of all those things which are green at t_0 or thereafter. It is as simply as this: 'grue' is the sum of two classes, just as green is the sum of two classes. This, at least, is Green's view when he objects to my interpretation. This view may be pictured as follows:



The Green-Picture

Grue's view may be pictued as follows:



The Grue-Picture

Green and Grue are now again members of the extensionalist camp of GOODMAN and ULLIAN, of course. They were not convinced by my analysis. And in Green's view, for example, one extension (e.g. I + II) is as good as another (e.g. II + III) for a class qua class, so that we must appeal to extra-logical factors in order to distinguish between these extensions in such a way that we may prefer the one to the other, living in a pre-Orwellian period. And as long as one defends a 'super-extensionalism', as Goodman does, the only extra-logical factor one can have recourse to is entrenchment. For, the being more entrenched of a predicate in comparison with a rival predicate means that it has the largest extension (via past projections, parent predicates, etc.). Within such a purely extensional interpretation of 'grue' it is enough for Green to establish that a thing, e.g. emerald *a*, belongs to II of his picture, in order to verify '*a* is grue'. No problem about a dispositional character arises and therefore there is no problem at the 'singular level', it seems.

This objection, however, which attempts to rehabilitate Goodman's problem as a riddle of induction, does not, I think, stand critical examination.

To see this, we confront both extensionalists, Green and Grue, with the question: "What would, according to you, the colour of emerald *a* be, if the time had not been t_1, but greater than t_0, provided that all relevant circumstances had, at that other time, been the same as they are now?"

Les us sketch the situation explicitly.[63] Both Green and Grue have examined emerald *a* and checked the time, t_1.

Green says:	*a* is green	(i)
Grue says:	*a* is grue	(ii)
Both say:	$t_1 < t_0$	(iii)

Both accept on the basis of their pictures: (i), (ii) and (iii) are true. Now suppose,

contrary-to-fact:	$t_1 \geqslant t_0$	(iv)

Both reject (iii) upon the supposition as true. But they must, when (iv) is supposed to be true, also reject one of (i) or (ii). And our question to Green and Grue comes down to: "Which of these two statements would you reject or maintain?"

> Green's answer is this: "I would maintain (i) and thus reject (ii). If you suppose that all relevant circumstances were the same at that other time as they are now, then you in fact say that no single event, that could possibly cause a colour change, would occur. And your question becomes something like this "What would be the colour of this green emerald at a time greater than t_0 if nothing were to occur that could possibly change its colour?" and this, in its turn, comes down to "What would be the colour of this green emerald at a time greater than t_0 if it would remain green?", at least in my view. And the answer to this question is simply that it would be green".

Grue's answer is, of course, completely symmetrical: he rejects (i) and upholds (ii) on extactly analogous grounds.

One may object, that it was in fact not Green or Grue, who gave the answer, but I. I made them reason in the way they did. I made them appeal to the lawlike trait of their respective colour concepts, which governs a lawlike constancy

behaviour. And I let them reduce the question I asked them to a tautological question, by indoctrinating them with the classical concept of change, which I discussed in ch. I, section 6. In fact, I did not allow them to remain the extensionalists they were at the beginning. Now, this is all true, but I think there is no other possibility left for them. I think they are forced to give an answer of the kind given a moment ago. Or course they could have given it more vaguely. They could, for example, have confessed their belief in a 'constancy of nature' or they could have said that a thing remains the same colour if 'nothing happens', etc.

However, I do not think that they can stick to their purely extensionalist view of their respective colour concepts as it was sketched in their respective pictures. The answer I let them give is the only *understandable* answer. If Grue, for example, had rejected (ii) and upheld (i), not only the symmetry would have been destroyed, as has frequently been remarked in the literature, but he would also have claimed a colour change at t_0, which would not be explainable in principle within his conceptual scheme, in which the classical concept of change is supposed to be applicable in the case of a colour change. It would be a completely ad hoc manoeuvre without any possible empirical foundation.

Grue rejects (i) precisely because he does not want to relinquish his understanding of colour phenomena. He cannot simply drop the built-in lawlikeness of his concept 'grue', as he would be obliged to do if he rejected (ii) instead, because this would have implied a severe shock to his conceptual system of colour concepts, through which he could observe and explain or understand all colour changes up till now. And he has not met with empirical data that may force him to do so. But it is precisely this lawlikeness, which constitutes the basis for Grue's saying that emerald *a* would be grue at a time greater than t_0, ceteris paribus, which at the same time, in Green's language, makes the aforementioned dispositional character of 'grue' reappear. When Green hears Grue say that that emerald would be grue at a time greater than t_0, he hears him say that it would be blue at that time. And for the same reason that Grue had to maintain (ii), Green upholds (i): that emerald *a* would be green at a time greater than t_0, ceteris paribus.

When we now return to the situation, explicitly stated before, and ask Green whether or not he can accept at t_1 ($t_1 < t_0$) Grue's description '*a* is grue' as true, his answer must be:

> "No, I cannot now accept '*a* is grue' as true, because this not only means that *a* is green now, but also that it is (will be, would be, would have been) blue at a time greater than t_0. As far as I know what 'grue' means, this concept implies a dispositional character that may be named 'blueable'. And the first time you tried to convince me of this dispositional character (at the beginning of this section), I said that I needed the other evidence, Grue must have in my view, before I could judge the truth or falsity of '*a* is grue'. But now the situation is different, since you introduced the proviso 'provided that all relevant circumstances would at that other time, greater than t_0, be the same as they are now' in the question you posed to Grue and

me. And, given this proviso, I cannot leave the question whether or not '*a* is grue' is true unanswered until I have inspected the other evidence, because the 'ceteris paribus' excludes the possibility of there being such other evidence. If all possibly relevant circumstances should remain unaltered, then that emerald remains green and it is then plainly false to say that it is 'blueable'. *Therefore it is simply false to say that it is grue now*".

And again Grue will give the symmetrical answer at t_1 ($t_1 < t_0$) to the question with respect to '*a* is green': he not only cannot accept this statement as true, but he must say that it is false.

The symmetry is not destroyed by the considerations of this section: there is complete symmetry in understanding and in *non-understanding*. This non-understanding is the root of the problem at the 'singular level'. It seems that Green has a green-blue theory of colours, formulated in L_1, and Grue a grue-bleen theory expressed in L_2, and the fact that Green (Grue) calls Grue's (Green's) description of the colour of emerald *a* false, while Grue (Green) calls it true, makes it clear, that they do not understand each other's colour theory. In the next section we shall go further into this conceptual problem.

There is no riddle of induction now, neither for Green nor for Grue in their respective languages, L_1 and L_2. From Green's point of view, '*a* is grue' is false and 'grue' is not a rival of 'green'. Green cannot accept 'grue' as an empirically confirmed concept (compare for this notion of confirmation sections 5 and 6 of Ch. 1), let alone find himself confronted with the problem of choosing between 'all emeralds are green' and 'all emeralds are grue'. As long as the mentioned conceptual problem has not been solved, the new riddle of induction cannot even be formulated in either language (perhaps it ceases to be a problem once the conceptual gap has been bridged!). Of course Green arrives at 'all emeralds are green' and Grue at 'all emeralds are grue', but the problem of making a choice between these statements is the same as the problem of choosing between two colour theories.

In concluding this section, let us suppose, just for the sake of argument, that the original Mr. Green (and with him all those who let him argue in that way) was right and that he indeed could call '*a* is grue' true on an extensional basis. This would imply that he could have used the term 'grue' in all those cases where he as a matter of fact used 'green' up to now. In particular 'grue' could have been used in all those cases in which 'green' was projected or in all those cases which made 'green' the veteran in comparison with 'grue', according to GOODMAN.[64] In other words, 'grue' could as well have been the veteran and 'green' the newcomer. As a matter of fact 'green' and 'grue' share exactly the same entrenchment, like 'green' and the German 'grün'. Therefore entrenchment cannot possibly be a criterion for distinguishing 'green' from 'grue'.

Of course this is only valid if one is liable to accept '$t_1 \geqslant t_0 \rightarrow a$ is blue' as true on the basis that '$t_1 \geqslant t_0$' is false. For in that case '$t_1 \geqslant t_0 \rightarrow a$ is blue' is, at a time prior to t_0, an empirically empty addition and 'grue' is nothing more, empirically, than the translation of 'green' in a, thus far, unknown language, L_2. So,

in a pre-Orwellian era and facing the matter extensionally, 'grue' and 'green' acquire exactly the same entrenchment.

GOODMAN cannot accept this, but, as far as I can see, he should then discern the non-truth-functional analysability of '$t_1 \geqslant t_0 \rightarrow a$ is blue' as the source of his not being prepared to do so. And this implies that he must acknowledge the dispositional character 'grue' has in Green's eyes, which brought us to the conceptual problem of 'green' and 'grue'.

3. THE CONCEPTUAL PROBLEM OF 'GREEN' AND 'GRUE'

Suppose that the analysis of section 2 is accepted as correct, as I think it is, what then, exactly, is the conceptual problem we arrived at?

It is this:

On the one side, there is Mr. Green with his green-blue theory of colours through which he can observe coloured things. His theory is the appropriate method to do this. With this theory he can also explain all phenomena of colour change he knows of. The lawlikeness of his colour concepts enables him to explain the behaviour of coloured things, be it that they remain the same colour or that they change colour. He can observe and explain these phenomena by doing normal science. He knows what are the suitable or normal instruments: normal organs of sight are required, the wearing of violet sun-glasses is forbidden but the wearing of normal (!) spectacles or contact lenses will be imperative in some cases etc.; and he knows what relevant circumstances are: things should be illuminated with white light of a minimum intensity. He does not view his concepts as isolated units of (primitive) meaning, but as concepts that are those very concepts by being part of his colour theory. We shall, in future, refer to these concepts by using the words 'green-1' and 'blue-1'. Then there are those artificial constructs, 'grue-1' and 'bleen-1', introduced in his language, L_1, by means of the definitions (L_1–1) and (L_1–2). But as they are constructed by these definitions, 'grue–1' and 'bleen-1' are mere symbols in a system of other symbolds, namely L_1. Green can only view them as possible empirical concepts by taking them to be conjectural concepts in the following sense. To say of a thing that it is, e.g. grue–1, means that it has the dispositional property of being 'blueable', i.e. turning blue under certain circumstances which come to be realised at t_0, while it is green before t_0. Green does not know anything about these 'certain circumstances' and he has therefore not the faintest idea of what the property of 'blueability' could be. But some new relevant circumstances may turn up and he cannot on the basis of his conceptual system exclude this possibility. Just as he might have accepted the concept 'grewn' as a confirmed empirical concept within his theory and have applied it to those oak leaves over there, but not to the leaves of the birch in my garden (they are grellow!), he might, at this moment, accept 'grue–1' and 'bleen–1' as candidate concepts. They may acquire the status of empirical concepts within his system upon further inquiry and theory formation.

Briefly, if Green wants to leave open the possibility of 'grue-1' and 'bleen-1' becoming empirical concepts in his green-blue theory, he must say that they entail a dispositional trait (or a lawlike trait), which has not yet been confirmed, as such, as far as he knows, and which therefore makes these concepts conjectural.

On the other side, there is Mr. Grue with his grue-bleen theory of colours through which he can observe coloured things, etc. etc. His primitive colour concepts shall be named 'grue–2' and 'bleen–2' and the constructed terms 'green–2' and 'blue–2'. These constructed terms are taken as candidate concepts by Grue. The question, in what sense 'grue' and 'green' can be said to be rival or conflicting concepts, should now be split up into two questions: one about the rivalry between 'green–1' and 'grue–1' and the other about the conflict between 'green–1' and 'grue–2'.

Let us first consider the couple 'green–1' and 'grue–1'. I do not think that there could be a conflict, which would be a conflict within L_1, between these two concepts.

Suppose Mr. Green–1 and Mr. Grue–1 examine emerald *a* at time t_1, prior to t_0.

Green–1 says: *a* is green–1
Grue–1 says: *a* is grue–1

By being speakers of L_1 or, more correctly, by being defenders of the green-blue theory, they agree on the follwoing points:

first, they both say that 'green–1' implies 'not blue–1', because this is a lawlike trait of those concepts (ch. I, section 4)[65];

second, both use the classical concept of change, so that every colour change must have a cause or that every colour change must be accompanied by a change of one or more relevant circumstances (ch. I, section 6);

third, and connected with the second point, both acknowledge the empirically confirmed lawlike character of 'green–1', which governs a lawlike constancy behaviour of green things (compare ch. I, sections 5 and 6);

and, finally, both say that 'grue–1' implies the disposition of 'blueability'.

As a consequence, both agree that Green–1's description implies:

a is (will be, would be, would have been) green–1 if t_1 is (will be, etc.) equal to or greater than t_0 *and ceteris paribus.*

And both agree that Grue–1's description implies:

a is (will be, etc.) blue–1 if t_1 is (will be, etc.) equal to or greater than t_0. (This is based on the dispositional character Grue–1 claims for 'grue–1').

Now, Green–1, facing 'grue–1' as a candidate concept, asks Grue–1 for the evidence for that dispositional part of 'grue–1'. He asks for the beforementioned 'other evidence' (section 2), Grue–1 must apparently have, so that he can judge whether 'grue–1' can be awarded the status of an empirically confirmed concept within his green–blue theory. He asks for evidence of the kind that can be given for the turning yellow of, e.g. white things when they are illuminated with sodium light or for the turning grey of coloured things upon darkening,

or of the kind that can be given for the 'brownability' (the turning brown in autumn) of oak leaves.

So, Grue–1 should do one of two things. First, he might show that there is a relevant circumstance for a thing's being green–1, not discovered by Green–1 up to now (as was the relevant circumstance of there being sufficient light), and that this circumstance shows its effect at t_0 and thereafter.

Second, Grue–1 might show, that, for example, emeralds change their lattice structure under certain circumstances, so that they only reflect light of a blue–1 wavelength, and that these circumstances shall be realised at t_0 and thereafter.

Up to now Grue–1 has not revealed anything of this kind to Green–1, but even if he had, there would, in neither of the two cases, be a conflict or a rivalry between 'green–1' and 'grue–1'. Green–1's prediction (or counterfactual statement) of emerald *a* being green–1 in the Orwellian era, was only taken as true under the ceteris paribus proviso. This proviso will be violated if Grue–1 could succeed in doing one of the two things suggested above.

Of course, our examination of 'green–1' vs. 'grue–1' within L_1 introduced an asymmetry at the very beginning, by viewing 'grue–1' as entailing a dispositional trait, which had not yet been confirmed and which was not entailed by 'green–1'. Therefore no rivalry could arise. We must now face a possible conflict between 'green–1' and 'grue–2'.

On the one side, there is Mr. Green (the same one as Mr. Green–1) with his green-blue theory, expressed in L_1, and on the other side, Mr. Grue (not to be identified with Mr. Grue–1) with his grue-bleen theory, expressed in L_2. In L_1 'green–1' and 'blue–1' are primitive colour concepts (but not isolated or theory-independent units of extensional meaning) and 'grue–1' and 'bleen–1' are candidate concepts, constructed in terms of 'green–1', 'blue–1' and a temporal term.

In L_1 'grue–2' and 'bleen–2' are colour concepts and 'green–2' and 'blue–2' are candidate concepts, constructed in terms of 'grue–2', 'bleen–2, and a temporal term.

Green and Grue examine emerald *a* at time t_1, prior to t_0.

Green: *a* is green–1

Grue: *a* is grue–2

Green implicitly predicts (or counterfactually states – we shall here restrict ourselves to predictions):

a is green–1 in the Orwellian era, ceteris paribus (I).

Grue predicts: *a* is grue–2 in the Orwellian era, ceteris paribus (II).

(We assume here, that 'green–1' and 'grue–2' both have a lawlike character, which makes these predictions implied by their mutual descriptions).

The only thing Green knows of 'grue–2' is 'grue–1' and he identifies them. So does Grue with 'green–1' and 'green–2'.

These identifications are crucial: they bridge the gap between L_1 and L_2 as the formulations of two colour thories.

Upon his identification, Green reads Grue's prediction (II) as:

a is blue–1 in the Orwellian era, ceteris paribus (III).

And (III) clearly conflicts with his own prediction (I).

Analogously, Grue reads Green's prediction (I) as:

a is bleen–2 in the Orwellian era, ceteris paribus (IV).

This contradicts Grue's prediction (II).

(We assume here that 'green–1' implies 'not blue–1' and that 'grue–2' implies 'not bleen–2').

Now the conflict is there:

Green takes his description, and therefore the implied prediction (I), as true. As a consequence he must call Grue's prediction (II), in his translation (III), and hence Grue's description of emerald *a*, false.

Analogously, Grue must call Green's description of that emerald false.

This is a conflict about singular descriptions and, I think, it is a symptom of a conceptual gap between the green-blue theory and the grue-bleen theory. This gap may be taken to be bridged by the definitions in L_1 for Green and the definitions in L_2 for Grue, but this is only apparently so. This will become clear if we take it seriously that L_1 and L_2 express *alternative colour theories.* Grue says of 'grue–2' that it is a colour concept within his theory. Green identifies 'grue–2' with 'grue–1'. But 'grue–1', as it is defined in L_1, and thus 'grue–2', is not a colour concept within Green's theory, but a mixture of a colour concept and a dispositional concept, determining a colour change at t_0, without any change of relevant circumstances. It is a concept which is not compatible with Green's colour theory: it cannot be understood by Green together with his colour theory.

Grue may remark, that he agrees with Green's identification of 'grue–2' with 'grue–1', but that nevertheless 'grue–2' (or 'grue–1') is a perfect colour concept, not implying a change of colour at t_0, ceteris paribus. And in spite of his definitions, Green is completely in the dark about what Grue could possibly mean by 'colour'.

Of course, the same holds for Grue's definitions of 'green–2' and 'blue–2'.

There is complete symmetry in non-understanding. The problem is not a problem of induction, but a problem of concept and theory formation. Green, to take him as our example, must learn to observe in the way Grue can observe. In other words, *he must learn to observe in a way directed by the alternative grue-bleen theory, before he can see what Grue sees.* He must form Grue's concepts. This is not an easy job for Green: he must learn to observe in familiar things what he could not see until now and his own colour theory is a severe hindrance to do this. He will need the continual assistance of Grue. He will have to dissolve necessary connections of his own theory and there will not be much left of it in the end.

Once Green has succeeded in forming the alternative theory, the question of which one should be preferred arises. And then "other considerations should certainly be given priority over entrenchment", as MARY HESSE remarked.[66] After he has learned to observe as Grue observes, Green may return to his own theory, because it is less ad hoc or because it explains the colour phenomena

better than its rival, which does not achieve anything that could not be achieved by the green-blue theory. But the contrary may also happen: Green may prefer Grue's theory, because it enables him to observe and explain many new phenomena, which could not even be seen through the 'old' theory. Up to now, Green had not met a Mr. Grue, to tell him the story of 'grue' and 'bleen' or to teach him the grue-bleen theory and as long as the situation in which two radically alternative colour theories actually exist, is not realised, Goodman's problem is spurious.[67] In particular, it is spurious in connection with the question of what empirical laws are, because this question is posed within an (at that moment) accepted conceptual framework, as we shall see.

And if we really find ourselves confronted with two rival theories, Goodman's problem as a problem of induction or of confirmation by positive instances does not arise either. Within Green's theory, a green emerald may be taken as a positive instance of 'all emeralds are green' and likewise, within Grue's theory, a grue emerald may be called a positive instance of 'all emeralds are grue' and, if positive instances confirm, and taken in isolation they cannot do this, as I argued in ch. I, section 6, then they confirm in both cases. *Green and Grue observe different things by observing through alternative conceptual systems.* Green's and Grue's observations are not the same in such a case, as GOODMAN has taken them to be. The problem which then arises is the problem of choosing between the rival theories and entrenchment cannot help us a bit in making such a choice "and to put emphasis upon it without investigating the full theoretical ramifications of the puzzle itself, is to betray an unacceptably restricted and conservative view of the progress of science".[68] The problem of theory-choice, and thereby of theory-formation, is not Goodman's problem, because that problem presupposes that Green and Grue, on the basis of *the same observations*, arrive at contradictory predictions, while Green and Grue, being defenders of alternative theories, arrive at contradictory predictions and descriptions *by observing different things.* It even remains to be seen whether their predictions and descriptions were in fact contradictory. Even this cannot be decided in advance. They may, in the future, appear to be compatible: the 'old' theory may, for example, become a special case of the, wider, 'new' theory.

If we take Goodman's problem seriously, it says that there may always come a new and perhaps better theory than the one we have now. As long as such a theory has not been revealed to us, no new riddle of induction arises and as soon as such a theory is there we should not judge the newcomer by its being inductively confirmed, but by its explanatory power, its (lack of) ad hocness, etc.

We should conclude, I think, that Goodman's riddle, in Goodman's interpretation, has no bearing whatsoever on a philosophy of science.

III. THE REGULARITY VIEW

1. THE REGULARITY VIEW AND SOME SYNTACTICAL-SEMANTICAL DIFFICULTIES

When discussing the regularity view, R, we must keep in mind the fact, that this view has been offered as an answer to the question of what *experimental* (NAGEL; CARNAP: empirical; SCHEFFLER: observational) laws of science are. The proponents of R presuppose a distinction between experimental laws and theories, which fundamentally goes back to the distinction between observational and theoretical terms. Experimental laws contain only observational terms with a fixed, theory-independent meaning and are therefore capable of direct confirmation by positive instances, while "a theory cannot be put to a direct experimental test"[69] because it contains not only observational terms but also theoretical terms, for which no direct observational procedure is available in order to identify their referents. As a consequence the meaning or truth (or falsity) of experimental laws is independent of the theory under which they may be (temporarily) subsumed: an experimental law may survive the eventual demise of this theory (compare note 8).[70] Although every logical positivist admits that the distinction between observational and theoretical terms is vague, and therefore also the one between experimental laws and theories, it is nevertheless taken to be of great importance.[71]

The regularity view then, constitutes the logical positivist answer to the question what experimental laws, as distinct from theories in a logical positivist context, are (compare ch. I, section 1, thesis iv). It is the answer to the question what laws of science in the observational language, L_0, are. This must be kept in mind when we discuss R.

As a first approximation, we may say that, according to R,

> an experimental law is a true synthetic statement of universal form, asserting a regularity without there being any exceptions.

We can elucidate this with some quotations.

According to REICHENBACH, laws (we omit the addition of 'experimental') are "statements about repeated occurrences, including the prediction that the same combination will occur in the future, without exception" [6] or it is "a relation of the form *if...then*, with the addition that the same relation holds at all times".[72]

AYER expresses his agreement in the following way: "a proposition expresses a law of nature when it states what invariably happens. Thus, to say that unsupported bodies fall, assuming this to be a law of nature, is to say that there is not, never has been, and never will be a body that not being supported does not fall."[73]

HEMPEL states: "The laws required for deductive-nomological explanations share a basic characteristic: they are, as we shall say, statements of universal form. (..) (That) is a statement to the effect that whenever and wherever condi-

tions of a specified kind F occur, then so will, allways and without exception, certain conditions of another kind, G".[74]

And finally BRAITHWAITE: "Scientific laws will be taken as asserting no more (and no less) then the *de facto* generalizations which they include; the law that every hydrogen atom consists of one proton together with one electron will be interpreted as meaning that, as a matter of fact every hydrogen atom, past, present and future, has this constitution."[7]

When describing their views, the proponents of R frequently add a remark about the necessity of a law. So REICHENBACH: "We do not wish to say that physical necessity is due to invisible forces tying things together, or that it is a priori, or that it is a law of reason projected into nature, or whatever else has been subtly devised by certain metaphysicians. (..) 'Physical necessity' is expressible in terms of 'always'."[6] And AYER: "The 'necessity' of a law consists, on this view, simply in the fact that there are no exceptions to it."[75] BRAITHWAITE also rejects the necessity view by stating that a law does not assert *more* than a de facto generalisation (compare the quotation above).

Every possible concept of necessity is expelled, unless it means nothing more than a *constant conjunction* of certain events or characteristics.

In its most simple form a law may be expressed by

all A's are B's

or: (x) (Ax→Bx).

The symbols 'A' and 'B' indicate descriptive terms like 'metal' 'expand upon heating' or 'water' and 'boiling point of 212° F.'

The major problem that proponents of R recognize is not the truth of such a statement, nor its synthetic character, nor the concept of regularity that is used, but *the distinction between lawlike and non-lawlike or accidental universals.* This may be elucidated by an example. When we compare the following two statements, which are both supposed to be true:

all metals expand upon heating (I)

all screws in Smith's car are rusty (Nagel's example) (II)

we could say that the first statement is a law, but that the second is not a law, if we at least presuppose that both are true. Both statements, being the result of a 'synthetic connective operation' (REICHENBACH), are of universal form and both assert a regularity without exceptions or a constant conjunction. What, then, is our criterion when calling the first statement a law, the second not?

The proponents of R should provide a criterion for demarcating those universal statements, which are laws if they happen to be true (the *lawlike universals* [76]), from those universals which are not laws even if they appear to be true (the *accidental universals*). (I shall invariably use the term 'lawlike'. Other terms are 'nomic' and 'nomological').

That the search for such a criterion of lawlikeness is in fact the central problem in a logical positivist analysis of the concept of law means that concepts such as the synthetic character of a law and the assertion of an exceptionless repetition are taken to be clear enough. The concept of regularity fares badly in modern literature. The use of expressions like 'uniform connection' (HEM-

PEL) and 'constant conjunction' (NAGEL) and the appeal to common sense leaves the impression that this concept is considered to be quite clear.

The analytic-synthetic distinction is still taken for granted, even after Quine's severe attack.

I shall come back to both these points later, but we must first say something more about the attempts to formulate a criterion of lawlikeness. This problem has been said to be intriguing, delicate and recalcitrant[77] and some of the attempts to solve it will be considered in this section.[78]

The first attempt to solve some of these problems, can be formulated as follows: *a universal conditional is lawlike when it contains no individual constants, i.e. terms referring to a particular object or a specific spatio-temporal area.*[79] The statement about the rusty screws, (II), contains an individual constant, namely 'Smith's car', and must therefore be qualified as accidental, while (I), the statement about the metals, does not contain such a term, and may therefore be called lawlike.

This proposal has been amended, since individual constants may not occur *essentially* or in an *uneliminable* way.[80] Otherwise we would get the strange situation, that (I), not containing individual constants, would be lawlike, whereas an equivalent reformulation of (I), e.g. 'all metals in my room or elsewhere expand upon heating', would be accidental. We do not arrive at this result when we speak of the essential or uneliminable occurrence of individual constants: the individual constant 'my room' in our example can be eliminated without change of meaning, according to PAP.

(REICHENBACH has chosen an alternative approach by defining 'universal' by the absence of individual constants and then requiring that a statement must be universal in order to count as a fundamental lawlike statement.[81] I shall come back to the addition of the word 'fundamental' in a moment).

(When GOODNAM, and AYER follows him in this respect, rejects the criterion of the non-occurrence of individual constants, he rejects it in its unqualified sense. In other words, he did not reject the criterion in its amended form in which the essential or uneliminable occurrence is used[82]).

This amended criterion meets with two difficulties. First, there seem to be universal statements which, containing individual constants essentially, are nevertheless regarded as lawlike, e.g. Kepler's first law and Galileo's law of freely falling bodies. Second, all individual constants, which at first sight seem uneliminable, appear nevertheless to be eliminable.

To eliminate the first difficulty, REICHENBACH made a distinction between original and derivative nomological statements[83] and HEMPEL, following him, speaks of fundamental and derivative lawlike statements.[84] Fundamental lawlike statements must satisfy the requirement that no individual constants occur essentially, but in derivative lawlike statements such constants may occur and they are lawlike because they can be derived from fundamental lawlike statements.[85] According to this view, Kepler's and Galileo's laws are lawlike, be it derivatively, because they can be deduced from Newton's laws and his theory of gravitation. But NAGEL and PAP have drawn attention to the fact that we

need additional premises for the deduction of e.g. Kepler's first law from Newtonian mechanics, and these additional premises essentially contain individual constants (e.g. about the relative masses and velocities of the planets and the sun). And if we do admit such additional premises in one case, why should they not be admitted in the case of the statement about the rusty screws, (II)? (II) is lawlike since it can be deduced from the fundamental lawlike statement 'all iron screws rust when exposed to air' and the additional premises 'all screws in Smith's car are iron' and 'all screws in Smith's car are exposed to air'.[86] Although HEMPEL considered this objection, he did not answer it in an acceptable way[87] and LAUTER, when expounding Reichenbach's criterion of lawlikeness[88], leaves this point undiscussed.

But even if this objection of NAGEL and PAP is invalid, the appeal to Reichenbach's distinction would not be of much help, a point raised in Nagel's remark that Kepler's and Galileo's laws were lawlike before the time of Newton.[89]

The second difficulty concerning the criterion of lawlikeness under discussion has been formulated by different authors.[90] It seems that we can *always* eliminate the individual constants occurring in a universal statement. We may, for example, reformulate (II) as:

all scarscrews are rusty

where 'scarscrew' uniquely refers to a screw in Smith's car. This statement does not contain an individual constant and is therefore, according to the present criterion, lawlike. But then (II), being equivalent to this statement, must also be lawlike. By using an analogous strategy, every statement whatsoever would be lawlike. One might protest that this is only a pseudo-elimination: we merely construct a new term, 'scarscrew', which is defined as 'screw in Smith's car', so that the individual constant is no longer overt, but hidden, and by hiding it we have not eliminated it.

This objection is fully justified and leads to a broader and less artificial attack on the problem. A new criterion comes to be formulated on the basis of different kinds of predicates.

A second attempt has been formulated: *a universal statement is lawlike if it contains only purely qualitative or non-positional predicates.*[91]

A predicate is purely qualitative when the explication of its meaning does not require the reference to a specific spatio-temporal area or a particular object.[92]

This criterion led to several objections. First, it has to face the objection that Kepler's and Galileo's laws (we shall restrict ourselves to these standard examples) should be called accidental rather than lawlike. An appeal to the Reichenbachian distinction between fundamental and derivative lawlike statements does not enable us to avoid this objection either.

Second, it may be objected that the notion of a purely qualitative predicate is very vague, because it makes use of 'the meaning' of a predicate, and we can hardly pretend to elucidate the concept of law on the basis of such a vague criterion. This objection was also raised by HEMPEL and GOODMAN.[93] HEMPEL, however, simply begs the question by offering a definition of an experimental law relative to a formal language L, in which each term is either primitive or defin-

able in terms of such primitives. And this is followed by his remark that these primitives "will be assumed to be qualitative in the sense just indicated" (that is in the sense quoted in note 92). But this manoeuvre is, to borrow Quine's expression, a 'feu follet par excellence'. We cannot solve the problem of law-likeness by constructing a language in which the original problem cannot be formulated any longer.

Now, the force of this second objection depends of course on our position regarding 'meaning as intention'. Like POPPER, one might be satisfied with the explication of the distinction between positional and non-positional predicates or between individual and universal concepts (to use Popper's terms), in which case this criterion would be free from this objection. But others, in particular QUINE and GOODMAN, are allergic to such 'meanings', which "as obscure intermediary entities, may well be abandoned".[94] In Goodman's terms the introduction of "ghostly entities called 'meanings' that are distinct from and lie between words and their extensions" is superfluous, because he thinks "that difference of meaning between any two terms can be fully accounted for without introducing anything beyond terms and their extensions."[95] The appeal to the meaning or intention of a predicate is completely excluded in this view, because there are no meanings to which one could eventually appeal. So, from this point of view the objection to the proposed criterion is a very severe one. As a matter of fact, I think that the proponents of R, defending their thesis within a logical positivist context, should indeed view this objection as disastrous.

It should be clear from the rough sketch of the logical positivist context, given in ch. I, section 1, that an appeal to the meaning of a concept, taken as its intention, is an inconsistent manoeuvre in that context. We have already shown that at the observational level – and we are still discussing laws at an experimental (observational) level – predicates must be characterized as isolated units of extensional meaning, and in my view QUINE and GOODMAN are consistent when adhering to extensionalism, whereas Hempel's appeal to the meaning of predicates is not (but because of its incidental character it does not seem to be quite relevant, since he quickly takes refuge in the safe context of an artificial language). There is no empirical ground on which to found intentions in a logical positivist context. Therefore there is neither a foundation for analytical statements which would express the result of an analysis of such intentions. Quine's attack upon the analytic-synthetic distinction is justified [96], but we should keep in mind that it is only justified as an attack against this distinction *within* the logical positivist context. The impossibility of a predicate having an intentional meaning has neither been proved by QUINE nor by GOODMAN. But the regularity view is embedded in this context and, therefore, this second objection scores a point. It would be perfectly sound to ask what the status of a statement, explicating the meaning of a given predicate, would be. It would surely have to be a universal statement, but if it is called analytic (perhaps via Carnap's semantical rules of an artificial language) it would be vulnerable to Quine's criticism or, supposing that one could make sense of the analytic-synthetic distinction even

when accepting this criticism, *the possibility of a lawlike statement being analytic* should then also be reconsidered (we shall return to this point in ch. IV). If, on the other hand, such an explication of the meaning of a predicate is called synthetic, formulating a regularity without exceptions, it would reintroduce the problem of distinguishing between lawlike and accidental universals, which we have tried to solve by means of the notion of purely qualitative predicates, at a'higher' level.

A third objection against this second criterion was formulated by GOODMAN: "qualitativeness is an entirely relative matter and does not by itself establish any dichotomy of predicates" (compare note 54). If any predicates could be called purely qualitative, colour predicates would be amongst them. GOODMAN even tried to show that the qualitativeness of e.g. 'green' is an entirely relative matter. I think I have said enough about this in ch. II to substantiate the conclusion that this objection cannot be accepted as it is intended. If we take Goodman's problem seriously, it means that, at some or other time, a theory could be formulated in which the predicate 'green' can no longer be taken to be non-positional. However, as long as such an alternative theory has not been formulated, the thesis that qualitativeness is an entirely relative matter is mere speculation. Therefore this objection could be avoided.

As a final objection it must be noticed that this criterion does not distinguish the famous 'all ravens are black' from 'all metals expand when heated'. Relying upon an intuitively clear notion of pure qualitativeness, both these statements should be called lawlike, but in the discussion about R the statement about the ravens is always taken to be accidental, a fact for which the proposed criterion cannot account.

I now want to consider a third, and last, attempt to solve the problem: *a universal statement is lawlike when it is unrestricted.*

The universal statement 'all F's are G's' or '(x) (Fx->Gx)' is unrestricted if F does not necessarily denote a finite number of instances or a closed or finite class[97] and it is restricted when the antecedent term indicates a finite number of elements. The antecedent term 'screws in Smith's car', for example, refers to a finite class of objects and this can be ascertained from an analysis of the term itself. We need not investigate any empirical facts to find out whether this is the case or not. HEMPEL formulated this requirement in a slightly different way: a lawlike statement should not be regarded as being logically equivalent to a finite conjunction of singular statements.[98] POPPER makes the same distinction when talking about strictly and numerically universal statements, the latter being equivalent to a finite conjunction of singular statements while laws are always strictly universal.[99] Of course, a class, e.g. the class of planets, denoted by an antecedent term, may prove to be closed on the basis of further empirical evidence, but this does not imply the restrictedness of the universal statement in question, e.g. Kepler's first law. The point is that the finiteness of the antecedent class should not be derivable from the meaning of the antecedent term.[100] Therefore, "familiarity must be assumed with the use or meaning of the expressions occurring in a sentence, in deciding whether the statement conveyed

by the sentence is unrestrictedly universal".[101]

This criterion of unrestrictedness must not be identified with Reichenbach's requirement of unrestricted exhaustiveness. LAUTER reformulated this requirement as: "(A universal statement) Q is *unrestrictedly exhaustive* if and only if it is not the case that (1) Q is about only a finite number of individuals and (2) the fact expressed in (1) is highly confirmed at some time."[102] And therefore "one must investigate non-linguistic facts to determine whether or not a sentence is unrestrictedly exhaustive while one only need consider syntactical and semantical matters to determine whether or not a sentence is an all-statement, general or universal."[103] With this requirement in mind REICHENBACH tried to characterize fundamental lawlike statements.

The criterion of unrestrictedness meets with the following difficulties.

First, it implies an appeal to the meaning of the antecedent term, which is as objectionable here as in the case of the purely qualitative predicates.

Second, it characterizes the famous 'all moa's die before reaching the age of fifty' (Popper's example) as lawlike, which is very questionable (compare ch. IV).

Third, it supplies us with a reason to say of 'all gold-cubes are smaller than 10 m^3' that it is lawlike, while all of us would rather call it accidental.

It also leads to the description of 'all ravens are black' as a lawlike statement, an idea which most authors would not be prepared to accept. There is at least an intuitive doubt concerning the lawlike character of this statement and a satisfactory explication of the concept 'lawlike' should account for this feeling.

Reichenbach's requirement is not subject to the first and second objections, but the last two objections also hold against his proposal. Moreover, his distinction between fundamental and derivative lawlike statements is not of much help to show the lawlike character of such laws as Kepler's and Galileo's, as we have seen before.

Looking back at these attempts to find a criterion of lawlikeness, we must say that this approach does not seem to be very fruitful. It seems that one should not restrict oneself to a syntactical-semantical analysis of universal statements in isolation. Such a restriction has been shown to have failed every time.[104] We shall therefore in the next sections discuss the problems of laws and counterfactuals (section 2) and of laws and confirmation (section 3).
In both discussions it is the function of a law in scientific reasoning which will be in the foreground.

2. LAWLIKE STATEMENTS AND COUNTERFACTUALS

Of the copper screw, here on my desk, we may truly say:

if that screw had been heated it would have expanded (III)

but we would call the statement

if that screw had been in Smith's car it would have been rusty (IV)

false.

The universal statement

all copper things expand when heated (V)

gives us the right to call (III) true, but the universal statement

all screws in Smith's car are rusty (II)

does not give us the right to call (IV) true.

In other words, (V) supports the corresponding counterfactual (or subjunctive conditional – for the present these terms will be used interchangeably) (III), while (II) does not support the corresponding counterfactual (IV).

These examples suggest the following criterion of lawlikeness: *if a universal statement supports the corresponding counterfactual it is lawlike; if not, it is accidental.*[105]

But such a criterion presupposes a satisfactory answer to 'the problem of counterfactuals', as it is usually called[106], in which one does not make use of the concept of lawlikeness. And we may agree with PAP[107], that an acceptable answer is not available, at least not within a logical positivist context, and that it is therefore vain to propose this criterion of lawlikeness. But I shall, nevertheless, examine the problem of counterfactuals here. It is more than 'a fussy little grammatical exercise' and its importance has been stressed by different authors.[108] We can also learn something from it that is directly relevant to our central problem.

Is shall by means of symbols express the form of a singular subjunctive conditional as:

$Aa \text{–o}\rightarrow Ba$ (VI)

(in words: if *a* had been A it would have been B).

For the time being, we have to assume with many authors,[109] that such a statement implies the falsity of 'Aa'.

CHISHOLM proposed two ways to solve the problem: "The alternatives are: (1) (..) reduce the subjunctive to the indicative; (2) accept the subjunctive as describing some kind of irreducible connection and thus reject, or alter radically, the extensional logic which most contemporary logicians have tried to apply to the philosophical problems of science (compare my thesis v in ch. I, section 1). The problem is not an easy one; indeed, we may be justified in asserting that it constitutes *the* basic problem in the logic of science."[110]

Defenders of the regularity view have all chosen Chisholm's first alternative and the first proposal in this line has been to translate (VI) in:

$(Aa \rightarrow Ba).\sim Aa$ (VIa)

This translation restores the possibility of an extensional analysis, but it has, from the extensional point of view, the disastrous consequence that all subjunctive conditionals must be called true, their antecedent clauses being false. Not only (III), i.e. 'if that screw had been heated it would have expanded', would be true, but also 'if that screw had been heated it would have contracted' and 'if that screw had been in Smith's car it would have been rusty'.

Therefore this proposed translation must be rejected and it as been generally rejected.[111] Then, why mention this proposal here? Simply because the argument for rejecting it reveals an important point: it is taken for granted that an

indicative or 'normal' conditional, i.e. '$Aa \rightarrow Ba$', is true merely because its antecedent, i.e. 'Aa', is false. This again illustrates the 'glasses of extensionality' most philosophers of science seem to wear during the investigation of their problems. This also became clear from the usual exposition of Goodman's riddle, reproduced in ch. II, section 2, where '$t_1 \geqslant t_0 \rightarrow a$ is blue' was taken to be true solely on the ground that t_1 was, as a matter of fact, before t_0.

If this is not accepted, and I do not think that it can be accepted, the problem of counterfactuals becomes (part of) a problem of normal conditionals. In other words, even if we could reduce subjunctive conditionals to indicative conditionals, the problem of counterfactuals would reappear as the problem of analysing indicative conditionals. Like subjunctive conditionals, indicative conditionals require an analysis of 'the grounds upon which their truth or falsity may be decided' (NAGEL). But we must postpone this matter until later in this section.

After the rejection of (VIa) as a translation of (VI), authors like GOODMAN, CHISHOLM, HIZ[112], NAGEL[113], and BRAITHWAITE[114] all want to remain within the extensional context: "The content of counterfactuals can nevertheless be plausibly explicated without recourse to any unanalyzable modal notions", as NAGEL put it.[115] It is indeed admitted that there is a certain kind of *connection between the antecedent and consequent of a subjunctive conditional*, "and the truth of statements of this kind – whether they have the form of counterfactual or factual conditionals or some other form – depends not upon the truth or falsity of the components but upon whether the intended connection obtains" and therefore "it must be borne in mind that a general solution would explain the kind of connection involved irrespective of any assumption as to the truth or falsity of the components". Thus GOODMAN.[116] This, I think, is an excellent statement of the problem through its emphasis on a connection between antecedent and consequent: it could be literally repeated with respect to normal or indicative conditionals. But let us first pay some attention to the second proposal for a translation of (VI) which purports to analyse the connection mentioned.

A subjunctive conditional is then characterized not as a statement about facts, but as a statement about other statements or, as a meta-statement. A subjunctive conditional is interpreted as asserting that its consequent in the indicative mood follows logically from its antecedent in the indicative mood in conjunction with a law and a statement of the initial conditions, required by that law.[117]

For example, (IV), i.e. 'if that screw had been heated it would have expanded', is in fact a meta-statement, asserting that

'that screw expands' follows logically from 'that screw is heated'
and 'all copper things expand when heated' and 'that screw is copper'.

Or, in general, (VI) is rendered by:

Aa.L.C. logically entails Ba (VIb)

in which L is the law and C the set of required initial circumstances.

In this way it becomes completely overt that this analysis must presuppose

a solution of the two major problems, mentioned by GOODMAN, namely to define relevant conditions and to define what laws are.[118]

But apart from this, (VIb) could not be the correct expression of what is intended by the proponents of this proposed translation. What is intended may be better expressed by:

> there is a law L and a set of relevant conditions C such that A*a*.L.C logically entails B*a*. (VIc)

So, in order to verify a subjunctive conditional, we must, while clearly assuming a (limited) concept of the nature of a law, investigate scientific textbooks and see whether there is indeed such an L and such a C. If we succeed, the subjunctive conditional is true; if not, it is false. Such a procedure would imply that "the introduction of modal categories other than those of formal logic is entirely gratuitous", as NAGEL expressed it.[119] Chisholm's second alternative, 'reject or alter radically the extensional logic', need not be considered.

I shall not continue with a review of other attempts, e.g. those of HAMPSHIRE and RESCHER, who both made a distinction between two kinds of subjunctive conditionals, i.e. the nomological ones, which can be translated in the way of (VIc), and the purely hypothetical ones, which cannot be so translated.[120] What has now been said about the usual ways in which the problem has been tackled will do for our purpose, and we shall have the opportunity to come back to Rescher's view in the next chapter.

I shall now try to show that the problem of counterfactuals, taken as a separate problem, requiring a separate answer to the question about the grounds on which these statements could be called true or false, does not exist. But there is a problem concerning normal conditionals which may be described in the same words in which NAGEL described the problem of counterfactuals (see note 106) and which cannot be solved by 'a reduction to the indicative' because it is a problem of conditionals in the indicative mood.

Up to now we have used the terms 'counterfactual' and 'subjunctive conditional' interchangeably, but it has been stressed by different authors that a subjunctive conditional does not entail the denial of its antecedent and that many counterfactual statements are not expressed in the subjunctive mood, so that the identification of these terms should be avoided.[121]

I shall not go into this matter, but shall simply take it for granted that the grammatical distinction between the subjunctive and the indicative mood is not parallelled by the distinction counterfactual-noncounterfactual. We are here interested in the latter distinction in connection with the concept of law. When is a conditional 'if A*a* then B*a*' counterfactual? Its being expressed in the subjunctive mood is not a good guide. I think it is correct to say, that an empirical conditional is counterfactual in a certain situation or context, when it is clear from that situation or context in which it has been uttered that its antecedent is false. It is true that in such a case the conditional is often expressed in the subjunctive mood, but the use of this grammatical device to convey what is clear from context or situation does not introduce by itself an extra problem of verification or falsification. The problem, with which we have to

deal is one of analysing the grounds upon which a normal conditional may be said to be true or false, in a situation in which its antecedent is false. The subjunctive mood may also be used to suggest that the antecedent of the conditional expressed in it is true, as may be clear from Anderson's example: when a physician tries to establish the cause of John's death and says that, if John had taken arsenic he would have shown the same symptoms, then that physician intends saying that it is highly probable that John has taken arsenic, rather than that he wants to announce that he decidedly did not take the poison.

Therefore a satisfactory analysis of empirical conditionals should include an answer to the question on what grounds such a conditional may be called true or false when it is uttered in such a situation or context that the falsity of the antecedent is clear, whether or not it happens to be formulated in the subjunctive mood. And if one wants to make a distinction between 'real' counterfactuals, i.e. conditionals uttered in a context from which it is clear that their antecedents *cannot* be true now or in the future, and 'contingent' counterfactuals, whose antecedents may become or be made true in the future, we must say that the analysis of conditionals should include an analysis of both kinds of counterfactuals. (An example of a real counterfactual would be 'if that piece of butter had been heated to 150°F., it would have melted', uttered in a situation where it is clear that that piece of butter was eaten yesterday – Goodman's example; an example of a contingent counterfactual is 'if that screw here on my desk is heated, it expands', uttered in the present situation, where I can directly see that it is not heated).

I shall now consider some typical cases which have to be solved by an analysis of conditionals, in order to determine whether the use of such an analysis is a satisfactory undertaking.

I shall make some suggestions and also indicate where the usual extensional analysis is inadequate.

First, we consider a situation in which someone says:

if that is water then it boils at 212°F.

or: if Wa then $B^{212}a$ (VII)

When uttering this statement a bottle containing a colourless liquid is pointed to. The refractive index of that liquid is determined, and we infer from this, that 'Wa' is true. The boiling temperature of the liquid is measured to be 212°F., so that '$B^{212}a$' is also true.

Upon an extensional analysis, the logical structure of (VII) is

$Wa \rightarrow B^{212}a$ (VIIa)

and (VII) is said to be true when (VIIa) is true and this latter statement is true if both its components are true, as in the present case. Does the evidence indeed constitute good grounds for the truth of (VII)? That we should be careful with an affirmative answer may be seen from a second example.

Again we measure the liquid's refractive index and conclude that it is water. Then we measure its boiling temperature: 216°F. The usual analysis results in: (VIIa) and hence (VII) is false. But should we take this evidence as a good ground for the falsity of (VII)? "Of course not", someone might remark,

"because the pressure increased in the meantime, as I myself established, so that the second experiment just had to yield a boiling temperature higher than 212°F. If you only had taken care of the relevant circumstances, the second experiment would also have led you to the truth of (VII), like the first. But now that the circumstances have changed the evidence has no verifying or falsifying significance." This rejoinder is justified and it shows that the extensional analysis of (VII) is insufficient. *An appeal to relevant circumstances is made:* extensional evidence cannot count as evidence for or against the truth of a conditional if it is taken in isolation; it can only count as such when it is obtained in an experiment in which the required circumstances have been fulfilled. This is also valid for the first experiment: the knowledge of the truth-values of the components alone is not enough, even if this knowledge implies that both are true; we also need knowledge about the relevant circumstances (and in this connection, STEVENSON speaks of 'intensional evidence'[122]). But this means that what GOODMAN called 'the first major problem' in connection with the analysis of counterfactuals also arises in connection with the verification or falsification of normal fulfilled singular conditionals. I want to suggest that we formulate this problem as: What are relevant circumstances? How can we judge certain conditions to be relevant and others to be irrelevant? What is our norm for accepting the evidence of one experiment as relevant for the truth or falsity of a conditional and for rejecting the result of another experiment as irrelevant?

I now want to formulate my answer briefly, and I will try to substantiate this in chapter V.

The covering law of the conditional, taken as a statement within a theory or conceptual system can provide us with such a norm. I also want to add that it follows that the analysis of conditionals also meets with what GOODMAN calls 'the second major problem' in connection with counterfactuals, namely the problem of an empirical law. An extensional analysis fails to bring these points to light. Of course, one frequently does refer to the required circumstances by means of such remarks as 'provided the system is in a normal state' or 'on the condition that suitable circumstances are fulfilled' or something similar. But it is one thing to pass such a remark rather incidentally and quite another to take full account of these relevant circumstances in a systematic philosophical analysis. GOODMAN tried to do this in connection with counterfactuals because these statements give rise to a very strong feeling of uneasiness whenever one adopts an extensionalist view. It is, however, necessary to offer a systematic account of conditionals in general simply because the counterfactuals are only a sub-class of this general class.

Let us now investigate a situation when we say:

if that copper screw is heated then it expands

or: if Hs then Es (VIII)

We say this about that screw over there at a time when we can easily see that it is not heated. 'Hs' would, therefore, be false.

According to the extensional analysis of (VIII),

$Hs \rightarrow Es$ (VIIIa)

this conditional should now be called true.

But should we regard our evidence, namely that that screw is not heated, as a sufficient reason for the truth of (VIII)? Of course, (VIIIa) is true, but should one regard (VIII) as true for *this* reason only? An extensionalist must answer "Yes" and we have met with two examples where he indeed said exactly that. In the normal exposition of Goodman's riddle (reproduced in ch. II, section 2) the conditional '$t_1 \geqslant t_0 \rightarrow a$ is blue' is taken to be true solely because t_1 is in fact prior to t_0.

When GOODMAN, NAGEL, CHISHOLM, and many others reject the translation of a counterfactual as '$(Aa \rightarrow Ba).\sim Aa$', they do so simply because the first conjunct, i.e. '$Aa \rightarrow Ba$', is true when its antecedent is false. When CARNAP proposed his method of reduction sentences for the introduction of dispositional predicates in the empirical language, he did so on the basis of the consideration that normally a conditional is true when its antecedent is false; if he had not taken this for granted, there would have been no direct need for an alternative method of reduction, but there would still have been a problem of conditionals.

I think it is necessary to state clearly that the falsity of the antecedent of an empirical conditional as such never constitutes sufficient evidence for the truth of that conditional. Every scientist would agree to this and I think this is fully justified. Should we then rather say that we do not have good grounds from which the truth or falsity of (VIII) may be inferred in the situation mentioned above? Must we not rather say that we should wait for the results of an experiment in which that screw is really heated? In other words, should we not rather in this situation view (VIII) as a prediction whose truth-value still remains to be established? It would be wrong to do that because every scientist would be prepared to call (VIII) true in this situation. Conditionals like (VIII) are taken to be assertible with warranty even when their antecedents are false. But is it not strange to appeal to scientific practice in a case like this? Certainly not, because this appeal to scientific practice is also made in connection with the formulation of the problem of subjunctive conditionals or, of what I have called, real counterfactuals. These real counterfactuals cannot be viewed as predictions, simply because their antecedents cannot be established as true in the future. The butter I ate yesterday cannot be heated now or in the future in order to test the statement 'if that piece of butter is heated then it melts'. These real counterfactuals cannot, therefore, be accounted for by regarding them as predictions, but nor can they be rejected as genuine empirical statements since they are used in scientific practice every day and are there taken to be completely bona fide empirical statements which are either true or false. But so are the contingent counterfactuals like (VIII), and it is therefore correct to ask on what grounds a counterfactual conditional could be called true or false, independently of the fact whether or not its antecedent may, as yet, be found to be true in the future. When we do this, we should also say that (VIII) is true of that screw which is on my desk and which is not heated, because it is deducible from the law that all copper things expand when heated, provided that the required conditions are fulfilled. And the conditional 'if that screw is heated it contracts' is false, even

if a truth-functional analysis would make us believe that it is true. This follows from the fact that the connection asserted in this conditional is excluded by the connection laid down in the law mentioned before.

Therefore, a counterfactual, whether real or contingent, whether expressed in the indicative or subjunctive mood, is true when it can be inferred from a law. This means that the connection intended by that counterfactual obtains*. And a counterfactual is false when the intended connection does not obtain, since it is excluded by a connection laid down in a law. It should be obvious that in this way the analysis of conditionals can be completely reduced to the analysis of laws and relevant circumstances. Moreover what we have said seems to imply that an experimental test is superfluous; the evidence obtained in the first example does not *by itself* verify the conditional 'If W*a* then $B^{212}a$'. However, I have to postpone any further discussion of this matter until ch. V. We should at this stage become aware of the distinction between normal and (r)evolutionary science. Whenever we appeal to scientific practice, we appeal to normal science and it is called normal because of the very fact that we are taking for granted certain laws embedded in a theory.

We must still make one short remark about conditionals, which are uttered in a situation where we have no idea about the truth-value of the antecedent. This is a simple matter as such a conditional is true when it applies a connection, laid down in a law, to a particular case, and it is false when the intended connection does not hold or is excluded by a law.

What has been said so far is restricted to conditionals which can be related to available laws. We could call such conditionals nomological conditionals, in the same way that RESCHER speaks of nomological counterfactuals.[120] Our analysis has no bearing on purely hypothetical conditionals (cf. again RESCHER).

Our conclusion must be that the analysis of conditionals, whether fulfilled or not, whether they are expressed in the subjunctive mood or the indicative, requires an answer to the question what an empirical law is. Our answer to this question must, if it is to be satisfactory, also supply an answer to the problem of relevant circumstances. This problem has never been answered by proponents of the regularity view and, I think, it would be extremely difficult to realize this within this view because knowledge of these conditions seems to require the occurrence of exceptions or irregularity (compare ch. I, section 6). R, on the other hand, characterizes a law as the formulation of a regularity without exceptions. But it may be that the appeal to a theory, to which the proponents of R also take recourse, could answer this question.

3. LAWS AND CONFIRMATION

Our analysis in section 1 showed that from the various attempts at a solution of the problem no satisfactory criterion of lawlikeness could be obtained.

* Using Goodman's terminology.

In section 2, which did not aim at the establishment of such a criterion, the need for such a criterion was stressed by pointing at the fact that we will not be able to offer a satisfactory analysis of empirical conditionals (including counterfactuals) as long as we are unable to give a satisfactory analysis of the concept of empirical law.

We shall now consider yet another attempt at such an analysis. Laws, it is remarked, have a certain function in science: they are used to make predictions. Therefore, *a universal statement is lawlike if it is acceptable as a true statement prior to the examination of all its instances.*[123] If the evidence for a law would coincide with its scope of predication, to use Nagel's term, it could not be used for making predictions.

Of course it is not meant that a statement like '(x)(Ax→Bx)' is lawlike only if it is acceptable as a true statement and the class of A's is infinite, since this would make the criterion depend on the question whether or not the universe is infinite, as STEGMÜLLER remarked.[124] What is meant by this proposal is that a universal statement is lawlike if it is acceptable as true before the examination of all elements of the antecedent class and it does not matter whether or not this class turns out to be finite in the future.

It is intended that, given this criterion, the function of a law as supporting counterfactual conditionals can also be accounted for. Counterfactuals are taken to stand in the same logical relation to a law as predictions do; in the same way that a law enables us to make predictions, i.e. statements about as yet unobserved instances, it also enables us to formulate counterfactuals, i.e. statements about unrealized, or rather unrealisable, instances.[125] Predictions and counterfactuals are taken to be similar in this respect.

After what has been said in section 2 about counterfactuals and conditionals in general, it may be clear that I fully agree with the idea of putting predictions and counterfactuals on the same level. Counterfactuals are, in the same way as predictions, derivable from a law in conjunction with the statements of relevant conditions. Whether they should be expressed in the subjunctive mood, a way in which predictions could also be expressed, is a matter of grammatical taste. But the assertion that a universal statement is lawlike if it supports the related predictions leads exactly to the same conclusion as the assertion that a universal statement is lawlike if it supports a corresponding counterfactual, i.e. the concept of lawlikeness presupposes a satisfactory analysis of (predictive) conditionals, which, as we have seen in section 2, requires an answer to two questions: What is a law? and What are relevant circumstances? Briefly, the fact that a law enables us to make predictions (and to assert counterfactuals) is a symptom of a universal statement's being lawlike and it cannot therefore be taken as a criterion to determine whether a statement is lawlike or not.

But someone might remark that it is not the predictive force which is used in the criterion in question, but a statement's being acceptable before all its instances have been examined. This, however, directly poses the following question: Why do we accept one universal statement as true *prior* to such an examination and not the other? Supposing that we do not accept as true the

statement about the screws in Smith's car. i.e. 'all those screws are rusty', before we have examined them all and before we have established that all are in fact rusty, then *why* are not we prepared to do that? Supposing also that we do accept 'all copper things expand when heated' as true, while we have not yet examined everything referred to by the antecedent term, why are we allowed to do this? We may translate this into Goodman's formulation of the problem and ask why a positive instance of the copper-statement does have a confirming force, while a positive instance of the screws-statement has no such confirming force?[126] Goodman's answer is clear enough: "The difference is that in the former case (i.e. in the case of the statement about copper) the hypothesis is a *lawlike* statement; while in the latter case (i.e. in the case of the statement about screws), the hypothesis is a merely contingent or accidental generality. Only a statement that is *lawlike* is capable of receiving confirmation from an instance of it; accidental statements are not".[127] Therefore the acceptability of a universal statement as true before the examination of all its instances too must be called a mark of lawlikeness, rather than a defining characteristic.

To conclude, if a universal statement is lawlike it has predictive force, it supports counterfactuals, it is confirmable by its positive instances, and it is acceptable as true prior to the determination of all instances. But all these indications presuppose the analysis of the concept of lawlikeness.[128] Hence the proposed criterion must be dropped. It is, however, informative to apply it, taking it in its intuitively clear form, to the famous case of the black ravens, because this may clarify what exactly the debate between the regularity and the necessity view is about.

I am taking it for granted that nobody would object if we were to say that

all ravens are black (I)

is accepted as true on the basis of the evidence available at this moment. This evidence is regarded as an exceptionless repetition by all proponents of R. It may also be taken for granted that this evidence does not exhaust the scope of predication of (I). Therefore, it should be called lawlike on the basis of the criterion under consideration. KNEALE, however, raised doubts with respect to such a view.[129] If (I) were a law, we should be justified to predict

if that particular thing is a raven then it is black (II)

and to assert

if that particular thing had been a raven then
it would have been black (III)

But, according to KNEALE, it may be an historical accident that no ravens ever lived in very snowy regions, and under such circumstances the truth of (I) may simply depend upon such an accident. It is indeed possible that the progeny of ravens, which migrate to polar regions, will be white, a likely theory if we accept the idea of natural selection. If we realise this, it becomes doubtful whether (I) allows us to assert the predictive statement (II) and the counterfactual (III), since these statements can now be taken to refer to a member of the mentioned progeny of ravens.[130]

A universal statement may be regarded as confirmable if there are positive

instances and it may even be accepted as true prior to the examination of its instances, and yet be accidental in the sense of asserting an historical accident on a cosmic scale. It may be well confirmed, there may even be a very long series of positive instances without any exception, and it may, on such an inductivist basis, even be accepted as true. However, it may nevertheless fail to enable us to make predictions and to assert counterfactuals; in other words, to be lawlike. (It seems therefore not to be correct to identify, as GOODMAN does, confirmability with lawlikeness). Such a statement is dropped when a negative instance turns up, as 'all swans are white' was dropped when black swans were discovered. According to KNEALE, the regularity view entails that a law is only concerned with actual and not with possible cases. This is the very point which makes a Humean account of laws, expressed in R, inadequate.[131]

We shall return to this view in the next chapter. The question here is how the proponents of R would answer this objection. They should at this stage of the argument answer the more specific question how we are to distinguish between universal statements which assert a regularity that is only an historical accident and universal statements asserting a lawlike regularity. Regularity alone is not enough: "To say that valid predictions are those based on past regularites, without being able to say *which* regularites, is thus quite pointless. Regularities are where you find them, and you can find them anywhere. As we have seen, Hume's failure to recognize and deal with this problem has been shared even by his most recent successors." Thus GOODMAN.[132] Something else over and above regularity is needed in order to decide between those regularities that would lead to lawlike statements and those that do not. There must be some *organizing factor*, some point of view which would make this decision possible. In Goodman's view the organization can be found in the use of language and more specifically in the entrenchment of those predicates occurring in a statement.[133] Such a criterion, however, has been put forward in connection with such a case as the green-grue conflict, which has been discussed in ch. II. I therefore fail to see the relevance of Goodman's 'organizing factor' for drawing a distinction between lawlike statements and accidental ones like 'all ravens are black', 'all gold cubes are smaller than 10 m^3', 'all robins' eggs are greenish-blue', etc.

But BRAITHWAITE, NAGEL and others *do appeal to a theory* or a scientific system or a corpus of knowledge whenever they maintain that *a universal statement is lawlike if it occupies a certain logical position in a scientific theory.*[134] With this criterion we have now reached the last attempt of proponents of R to make the required distinction. NAGEL, explicitly responding to Kneale's criticism, states: "The criticism under discussion does not undermine the Humean analysis of nomic universality. The criticism does bring into clearer light, however, the important point that a statement is usually classified as a law of nature because the statement occupies a distinctive position in the system of explanations in some area of knowledge, and because the statement is supported by evidence satisfying certain specifications."[135]

Based on such a characterization of lawlikeness, the lawlike universals are

distinct from accidental ones in the following two respects: first, lawlike universals can, because of the fact that they occupy a logical position in a theory, *be confirmed indirectly*, while accidental universals cannot be confirmed in the same way, but only directly by means of their positive instances; second, lawlike universals come to *be connected with relevant circumstances* within the theory they are embedded in.

Let us briefly consider both these points.

The statement 'all ravens are black' can only be directly confirmed. It does not form part of a theory which would lead to any indirect evidence. The statement 'all copper things expand when heated', on the other hand, can be confirmed both directly and indirectly. The direct evidence for 'all iron things expand when heated' constitutes indirect evidence for 'all copper things expand when heated' via other statements within the theory, e.g. 'all metals expand when heated'. The latter statement is indirectly confirmable in a different way. It makes the derivation of other laws possible (e.g. the laws mentioned above about copper and iron things), and direct evidence for such derived laws would count as indirect evidence for 'all metals expand when heated'. It follows that there are two kinds of indirect evidence, and when applying the criterion of lawlikeness, which is now being considered, it would imply that indirect evidence of either kind should be available for a universal statement to count as lawlike.[136] But we should not take the availability of indirect evidence of either kind in the case of a universal statement as a criterion for the lawlikeness of that statement, a point which GOODMAN conclusively established. The fact – let us suppose it to be the case – that all screws in the cars of my friends are rusty, would have to count as indirect evidence for 'all screws in Smith's car are rusty' via the higher-level statement 'all screws in Smith's car and the cars of my friends are rusty', as long as we have no criterion for distinguishing between this latter statement and, for example, 'all metals expand when heated'; this means as long as we have no criterion of lawlikeness.[137] When we use the criterion of a universal statement being lawlike due to its occupation of a certain position in a theory – the very criterion we are considering here – Goodman's criticism poses the following question: *What should count as a theory and what does it mean for a statement to occupy a position in such a theory*? The analysis of the concept of lawlikeness then presupposes an adequate analysis of the concept of a theory.[138]

Let us now turn to the second and, I think, far more interesting point. By making the being lawlike of a universal statement depend upon its being part of a scientific system, the proponents of R also intend to answer Kneale's criticism (stated above) in such a way that a Humean view of laws can be maintained. The introduction of modal concepts would then become entirely superfluous. Let us now examine Nagel's answer to KNEALE.[139] Suppose there were a biological theory about the factors determining the colour of the plumage of birds and that one of these factors is the genetic code of birds. In such a case we would be able to give a reason for the blackness of all ravens by means of the theory to which 'all ravens are black' belongs (compare BRAITHWAITE in note

134). This generalisation should then be regarded as lawlike. Accepting the existing biological theories we know that the genetic code of an animal can be changed by e.g. X-ray radiation. In other words, the presence of X-ray radiation constitutes a *relevant circumstance* as regards the colour of a bird's plumage. When we now normalize this circumstance in such a way that the presence of X-ray radiation of an intensity below a certain minimum becomes a *normal circumstance*, then 'all ravens are black' enables us to predict

if that polar inhabitant is a raven then it is black, provided
that all circumstances are normal

and to assert

if that polar inhabitant had been a raven then it would have
been black, provided that all circumstances are normal.

The addition of such a ceteris paribus clause is completely in order. Every prediction and every counterfactual, that means every conditional derivable from a law, is only taken to be true on the tacit assumption that the required circumstances are fulfilled in the case in question. When, for example, we say of a liquid 'if it is water then it boils at 212 °F.', we always do so on the assumption that the circumstances required by the law, 'all water boils at 212 °F.', have been fulfilled, e.g. a pressure of 1 atm.

In my opinion, NAGEL makes a first step in the right direction in his answer to Kneale's criticism: once 'all ravens are black' is embedded in such a theory, in which a reason can be given for the blackness of ravens and in which certain relevant circumstances come to be known, this statement may count as a law. But this attempt should not be taken as a final step. Moreover, it is a step which seems to be hardly compatible with the distinction between experimental laws and a theory, which is presupposed by the proponents of R, including NAGEL. Let us now consider these points.

Nagel's attempt should be followed by an analysis of the concept of a theory and in particular by an answer to the question as to how a theory is to be constructed in order to account for such a universal statement. An answer to this question could, perhaps, make it clear what it means to say that a universal statement is embedded in a theory and it could thereby, taking this embedding as a criterion of lawlikeness, perhaps supply us with a satisfactory analysis of lawlikeness. But to me it seems impossible to answer this question as long as one insists on the idea that a law, basically, asserts an exceptionless repetition which can be known through direct or theory-free observation. The reason being that I cannot find any statement, that would now qualify as a law, which is merely the assertion of an exceptionless regularity given in theory-free observation.

Let us take as an example the following statement

all water boils at 212 °F.

This statement is undoubtedly not the expression of a theory-independent constant conjunction. Isolated from the theory within which this statement functions as a law, there is no regularity at all, but an irregularity. In all cases of 'direct observation', which here means the separation of an 'observation' from a theory about boiling-point-retardation, increase of the boiling-point by

impurities, the connection between pressure and boiling-point, the existence of the hydrogen isotope deuterium, the connection between the structure of a liquid and its boiling-point, etc., we face an irregularity and could at best say that water often boils at 212°F. The statement 'all water boils at 212°F.' could then be falsified a great many times and it would be completely nonsensical to insist on regarding it as the formulation of a regularity without exceptions. What happens here is simply that the negative instances are not taken as falsifications, but as *genuine exceptions* (compare ch. II, sections 5 and 6) on the basis of the normative but conjectural statement 'all water boils at 212°F.'. *And these genuine exceptions constitute the incitement to further theory formation, i.e. the formation of other conjectural universals and the discovery of relevant circumstances.* In such a way the statement 'all water boils at 212°F.' loses its ad hoc character and comes to be confirmed as the assertion of a necessary connection by its being an element within the newly formed theory. It is because an observation is made by means of this theory, that one can assert a regularity. *Without a theory there are at best regularities as historical accidents on a cosmic scale, and such historical accidents are of no interest within the formation of a theory and the growth of science. Lawlike regularities are co-constituted by means of our theories.* This is only a very brief formulation of the idea that will be developed in ch. V. I regard it as the greatest shortcoming of the proponents of the regularity view that they take a naive and unexplicated concept of regularity for granted. As long as they do that, their rejection of the necessity view, which is founded on the obscurity of the concept of necessity, cannot be very convincing. As long as they do this, all the attempts, considered in this chapter, to distinguish between *accidental and lawlike regularities*, are doomed to remain peripheral remarks.

It seems to me to be correct to say that it is because it is part of a theory that a universal statement is lawlike. However, we should not interpret this to mean that we first of all have the universal statement in isolation, which is then followed by the theory in which it happens to be taken up. We can indeed say that a law asserts a regularity, but this regularity is only brought about by the confirmation of the theory in which the law is 'automatically' embedded. *A lawlike regularity is co-constituted by a theory and therefore the law, which formulates that regularity, is indissolubly bound to that theory.* We can only divorce the law from the theory when we are prepared to sacrifice the regularity and thereby the law itself. The concepts 'water' and 'boiling-point', for example, which are connected in the law 'all water boils at 212°F.', are not theory-independent concepts; they have a particular meaning because they are connected to a great many other concepts in the theory, and this is the reason why this law can neither be theory-independent.

This contradicts the very starting point of the regularity view, namely the important distinction between experimental laws and theories, a distinction which goes back to the fundamental logical positivist distinction between observational and theoretical terms, which, again, is founded on the notion of direct or theory-free observation.

Within the logical positivist view, the meaning of the concepts in an experimental law, the truth of a law and its confirmation, are all theory-independent (compare the beginning of this chapter and ch. I, section 1), but according to what has just been said about the relation between a law and a theory, such an independence cannot possibly exist unless one is prepared to waive the law. I therefore do think that Nagel's appeal to a theory in order to characterize lawlikeness, an attempt which I called a first step in the right direction, would appear on further elaboration to be incompatible with his own philosophical point of view. This appeal has by no means shown that a Humean analysis of nomic universality can be upheld or that the introduction of modal categories is entirely gratuitous.[140]

IV. THE NECESSITY VIEW

1. LOGICAL NECESSITY

In the contemporary discussion between proponents of the regularity and the necessity view the possibility of an empirical law asserting a logically necessary connection has been rejected by both.

However, I consider it as important to consider the reasons that have been given for such a rejection and this will be my primary aim in this section.

The first requisite for such a rejection is a clear concept of logical necessity. This concept is usually taken to be quite transparent; a generally accepted analysis of this concept has been provided by logical theory.[141] A statement, according to this theory, is logically necessary if its negation is a contradiction. If this concept of logical necessity is taken for granted, no empirical law is logically necessary, because logic alone does not enable us to prove its negation to be contradictory. NAGEL even goes further when asserting that logic enables us to *prove* that a law is not self-contradictory. If one nevertheless maintains that laws are logically necessary or that they assert a logical necessity, one should either say that there are no empirical laws (until now, at least, for in the future our insight may become sufficient to establish that they are logically necessary), or that the methods of logical proof can no longer be applied.[142] A second objection to the logical necessity view has been raised. If a statement is logically necessary it cannot have any empirical or factual content and, therefore, if it has such a content, it cannot be logically necessary.[143] If a statement is logically necessary, empirical evidence cannot have the slightest influence on its truth-value; the statement cannot be empirically confirmed or falsified.[144] This would be sufficient to reject the necessity view in which necessity is interpreted as logical necessity.

Of course, this rejection essentially hinges upon the concept of logical necessity and we shall now see that the transparency of this concept is only apparent.

First, there is the class of logically necessary statements which are usually called logical truths: "a logical truth is a statement which is true and remains true under all reinterpretations of its components other than the logical particles"[145], for example 'no unmarried man is married'. But if it is said that laws are logically necessary it is not intended that they are logical truths, but even with respect to these statements the matter is not as clear as the opponents of logical necessity take it to be, as Strawson has shown.[146]

Second, there is the class of logically necessary statements, which can be reduced to logical truths by means of an analysis of the non-logical terms occurring in these statements: the analytic statements. One need not inspect empirical matters in order to know that such statements are true, they have no empirical content and they cannot be denied without self-contradiction. If one rejects that an empirical law is logically necessary one also rejects the view that

it is an analytic statement. Therefore, the proponents of R, defending the synthetic character of a law and taking the analytic-synthetic dichotomy for granted, reject the view of logical necessity. But they frequently add an interesting remark about the fact that laws often appear to be analytic. It is said that a sentence of universal form could be used in different ways. Consider for example (NAGEL), the sentence 'copper is a good electrical conductor'. It could be used to express an empirical law, in which case it is synthetic; but the same sentence could also be used to express a defining property of copper, namely its high conductivity. In the latter case the sentence is used to express a logically necessary truth and the statement 'copper is a good electrical conductor' is analytic: "(it) cannot be denied without self contradiction, so that it does not express an empirical law for which experimental evidence is relevant but states a logically necessary truth". And "Sentences which appear to state laws but which are in fact employed as definitions, are commonly called 'conventions'.".[147]

If these two kinds of usage of sentences of universal form are distinguished we need no longer be deceived by the fact that, at first sight, empirical laws seem to be analytic.

AYER has also drawn attention to this usage of a universal sentence which may sometimes assert an empirical law and at some other time express a convention and he adds that "we are free to settle the matter in whichever way we please".[148] PAP also referred to this twofold status of a universal statement, characterizing it by the expressions 'a statement about the course of nature' and 'a statement about the meaning of a word'.[149]

But, first, the distinction between analytic and synthetic, and connected with it, the distinctions between nonempirical and empirical, definition (convention) and empirical law, as well as statements about the meaning of a word and statements about the course of nature, is not at all as clear as would be required for an adequate rejection of the logical necessity view!

Moreover, even if we take this questionable dichotomy for granted, the Goodmannian question (for obvious reasons called 'Goodmannian') why in the one case such a shift of meaning of a universal sentence from synthetic universal statement to convention or definition takes place as a matter of fact and not in the other, still remains to be answered.

We shall now examine both these points somewhat further.

The transparency of the analytic-synthetic dichotomy has been badly affected by Quine's attack.[150] All statements have, in Quine's view, the same cognitive status within 'the whole of science', and the unit of empirical significance is not the empirical statement in isolation, but the conceptual scheme of science or the whole of science, which is, ultimately, an instrument for making predictions.[151]

There are three theses here: first, the unit of empirical significance is the conceptual system as a whole and not the statement in isolation; second, this system is a tool for making predictions; and third, all statements have the same cognitive status within this system.

I am in complete agreement with the first of these theses, though for reasons different from those given by QUINE. I do not agree with the second thesis which expresses Quine's instrumentalist view, and I do not think that this thesis is a consequence of the other two, but I cannot go further into this point at present. Nor do I agree with the third thesis. Quine's argument for this thesis is that, within a conceptual system, and we cannot but consider a statement as being taken up into such a system (the first thesis), "Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system (..). Conversely, by the same token, no statement is immune to revision. Revision even of the logical law of the excluded middle has been proposed as a means of simplifying quantum mechanics".[152] Apparently we should interpret this as follows: analyticity is identified with 'true come what may' or with irrevisability and a statement's being synthetic is identified with its being revisable upon evidence that may come. It is clear that QUINE does not view the conceptual scheme of science as a conceptual system whereby the individual concepts acquire the meaning they have. For if this view were upheld, a 'drastic adjustment elsewhere in the system', in order to keep some statement true, would imply a change in the meaning of the concepts of that very statement. The content of that statement would then also change and one can no longer say that the *original* statement is kept true. QUINE does indeed take the conceptual scheme as a tool for prediction.

I have said that I do not agree with Quine's thesis that all statements have the same cognitive status, but let me first make it clear that I do agree with it if taken in one particular sense. I do agree with it if it is directed against those who take isolated statements as units of empirical significance which can be confirmed and which convey information. For, in such a case, to say of such an isolated statement that it is analytic could indeed hardly mean anything else than 'true come what may' or 'immune to revision'. But, and now I come to my disagreement with QUINE, one can subscribe to Quine's first thesis, that statements in isolation should not be taken as the units of empirical significance, and at the same time uphold a distinction between analytic and synthetic statements which occupy a position in a theory or conceptual system. In other words, it is possible to make sense of the analytic-synthetic dichotomy relative to a given conceptual system. It could be perfectly meaningful to say of a statement that it is analytic when we do not view this statement in isolation, but as embedded into a theory. This has been the line of defense of the 'dogma' by GRICE and STRAWSON.[153] They argue that there is no necessity for the adoption of a conceptual system and, therefore, QUINE is right when he says that no statement, being taken up in such a system, is immune to revision, as the system as a whole may be rejected on the basis of what may come. But this does not exclude the possibility of there being necessary connections within a given conceptual system which are expressed in analytic statements (cf. note 34). There are universal statements of which a revision, i.e. a switch from 'true' to 'false', would require a conceptual revision, e.g. 'all bachelors are unmarried men', and there are statements of which a revision does not require such a conceptual

change, e.g. 'all swans are white' or 'all screws in Smith's car are rusty'. A statement of the first kind may be called analytic and a statement of that latter kind synthetic.[154]

Once we have adopted a certain conceptual scheme or theory – I would rather say: once we have formed a theory or conceptual system, because the term 'adoption' of GRICE and STRAWSON suggests too much, it becomes a matter of take it or leave it – hence once we have formed a theory, we also know certain meaning connections to be necessary (compare as regards this point and what follows, ch. I, sections 4, 5 and 6). In other words, once we have formed a conceptual system, we also know certain statements to be analytic. The *necessary connections expressed by those statements consitute that very system.* The denial of those statements would come down to the denial of that system or part of it. But it is that very system that enables us to observe certain data through the very fact that they are but those data because they have been co-constituted by that system. Without it we would not be able to observe certain data, we would not be able to consider them as confirmatory evidence for statements in that system, and we would not be able to explain certain phenomena which can be observed through that system. And, of course, it has also been stressed in the sections of ch. I, just mentioned, that if QUINE intends saying that this system as a whole is not immune to revision, he is right: *what may come may indeed force us to dissolve these necessary connections.* In this way the analytic statements are revisable in principle. But such a revision can only take place by the formation of new necessary connections, which are based upon new empirical evidence, not available before (evolutionary science) or by the formation of an alternative theory, which, in its turn, is marked by other necessary connections, making the alternative just that particular theory that it is (revolutionary science). The revision of analytic statements or the dissolution of necessary connections means that we must form new concepts in order to restore intersubjective understanding of the empirical phenomena. The revision of statements like 'all screws in Smith's car are rusty' does not imply anything of this kind, because this statement does not assert a necessary connection.

This will, I think, do to show that the rejection of the logical necessity view of empirical laws, on the basis of a concept of analyticity that is indeed connected to statements in isolation, cannot be accepted. Of course, we may say that the negation of an analytic statement is a contradiction, but such a remark should be related to the system in which the statement in question is taken up and it does not imply that that statement is empirically or factually empty. An analytic statement, taken as a statement within a theory, of course has empirical content: *it states that certain phenomena cannot occur in the empirical reality*, but this reality, it must be stressed again, is not an observer- or theory-independent reality; it is the empirical reality, observable through or co-constituted by our theories.

I want to clarify the notion of 'phenomena or events which cannot occur' by means of the following example.

The classical theory of electromagnetism 'forbids' that a charged particle

be accelerated and at the same time not emit radiation. Therefore, the existence of a Bohr-atom is a phenomenon that could not occur in the eyes of a nineteenth-century physicist. A physicist accepting the classical view can, in fact, only speak *in a metaphorical way*, of a 'forbidden' phenomenon or of an event that cannot possibly occur. He cannot answer the question "What do you understand by such an impossible event or phenomenon, or what do you imagine it to be?" because his conceptual system excludes such an understanding or imagining. In other words, if he could answer this question (and a 20th century physicist can), he would already have formed the required concepts (as a 20th century physicist has done). I hope it will be clear from this example in which sense we speak of forbidden or impossible phenomena or events.

It might be remarked at this moment that, by relating analyticity to a conceptual system or theory, I no longer speak of *logical* necessity (but, for example, of physical necessity, which will be examined in the next section). If this remark is intended to say that the being analytic of a statement is a characteristic of that statement in isolation, I would answer that the class of analytic statements should in that case be identified with the class of logical truths. If this remark, however, only means that 'logical necessity' is not any longer the appropriate term to be used, then I completely support the exchanging of it for a better one.

Now if we use this concept of analyticity in relation to a conceptual system or theory, and in particular to conceptual change, I think the possibility of a law being analytic is still open. The rejection of the logical necessity view on the basis of a concept of analyticity, that is taken to be applicable to statements in isolation, does not automatically imply the rejection of the logical necessity view in which a concept of analyticity, related to conceptual change and taken to be applicable to statements *within* a theory, is used. To show this was primarily the aim of this digression concerning the famous dichotomy.

We must now turn to what we called the Goodmannian question, posed in connection with the possibility of a sentence expressing a synthetic universal statement or an empirical law at one time and a definition or convention at another: why do we 'conventionalize', if we do, in some cases (e.g. of copper and its conductivity) and not in others (e.g. of ravens and their blackness) and why is this done at all at the moment it is done and not earlier or later? Ayer's 'answer', that we are free to settle the matter as we please, says in fact that these questions are illegitimate, because we are indeed free to do what we want; thus there is no evidence upon which the matter can be settled and it is therefore not meaningful to ask for such evidence. As far as I am concerned, this view is completely unacceptable and I shall not go further into it. But NAGEL has made a very interesting remark: "the shifts in meaning to which sentences are subject *as a consequence of advances in knowledge* (my italics, H.K.) are an important feature in the development of comprehensive systems of explanation".[155] It is this feature that will receive further attention in ch. V and which has already been analysed in connection with concept formation in ch. I; the advance of knowledge, the attempt to reach better understanding on the basis of new empirical evidence, forces us to regard certain connections necessary or lawlike.

At first these have an ad hoc status, but they may become empirically confirmed as necessary connections by the successful formation of a theory on the basis of on-going inquiry. It is in this way that these statements acquire a position within a theory and we are not free to settle the matter as we please, as we shall see later.

Now when we leave open the possibility of laws being analytic statments, asserting a logical necessity, this does not mean that we agree with the views of e.g. BLANSHARD and EWING.[156] In these views a theory-independent reality, 'shot through with necessities', is presupposed and this is as unacceptable as the thesis that there are contingent regularities in an observer-independent reality. Empirical knowledge is not knowledge of such an independent reality, but of a theoretically or conceptually co-constituted reality. I shall therefore not further examine these views, but instead turn to Kneale's view.[157]

2. PHYSICAL NECESSITY

The discussion in ch. III, section 3, of the raven-example has already given an idea of Kneale's view of empirical laws, but we shall briefly restate it here.

Suppose that certain birds, called dodos, only lived between 1500 A.D. and 1800 A.D. and suppose that every one had a white feather in its tail. Now 'all, dodos have a white feather in their tail' is a true universal synthetic statement and within the regularity view it should count as a law, according to KNEALE[158] (he takes the regularity view without its appeal to a theoretical context; cf. ch. III, section 3). But this dodo-statement does not enable us to assert a corresponding counterfactual conditional, as laws are generally taken to do. The reason for this is, in Kneale's view, that this statement only asserts a regularity of actual instances and says nothing about possible but unrealized cases (cf. note 131). This statement may very well assert an historical accident, but a law must be more than the report of a factual regularity, it must also assert something about the unrealized possibilities if we want to account for counterfactual reasoning.

The example of the raven-statement, discussed earlier, amounts to the same: a law formulates a stronger then purely extensional connection (cf. SCHEFFLER in note 70).

A law, then, is a *principle of necessitation:* if '(x)(Ax→Bx)' is a law, then it is impossible for an A-thing not to be B. A law sets limits to what is possible.[159] And then it can be understood that a law is the basis of counterfactual reasoning.

The question, however, how the underlying concept of necessity or of necessary connection should be further explained is not taken very seriously by KNEALE: "In fact, the word 'necessity' is the least troublesome of those with which we have to deal in this part of philosophy. For it has the same sense as elsewhere. A principle of necessitation is a boundary of possibility, and we know quite well how possibility is bounded from consideration of such cases as the incompatibility of redness and greenness".[160] But this is not satisfactory:

the necessity of such statements as 'no red thing is also green' should first be analysed, in the way it has been done, for example, in ch. I, section 4, but KNEALE takes this necessity for granted or, as fundamental (note 160) or, as intuitively clear.

POPPER has made a much greater effort to explain physical necessity than KNEALE has done and I shall now consider their discussion.[161]

POPPER, whose objection to KNEALE is that he renders himself guilty of essentialism[162], a point that will not be considered here, also says that a law is a boundary to what is possible, or that it forbids certain events to happen.[163] He proposed the following definition of 'physically necessary': "A statement may be said to be naturally or physically necessary if, and only if, it is deducible from a statement function which is satisfied in all worlds that differ from our world, if at all, only with respect to initial conditions".[164] We may explain this using his own example which is similar to the dodo-example of KNEALE. Suppose that within a certain period of time, and only at that time, moas lived only in New Zealand, and suppose that they all died before reaching the age of fifty, now 'all moas die before reaching the age of fifty' is a true synthetic universal statement (it is strictly universal in Popper's terminology; note 99). Nevertheless, it is not a law, but an accidental universal, because, as POPPER states it, "it would be *possible* for a moa to live longer, and it is only due to *accidental* or *contingent* conditions (cf. Kneale's historical accident) – such as the co-presence of a certain virus – that in fact no moa did live longer".[165] In another possible world, different from ours only in initial conditions, e.g. a world in which the mentioned virus would not be present, this statement about moas might well be false. Only if this statement were true in all possible worlds, different from ours only in the mentioned way, would it count as an empirical law. This also explains why the moa-statement does not permit the deduction of e.g. 'if this had been a moa, it would have died before reaching the age of fifty', for this possible moa could live in a different possible world in which it could easily have reached the age of, say, seventy. Like KNEALE, POPPER also connects the concept of law with the notion of its validity for possible events.[166]

However, this definition directly confronts us with the question as to what we should understand by a 'possible world', different from ours only with respect to initial conditions. KNEALE, and also NERLICH and SUCHTING, describe these worlds as those in which the same properties and relations can be found as in our actual world, but in which the initial conditions may be different.[167] POPPER also characterises such a world as one in which the same empirical laws hold as in our world. [168]

Two objections can be raised, and have indeed been raised by NERLICH and SUCHTING[169], against such a description of a possible world. First, it does not agree with Popper's definition of 'physically necessary', just given. For suppose that in our world, W_1, the laws L are valid, that one of the initial conditions is the co-presence of a certain virus (C_1), and that the moa-statement, M, is true. In a possible world, W_2, the laws L hold, but C_1 is supposed to be false. Now what about M? When we say that M is false in W_2, then W_2 does not differ

from W_1 in the way required by the definition, i.e. only in the initial conditions, but also with respect to M. W_2 is then not a possible world as intended by Popper's definition. In fact, this definition only counts as possible worlds those worlds in which M is true, as is the case in W_1. But then M is a law, while the definition was intended to qualify M as an accidental statement. The definition appears to have no discriminating force at all.

To meet this first objection, POPPER proposed a revised definition of physical necessity[170], but this revised definition does not meet with the second objection to be examined in a moment and, as POPPER later remarked, his reply to this first objection was not satisfactory and we shall therefore not discuss this revision here.[171]

The second objection against his definition has also been stated by POPPER himself: the definition seems to become circular when we describe a possible world as one in which the same laws hold as in our actual world. For in that case the concept 'possible world' from the definiens presupposes the concept of law which, in its turn, presupposes the concept of physical necessity, which is the definiendum.[172] POPPER does not take this to be a serious objection, because in this sense all definitions are circular. Circularity is only disastrous in the case of technical circularity i.e. in cases where an understanding of some term from the definiens goes straight back to the term to be defined. And this is not the case here: "the *definiens* operates with a perfectly clear intuitive idea – that of varying the initial conditions of our world".[173] In other words, the notion of different possible or accidental or contingent initial conditions should be taken as intuitively clear and may then be used to define 'physical necessity'; or the notion of possible initial conditions may adequately do the job of a primitive. But the appeal to intuition always leaves the possibility open of the discussion partner honestly not understanding what is intended. And, to be honest, I do not share the clear intuition POPPER seems to have with respect to this varying of initial conditions. In fact, I am not even sure of what has to be understood by initial conditions themselves. Intuitively, I would say that an initial condition is a fact or event that is not physically necessary but accidental, and in that case we have to deal with a genuine technical circularity.

However, apart from the appeal to intuition, there is a more fundamental point involved, which has not received any attention as far as I know. We can make this point clear with some examples. Is 'all water boils at 212 °F' accidental because it is not true in a world in which the pressure deviates from 1 atm.; must we refuse to call 'copper is a good electrical conductor' a law, because it is not true in a world in which the temperature is very high; etc.? The possible reply that, in these cases, we have not to do with initial conditions as they are intended by POPPER, would, of course, only strengthen the point I want to make, namely that the intuitive idea of initial conditions is not quite as clear as POPPER would make us believe. If the conditions referred to in the examples are not the Popperian initial conditions, what then are they? Necessary initial conditions perhaps? But then not physically necessary, unless one is prepared to accept a technical circularity once more.

Once again we meet with the problem of initial conditions or relevant (and irrelevant) circumstances, a problem which is not solved within the physical necessity view of KNEALE and POPPER. I think there is no law that is not connected with certain specific conditions, or that does not require the fulfilment of certain relevant circumstances for its applicability. Therefore the independence from initial conditions of a statement, that is its being true in all possible worlds different from ours only in initial conditions (taking this expression for a moment for granted), can never supply a criterion of lawlikeness. I think that NAGEL, in this respect, is nearer to the truth than POPPER when he leaves room for the possibility of a statement being lawlike (e.g. 'all ravens are black'), even if it would perhaps not be true under certain conditions (e.g. in the case of the exposure of some generations of ravens to X-ray radiation of a certain intensity). But the proponents of the regularity view, including NAGEL, as well as those of the physical necessity view (e.g. KNEALE and POPPER) all fail to answer the fundamental question: What is the connection between a law and the circumstances required by that law; and how can we know this?

If the proponents of the physical necessity view do not want to acknowledge such conditions as the pressure, the temperature, the presence of enough O_2, the presence of X-ray radiation, etc., as conditions of the same kind as the presence of a certain virus in the moa-period, they should formulate a criterion of distinguishing between these two kinds of conditions without becoming involved in the unacceptable circularity. As long as this has not been done they cannot use physical necessity as a criterion of distinguishing between accidental and lawlike universals. Neither Popper's appeal to the notion of initial conditions, which should be intuitively clear, nor Kneale's reference to the incompatibility of redness and greenness, which should be intuitively clear, offers a sufficiently clear explication of the concept of physical necessity. Of course, we may, at some time, make use of concepts which are taken to be clear enough, but I think that we may make some progress in the analysis of the concept of law when we start from the notion of a necessary connection as it has been developed in ch. I. And then we do not any longer speak of a necessity in an independent reality, but of a necessity in a conceptually co-constituted empirical world. It is then such a conceptual system that provides us with a norm for judging the circumstances.

3. RESCHER'S VIEW

Rescher's view of laws[174] will be discussed separately because it introduces a new element in the discussion which I regard as important.

A lawlike statement (Rescher's term is 'lawful') can be distinguished from an accidental statement by its *nomic or physical necessity*, which manifests itself particularly in its *hypothetical force*. This necessity introduces the element of inevitability: if '(x)(Ax→Bx)' is a law and a certain thing is A, then it *must* be B. By 'hypothetical force' RESCHER refers to the fact that a law supports corresponding counterfactual conditionals.[175] This has also been said by

POPPER and KNEALE, but in connection with the question how an empirical generalisation, which, as such, is nothing else than the formulation of a de facto regularity, becomes lawlike or acquires nomic necessity and hypothetical force, RESCHER introduces the new element, mentioned before. There is neither a deductively sufficient nor an inductively sufficient evidential basis of lawlikeness: "The basic fact of the matter – and it is a fact whose importance cannot be overemphasized – is that the elements of nomic necessity and hypothetical force are not to be extracted from the evidence: they are not *discovered* on some basis of observed fact at all – they are *supplied.* The realm of hypothetical counterfact is inaccessible to observational or experimental exploration. Lawfulness is not found in or extracted from the evidence, it is superadded to it. *Lawfulness is a matter of imputation:* when an empirical generalization is designated as a law, this epistemological status is *imputed* to it".[176] This imputation comes down to *the accommodation of an empirical generalisation within the system of knowledge or within a conceptual system or theory.* At a certain moment we *decide* to rank an empirical generalisation with respect to other laws in a theoretical system. And it is this fact that renders a universal statement lawlike. The individual properties of a statement in isolation, i.e. its *form* (it must be of universal form, it must not contain individual constants essentially, etc.) and its *evidential basis* (its being confirmed by an exceptionless series of positive instances) (for these two factors cf. ch. III) are not enough. A third factor, namely its being taken up into a theory, plays a crucial role and this factor is imputed to the statement, which comes to be accepted as a law.[177] Once such a status has been imputed to an empirical generalisation, it is lawlike; it is then marked by nomic necessity and hypothetical force.

This imputation, however, is not a matter of random choice or personal predilection. The shift in meaning of a sentence of universal form from expressing an empirical generalisation to expressing a lawlike statement, or a statement of a necessary connection, is not a matter of convention or arbitrary decision. (Compare what has been said about this shift in meaning at the end of section 1. Ayer's view is clearly rejected by RESCHER). The imputation is a decision, but it must have an empirical grounding in the evidence for the empirical generalisation upon which lawlikeness comes to be imputed, and in the theoretical context in which this generalisation comes to be taken up. Lawlikeness is imputed to an empirical generalisation, which has been confirmed by an exceptionless series of positive instances, on the basis of theoretical considerations: "Lawfulness is the product of the well-founded imputation to empirical generalisations of nomic necessity and hypothetical force".[178]

By introducing this decision by which necessity and hypothetical force are superadded to an empirical generalisation, RESCHER stands in radical opposition to the regularity view, the proponents of which also had their recourse, ultimately, to a 'third factor', i.e. being accommodated in a theory of an empirical generalisation in order to count as a law (ch. III, section 3). But within R such an accommodation could never mean that that statement could acquire the character of necessity.

Let us now examine this view somewhat more closely. RESCHER himself says that his view agrees with that of HUME in that it states that lawlikeness cannot be extracted from observational evidence. An extra factor (cf. Goodman's 'organization'; note 133) is needed: the imputation, which is similar to Kant's view that lawlikeness is a matter of projection. However, RESCHER does not find the source of lawlikeness in the way in which the mind inherently works, but in *the conceptual schemata that we in fact deploy for explanatory purposes.* Lawlikeness is therefore *mind-dependent.*[179]

It is this very point of mind-dependency which is at the same time the strong and the weak point of Rescher's view. It is the strong point because it indicates the direction in which a solution of the problem of lawlikeness can be found. It is weak, not because it is untenable, but because it does not go far enough and has thereby too much of an ad hoc status. There is, in Rescher's view, a mind-independent and hence observer-independent reality, a nature which is in various respects regular 'an sich' and it would take a bold act of rashness to deny this. This regularity is an ontological fact; it is not man-made and it would be there even if there were no men or no rational minds (cf. note 5). These regularities come to be formulated in empirical generalisations as isolated and theory-independent statements (cf. ch. I, section 1).

On the other hand, laws are the result of the imputation of necessity and hypothetical force to such empirical generalisations, by which they come to be embedded in the conceptual system at hand, which is used for explanation.

Regularities are, according to RESCHER, mind-independent and theory-independent and they exist in an independent world. Laws, however, are man-made, mind-dependent and indissolubly bound to a theory; they do not exist in a mindless universe.[180] But if RESCHER now says that laws admit of no exceptions[181] (and we can agree with this), we must apparently assume that the underlying regularities, with their 'objective' status, are also without exceptions. But then RESCHER should have faced the fact that in scientific practice (and it is the same scientific practice that RESCHER appeals to in connection with hypothetical or counterfactual reasoning!), one frequently seems to encounter exceptions to a 'regularity'. When we speak of regularities as 'given' independently from our theories or concepts and we simply become aware of them in direct observation, then there seems to be hardly any exceptionless regularity. At best we could then speak of such scientifically uninteresting regularities as 'all gold cubes are smaller than 10 m^3', for these seem at the same time to be theory-independent and without exceptions. But regularities which could be taken to underly lawlike statements, such as 'all water boils at 212°F.' and 'copper is a good electrical conductor' are, taken as theory-independent statements, certainly not without exceptions. When we view this point historically, we might say that at a time when the scientific theory of boiling phenomena was not yet available, so that no accommodation of an empirical generalisation could be made, there were many exceptions to the empirical generalisation 'all water boils at 212°F.'; and before the discovery of electricity, pieces of copper sometimes conducted electricity badly (Nagel's example); etc. So, what could be

these regularities, and hence these empirical generalisations, which are taken to be empirically well confirmed by an exceptionless series of positive instances, to which RESCHER, is referring? One cannot appeal to relevant circumstances because this would imply that one already had made the statement in question lawlike or normative, so that these relevant circumstances could be discovered; or this would imply that theory formation up to a certain level had already taken place. As long as one views regularities as theory independently given things, this appeal to circumstances is forbidden.

We are drawn back to the problem of the circumstances and we find no systematic treatment of it in Rescher's theory.[182] The presupposition of there being empirical generalisations asserting a regularity in a theory-independent reality upon which, then, lawlikeness is imputed, cannot be upheld, in my view. Like all proponents of the regularity view, RESCHER takes some intuitive concept of regularity for granted and, as I have already remarked in connection with R, it is a great shortcoming of Rescher's view that it does not present an analysis of this crucial concept and thereby no analysis of the notion of relevant circumstances.

In my opinion, such an analysis would reveal that *the establishing of a regularity usually also requires the imputational step of* RESCHER. *It requires the formation of a theoretical context or the formation of a theory in which the universal statement, asserting this regularity, is then automatically taken up. It requires a theory formation to which we were forced by exceptions, taken as genuine exceptions on the basis of the universal statement which has been made lawlike or normative.* Of course, this does not lead to a restoring of a law as mind-independent or as a 'purely objective' statement. On the contrary, it leads to the view that a law asserts a regularity as a necessary connection not in an observer-independent reality, but in a conceptually co-constituted (if you like mind-dependent) empirical reality.

I think it is correct to say that my view, which will be explained in the next chapter, follows the same general direction as Rescher's, but that it is taken much further and that the result is essentially different from his view. By recognising negative instances as genuine exceptions, I try to give an answer to the Goodmannian question as to why and when the imputation of lawlikeness takes place, such an imputation implying in my view the formation of the very theory in which the law comes to be accommodated. I do not think that Rescher's view contains a satisfactory answer to this question since he does not acknowledge the all-important function of genuine exceptions.

V. THE CONCEPT OF EMPIRICAL LAW

1. LAWLIKENESS OF LAWS

In this final chapter I shall develop my own view of what an empirical law is. This will be done in a way similar to my analysis of the lawlikeness of empirical concepts explained in ch. I, sections 5 and 6. The same terminology will be used here.

I shall start the exposition using an example, which is not intended as a description of historical facts, but which may serve to clarify the general ideas I have in mind. Let us imagine a situation in which no theory about boiling phenomena is available as is the case at present. We assume the existence of a Fahrenheit thermometer. We also assume that some facts about water are known, e.g. that it is a colourless and odourless liquid, and that it is also known that a certain quantity of a liquid boils when bubbles are formed throughout the liquid.

Suppose that the temperature at which n samples of water start to boil is measured and that the results are (W = water, B^x = boils at x °F.):

$Wa_1.B^{212,\ 0}a_1$
$Wa_2.B^{211,\ 8}a_2$
$Wa_3.B^{211,\ 9}a_3$
$Wa_4.B^{212,\ 1}a_4$
$Wa_5.B^{212,\ 2}a_5$
.
.
.
.
$Wa_n.B^{211,\ 9}a_n$

When we now take an experimental error within a certain margin for granted (say, $\pm$ 0,2 °), this would seem to be the ideal result for a proponent of R. We have a constant conjunction or an exceptionless repetition of our samples being W and being B^{212}. Via a process of inductive generalisation to all cases in the past, present and future, we arrive at:

all water boils at 212 °F.
or: all W is B^{212}.

Such an empirical generalisation, based on an exceptionless repetition, is also needed in Rescher's view because it constitutes that to which lawlikeness may be imputed.

Such an exceptionless regularity, in other words, is the ideal of both the regularity view and a necessity view. But however natural this view may seem, it will prove to be entirely inadequate if we desire to understand what empirical laws are.

The statement arrived at above, i.e. 'all W is B^{212}', is very much like the

statement 'all ravens are black' or 'all gold cubes are smaller than 10 m^3', and it may at the time of its formulation very well be the expression of an historical accident, possibly on a cosmic scale. It is an empirical generalisation to which no exceptions are known and it is therefore *not armed against negative instances.* This is, I think, the point of greatest importance. What will be our reaction towards an observation of a negative instance? This is a very urgent question in connection with the analysis of the concept of empirical law. The answer of a persistent positivist or proponent of the regularity view would surely be that the empirical generalisation has been falsified, that the negative instance is a *falsification* and the regularity in nature, as it was supposed to exist, simply appears not to exist. Now, in this view the statement 'all W is B^{212}', not being armed against negative instances, becomes directly vulnerable to such a case. Hence it cannot in any way support corresponding retrodictions, predictions or counterfactuals, simply because these could supply, or could have supplied, those negative instances against which it has not yet been armed. Therefore such empirical generalisations are quite barren and scientifically uninteresting.

However, in scientific practice, negative instances play a completely different and even fundamental role (and, fortunately, the proponent of R is not as consistent as he was taken to be a moment ago, although this does imply the inadequacy of his regularity view). In order to elucidate what has just been said, let us now see what happens in the case of a negative instance.

Suppose a scientist to observe:

$Wa.B^{216}a$

When confronted with such a result the empirical generalisation is not rejected on the grounds of such an observation. The scientist does not regard this as a falsification, but as a *genuine exception, enabling him to establish 'all W is* B^{212}' *as a law, or, enabling him to arm the generalisation against negative instances.*

But to do this he first has to change the epistomological status of the statement. He *takes* it as necessary that water boils at 212°F. and on this basis he views '$Wa.B^{216}a$' as a genuine exception. In other words, he takes 'all W is B^{212}' as the formulation of a necessary connection or, *he makes this statement lawlike.*

Without such a norm he could never face his observational result as an exception; without such a norm it would count as a falsification and thereby as normal a case as e.g. '$Wa_k.B^{212}, {}^0a_k$'. He would have no problem, but then neither would he be able to discover a law.

Now this lawlikeness is imputed to a statement, to borrow Rescher's term, but we have to keep in mind that there is not, first of all, the statement of an empirical generalisation, based on an exceptionless repetition, to which, afterwards, lawlikeness is superadded. On the contrary, it is the very occurrence of a negative instance that seems to break the regularity which constitutes the occasion for the imputation of lawlikeness. Sticking to the evidence only, there is no regularity without exceptions, which RESCHER supposes to exist in a mind-independent world. The same objection could also be applied to GEURTS. There

is not, as he maintains, first of all, a synthetic universal statement of which a great number of positive instances and no negative instances are known, which is later taken to be analytic or necessary. On the contrary, observed negative instances constitute the occasion for taking the statement to be the formulation of a necessary connection. It is by means of such an imputation of lawlikeness, which is an ad hoc manoeuvre, that the scientist keeps open the road to a better understanding of the observed phenomena. If he keeps to the observational results, he would block this road and he could at best say that water very often boils at 212°F., but that it may also boil at 216°F., and that there is nothing more to say. Of course, the ad hoc conjecture that water *must* boil at 212°F. does not by itself yield this better understanding, but it is a first step towards it. *The confirmation of this lawlike statement, and thereby the establishing of an empirical law, can be gained by on-going concept or theory formation on the basis of further empirical inquiry.* Such a dynamic process should remove this ad hoc character since the conjecture is an integral part of the extended or newly formed theoretical framework. And the scientist should then be able, through observing by means of this theory, to see the earlier genuine exception *as a normal or explainable case.* If he succeeds in doing this, he has really established an empirical law which has been armed against certain negative instances and which then enables him to make retrodictions and predictions, and it forms a basis for counterfactuals.

Now, after this first step, a second step must necessarily follow, else no better understanding would be gained either and the scientist in question could just as well have taken the negative instance as a direct falsification. He then formulates a new conjecture: in order to obtain the correct (!) boiling temperature of a liquid, the liquid must be pure. In other words, he states a connection between the boiling temperature of a liquid and its purity. In other words, he claims that the purity is a *relevant circumstance* for the determination of the boiling temperature of a liquid. And indeed he finds that if sugar is dissolved in sample *a*, the result is different; it is an exception. He continues his investigation, trying to establish a quantitative relationship between the concentration of a solution (possibly a new concept) and the increase of boiling point (a new concept), a phenomenon he has now learned to observe, and which he could not observe before. He may find positive instances of this quantitative relation, but also negative ones. By taking the latter as genuine exceptions he may discover ionization phenomena, which caused the exceptional or abnormal rise in boiling point (and again he forms a new concept of new phenomena). But he will also meet with exceptions to the conjecture that the boiling point of a liquid is increased by dissolving a certain chemical compound in it (a qualitative relation): he may e.g. establish that the addition of AgCl to water does not cause an increase of the boiling point; he is bound to discover pseudo-solutions and colloids and at the same time will form a more precise concept of solution. And so on.

Negative instances, which cannot be reduced to normal cases, even when they are observed through the extended or newly formed conceptual system, may

then occur. A sample of water heated at the top of the Mont Blanc may be pure, yet it will not boil at 212 °F., but at some lower temperature. Again this is not taken as a falsification, but as a genuine exception initiating further theory formation.

A first ad hoc conjecture might be that the height is a relevant circumstance. And indeed, when the same sample is brought to the boil in a laboratory in Wageningen, it boils at 212,1 °F. There is no beaten road to 'the true' conjecture. And it may take a long time before one discovers that the conjecture leads to a piling up of ad hocness, so that it is at last rejected. One should try again and the pressure is then established as a relevant circumstance and new concepts like 'vapour pressure', 'partial pressure', etc., are formed. In such a way the result of the Mont Blanc experiment becomes normal within the context of the newly extended conceptual system. It would now even be abnormal if that sample boiled at 212 °F!

Once again negative instances which cannot be explained may be observed, e.g.

$$Wb.B^{214,\ 5}b.$$

It may take him a very long time before he is able to understand this, the water being pure and the pressure normal, but the result being abnormal.

The lawlike conjecture, however, is still not rejected on the basis of such isolated negative instances. We may have a sample of D_2O instead of H_2O, and the discovery of the hydrogen isotope deuterium is required in order to restore an understanding of this phenomenon (Urey, 1932).

We shall not continue this very fragmentary and artificial reasoning. I think that what has just been said is enough to make the idea clear, if we keep in mind what has been said in sections 5 and 6 of ch. I about the lawlikeness of concepts.

We may summarize this in a schematic way, and at the same time generalise it.

i. Knowing A and B to be mutually independent concepts, one may on the basis of

$$Aa_1.Ba_1,\ Aa_2.Ba_2,\ \ldots\ldots\ldots,\ Aa_n.Ba_n$$

by induction arrive at

All A's are B's.

As long as no theory has been formed in which this statement comes to be embedded, it has the same status as statements like 'all ravens are black' and 'all gold cubes are smaller than 10 m^3'. That is to say, it may just as well be an assertion of an historical accident on a cosmic scale; it does not supply any better understanding of the phenomena which have been observed (i.e. an exceptionless repetition) than the finite series of conjunctions does; it does not enable retrodiction, prediction or the assertion of a counterfactual because it has not been confronted with negative instances and therefore not been armed against them. Such an empirical generalisation is quite barren, it leads nowhere.

ii. Contrary to the positivist ideal of exceptionless regularities (also presupposed by proponents of a necessity view), negative instances are exactly what are needed for the formulation of an empirical law. In order to reach an understand-

ing of the observed phenomena, one should wait for, or rather look out for negative instances (in accordance with Popper's view as against the inductivists). Fortunately, exceptionless repetition is nearly always absent in scientific practice. The search for negative instances is nearly always a succesful undertaking. It is even true to say that it is usual for negative instances to be present from the very beginning.

iii. There are, therefore, two cases that do not differ in principle. First, there is a situation in which an empirical generalisation, based on an exceptionless series of positive instances plus induction, happens to be confronted with a negative instance.

Second, the situation may be such that an irregularity is observed from the beginning, and in this case there is not, first of all, an empirical generalisation. But it happens to be the case that neither the negative instances of the first case, nor the irregularity of the second, prevent the scientist from asserting a universal statement. If he were to take the negative instances of the first case as falsifications, or the irregularity of the second case as a sign of the absence of regularity, he would deprive himself of the possibility of reaching a better understanding of the phenomena he can observe through the concepts he has formed before. This would in fact mean abandoning science. He could at best arrive at unfertile empirical generalisations when adopting such a methodological point of view.

iv. The real scientist will, in the first case, ascribe lawlikeness to the generalisation or take it to be the expression of a necessary connection; and, in the second case, he will formulate a lawlike conjecture, in spite of the negative instances it has to face from the very beginning. On such a basis he will regard the negative instances as genuine exceptions. He knows that such an ad hoc manoeuvre opens the possibility to reach a better understanding.

v. It is by means of the discovery of new relevant circumstances and the formation of new concepts or the extension of the 'old' conceptual system, that the ad hoc character of the lawlike conjecture is removed. In such a way an empirical law comes to be confirmed, not as an isolated universal statement that may survive the eventual demise of the theory, but as the statement of a regularity being a necessary connection within that theory which was formed in establishing the law.

The genuine exceptions then have been turned into normal cases, explainable by means of the theory available by now, in particular by the appeal to normal or relevant circumstances which are laid down in that theory. These relevant circumstances came to be known in the same process of theory formation in which the law was established and the law and 'its' circumstances are therefore indissolubly connected within that theory.

The law is now a universal statement that has been armed against negative instances. It is not the formulation of an historical accident, possibly on a cosmic scale, but it sets a boundary to possibilities and it may thereby perform its function in scientific prediction, in retrodiction and in supporting counterfactuals.

vi. *An empirical law, then, may indeed be characterized as the formulation of a regularity, but this is a regularity that is shaped by the theory in which the law has been taken up, or, to put it more clearly, it is the formulation of a regularity as a necessary connection.*

In the same way that all empirical data are modeled (FEYERABEND) by some theory, a regularity is also modeled by a theory, viz. that theory in which the law that asserts it is embedded. The regularity can only be observed as a regularity through that theory by which it has been co-constituted. It is therefore completely inconceivable that a law could be an isolated statement. The suggestion of, e.g. NAGEL and BRAITHWAITE (compare ch. III, section 3) that a universal statement counts as a law when it "occupies a distinctive position in the system of explanations in some area of knowledge" (NAGEL) is correct. But it is also true that genuine exceptions, and therefore necessary connections, are needed *somewhere* in the on-going inquiry in order to form the very theory in which the universal statement is accommodated, which makes it a law and which lays down the relevant circumstances. Genuine exceptions enable the scientist to jump to new concepts and the formation of the required theory.

At this stage we must return to our example and examine the question when the ad hoc character of the original lawlike conjecture, i.e. that water must boil at 212 °F., can be dismissed. We have examined the analogous question in connection with the lawlikeness of colour concepts in ch. I.

Is it not true, one might ask, that only a shift of the ad hoc character to other conjectures has taken place, and hence that the original conjecture can never be taken to be definitively confirmed?

At a particular moment we met with a negative instance, '$Wa.B^{216}a$', and viewed it as a genuine exception, conjecturing that *a* was not a sample of pure water, and we called this a case of the increase of boiling point. In other words, the exception was given a name, in the same way as the exception in ch. I, section 5, was called 'colour blind' and the exception in ch. I, section 6, 'phosphorescence'. But, of course, naming by itself does not solve any problem. The on-going investigation then led to theories of ionization and colloids, a number of new phenomena was discovered and many new concepts were formed.

But does not this simply mean a shift of ad hocness? I think we should here give the same answer as that given in ch. I. When we take the theoretical system as a system of explanations for doing normal science, the answer is a definite "no". In that case, the boiling point of water is a means for determining the purity of a sample of water; the relation $\triangle T = A.\frac{C}{M}$ ($\triangle T$ = the increase of boiling point, C = concentration, M = molecular weight) is used to determine the molecular weight of a certain compound, or the degree of ionization of a compound in a certain solvent; etc.

At a particular moment, then, a conceptual system or theory acquires a relatively closed status or a great stability. It makes normal science possible by providing its norm. It enables us to observe and explain many empirical phenomena which we could not understand, and of which we could not even be aware

without this theory. Internally, to use the same term we used in ch. I, i.e. looking at the theory from within, it is a well-confirmed system of lawlike connections, excluding or forbidding certain events to happen. But externally, viewing the theory as a stage in an on-going inquiry, it has 'open ends' everywhere. This means that there is no guarantee whatsoever, that new genuine exceptions can be continually accounted for, or be explained by the system at hand or by an extension of it, i.e. without the dissolution of certain necessary connections.

Such exceptions may occur at all levels; with respect to the most advanced as well as less advanced statements.

The fact should, however, be noticed that even if we view a theory in this way, it is taken to have a relative stability; it then functions as a take-off point towards new concepts, laws and theories.

2. Some consequences of this view

I now want to compare this view with the regularity and necessity view. I shall do this by examining a few crucial concepts. For the sake of clarity, I want to formulate my view very briefly:

> an empirical law asserts a regularity as the necessary connection between two or more concepts, and hence also between the empirical data which are co-constituted by these concepts.

i. The concept of regularity.

The proponents of the regularity view make use of a concept of regularity which is not further analysed, but taken to be intuitively clear: regularities can be directly, i.e. independently of a theory, observed as a series of conjunctions. When Goodman appeals to an 'organization effected by the use of language', he does so in order to distinguish between those regularities which give rise to lawlike universal statements and those which do not. He does not appeal to an organization in order to co-constitute regularities themselves because these are taken to be given independently, using the rather naive notion of regularity.

Popper is much nearer to the truth when he says: "The kind of repetition envisaged by Hume can never be perfect; the cases he has in mind cannot be cases of perfect sameness; they can only be cases of similarity. Thus *they are repetitions only from a certain point of view.* But this means that, for logical reasons, there must always be a point of view – such as a system of expectations, anticipations, or interests – *before* there can be any repetition; which point of view, consequently, cannot be merely the result of repetition."[183] What Popper in fact says here is that a regularity is co-constituted by the theory through which it can only be observed. The regularity is present only when it is observed from the required point of view. The regularity only allows itself to be observed when it is observed in the appropriate way. But Popper's view suggests that there is, temporally, first a theory or point of view which is then followed by the regularity, while, according to my view, these should not be separated in this way. The formation of the theory which is required for the observation of a repetition is,

at the same time, the co-constitution of that regularity, as the formation of an empirical concept is, at the same time, the co-constitution of those date of which the concept is formed and to which it can be applied afterwards. Of course, such a process entails 'illogical' steps, but if one wants to do justice to the growth of science one should fully recognize such steps.

We may then say that *the regularity expressed by an empirical law is a regularity co-constituted by the theory, through which it can be observed, as a necessary connection between certain empirical data.* The organizing factor is not a means of distinguishing between two kinds of *given* regularities, but it is the factor that *co-constitutes* them. When explicated in this way, Nagel's and Braithwaite's appeal to a theory as a criterion of distinguishing between accidental and lawlike universals is justified, but I doubt whether they had such an explication in mind, since they adhere to direct or theory-free observation.

Although I agree with Rescher's view in many respects, I disagree with his presupposition of there being regularities in an observer-independent reality. I agree that lawlikeness is a matter of imputation, but this imputation has to be explicated, in my view, as the co-constitution of a regularity as a necessary connection. There is not, and I want to repeat this, first of all a given regularity, expressed in an empirical generalisation, upon which lawlikeness is then imputed by accommodating it within a theory. The co-constitution of a regularity as a necessary connection implies the formation of the theory in which the law, asserting such a regularity, occupies a distinctive position. This determines the 'epistemic commitment' we make to a law, and to which RESCHER rightly draws attention: the laws come to be armed against negative instances and they are to certain extent (see below) 'justifiably regarded as immune to rejection in the face of hypothetical considerations'.[184] Laws are indeed man-made, as RESCHER has said. But I want to add that this is true only because they assert regularities which, being necessary connections, also have to be taken as man-made.

ii. The concept of necessity.

I hope it will be clear, that the necessity, which is claimed as an essential characteristic of a law, is not a necessity in a 'mindless' universe. When I say that an empirical law asserts a necessary connection, I am referring to a necessary connection in a conceptually or theoretically co-constituted empirical world. The necessary connections laid down in empirical laws are bound to a theoretical context and thereby to particular circumstances which are called relevant by their being bound to such lawlike connections. In the empirical world it is necessary that water, under normal circumstances, boils at 212 °F., but it is equally necessary that a sample of water in which some NaCl has been dissolved boils at a higher temperature, or that a sample of water to which some AgCl has been added boils at 212 °F., or that water boils at a temperature below 212 °F. at the top of the Mont Blanc.

The necessity of laws is a necessity within the theoretical framework in which those laws are indissolubly taken up. Therefore, the negation of those laws would be completely ununderstandable or unintelligible, as HANSON says.[185] *The*

revision of a law would make a more or less drastic conceptual change necessary, so that the phenomena, which could be observed and explained by the theory of which the law is an essential part, cannot any longer be observed and explained in what way. A law cannot simply be replaced by an alternative universal statement because the system in which it occurs has not been constructed to accommodate that alternative. On the contrary, it will exclude the accommodation of such an alternative. In this sense a law is necessary, not as an isolated unit, but as a statement within a theory, and in this sense it sets limits to what is possible (KNEALE) or forbids certain events to happen (POPPER).

To accept a theory or, better perhaps, to form a theory, implies the establishing of laws as asserting necessary connections because these determine the theory to be what it in fact is. Once a theory is formed, normal science has been made possible, i.e. science guided by the theory and its laws, functioning as the norms of that activity.

Does it follow that laws are analytic statements? I think it follows, if we adopt the notion of analyticity proposed by GRICE and STRAWSON in their defence of the analytic-synthetic distinction against Quine's attack. If we adopt the notion of 'analyticity within a conceptual scheme', as it has been called by GRICE and STRAWSON, we are fully justified in calling a law an analytic statement (compare ch. IV, section 1): the revision of a law, i.e. the change of its truth-value from true to false, undoubtedly causes a conceptual change.[186] When we are forced to revise 'all water boils at 212°F.', we would also be forced to re-form the concept of 'water' and along with it many other concepts within that theory of boiling phenomena from which the boiling point of water may also be inferred from statements about the energy which are based on the structure of the liquid.

Analyticity in this sense, however, does not mean irrevisability come what may, nor the absence of any empirical or factual content. I have already mentioned this point several times and shall only briefly deal with it here. A law, as the statement of a necessary connection, has an empirical content since it co-constitutes part of the empirical world, known in science, but the results of on-going investigation ('what may come') may force us to dissolve lawlike connections and thereby to form an alternative theory co-constituting another empirical reality.

This means that we should not oppose statements about the meaning of a word, in which 'the linguistic component is all that matters'[187], to statements about facts. This opposition should be dropped.[188] Once a theory, including empirical laws, has been formed, once a relatively stable situation has been reached, "scientists will cease to distinguish between its structure and that of the phenomena themselves", as HANSON put it. An empirical law is an analytic statement and *asserts a necessary connection between empirical concepts*, but it thereby *asserts a necessary connection between the empirical data*, which can only be observed as being those very data through these concepts.

When we no longer oppose statements about the meaning of words to statements about facts by emphasizing that the empirical world, known in science, is

always a conceptually or theoretically co-constituted world, we are able to see how a statement can have empirical content and at the same time be necessary. The question "How could a statement be logically necessary or analytic and at the same time have empirical content?" only becomes rhetorical (as it is taken to be, e.g., by AYER, HAMLYN, WAISMANN) when the opposition between 'logical' and 'factual' is presupposed.

We may now say that a law like 'copper is a good electrical conductor', to use Nagel's example, asserts that conductivity is a defining property of copper. This, however, should not be taken as a matter of convention. We are not free to settle the matter as we please, as AYER would make us believe. On the contrary, a scientist has to make the assumption that certain connections are necessary or lawlike if he wants to leave open the possibility for science to be a dynamic undertaking. If he maintains the possibility of our understanding the phenomena, he needs necessary connections and it can, therefore, not be a matter of 'take it or leave it'. The result of on-going investigation also confirms, *with retrospective effect*, the necessary connections that hold in the empirical world. We are, for example, not free to choose between 'it is necessary that water consists of H_2O' and 'it is not necessary that water consists of H_2O' (Ayer's example). We must choose the first alternative, else an integral part of our theories would be radically undermined. What would it, e.g., mean to talk about the boiling point of water when we are allowed to say that water consists of H_2O and Na^+ and Cl^- ions? What would be the sence of the statements about the cohesion energy of water? Many phenomena would then indeed become unintelligible. The 'method of conventionalizing' does not solve any methodological problem. In a way similar to the colour-blind person who is someone who does not understand what certain concepts mean, and not someone who refuses to join a convention (ch. I, section 5), a person who denies that water consists of H_2O is someone who does not understand certain parts of scientific theory and not someone who refuses to join a convention.

When we say that a law asserts a necessary connection, does it mean that it is true in all possible worlds different from ours, if at all, only as regards initial conditions? If such a possible world is a world in which only a few of the relevant circumstances of the law, known by means of the theory in which it is taken up, differ from those in the actual world, the answer should obviously be "Yes". For a law is that very statement that has been armed against possible negative instances brought about by a change of relevant circumstances. The theory in which the law has its place, is *a theory about all such possible worlds.* It is the theory that determines what these possible and impossible worlds are. A world in which water does not boil at 212°F. or in which copper is not a good electrical conductor is impossible, provided that the relevant circumstances are taken into account. By binding the law to the relevant circumstances, a theory determines a set of possible worlds, and these worlds should not be imagined as being theory-independent. This means that we cannot explain the concept of law using the concept of possible world.

iii. The concept of confirmation.

My view on confirmation and falsification has already been stated in this inquiry. I shall now briefly summarize it and add some remarks about singular empirical conditionals, regardless of whether they be expressed in the indicative or the subjunctive mood.

A law, being the formulation of a regularity as a necessary connection, comes to be *confirmed as such* by the success of on-going theory formation. It is established as a law by the removal of the original ad hoc character, which means the formation of a theory that enables us to face the former genuine exceptions as normal cases, or the restoration of the possibility of normal science, i.e. science governed by this theory. Confirmation is, in this way, clearly a process of feedback; it is realized in such a way as to have a retrospective effect.

Examples of such a way of confirmation, in which negative instances taken as genuine exceptions play a fundamental role, are well known. Leverrier's prediction and discovery of Neptune to explain the genuine exception constituted by the abnormal (!) orbit of Uranus[189] constituted an excellent confirmation of the law that planets move in elliptical orbits around the sun, provided that no disturbing (!) forces are present, in which case the orbit cannot (or may not) be elliptical. Other examples are the discovery of certain chemical elements, using the theory of the periodical system as a norm; the discovery of the meson on the basis of the laws of conservation; Eddington's experiment as a confirmation of Einstein's theory; and the artificial case of the discovery of the increase of the boiling point on the basis of our paradigm law.[190]

But there is no guarantee at all that on-going investigation will always lead to a positive result confirming the former ad hoc conjecture. An on-going investigation may remain without such a required result, as it may lead to mere piling up of ad hoc conjectures. Instead of reducing the exceptions to normal cases, more and more ad hoc conjectures have to be added leading to a result which is no longer intelligible. Instead of reaching a better understanding of the observed phenomena, any further investigation leads to a state of complete incomprehensibility. And in a way similar to the success of an on-going investigation leading to a firm belief in the theory where we cannot but conceive and observe the empirical world to be as it is co-constituted by that theory, the failure of an on-going investigation makes us lose our confidence in the theory and thereby in the necessary connections asserted by the laws belonging to that theory.

And here the genuine exceptions start to function as falsifications. An empirical law may be *falsified.* This means that *the necessary connection may be dissolved* by the failure to reduce genuine exceptions to normal cases. What may come may then force us to give up the previously accepted conceptual framework, because this, at a certain time, may be the only way to restore the understanding of the phenomena considered. Sticking to the previously accepted theory may, in many cases, block the road to a better understanding. Evolutionary science will then not do any longer because a revolutionary alternative theory is needed to restore normal science. The accumulation of ad hoc conjectures to save the ether theory against the result of the Michelson-Morley

experiment is a good example: the contraction hypothesis of Lorentz and Fitzgerald remained an ad hoc conjecture.

It should be noticed, however, that necessary connections are not dissolved and that the theory is not abandoned before the alternative, referred to above, has been formulated. Only then can a genuine exception be taken as a falsification. Mercury's unexplained orbit, for example, existed for a long time alongside classical mechanics. Evolutionary science, although successful in the case of Uranus, did not lead to an understanding of this phenomenon. The conjecture of the existence of Vulcan could only be maintained by the formulation of additional ad hoc conjectures. Nevertheless, the theory was not dropped on this basis and only Einstein's theory could bring an end to this situation by restoring understanding through a new conceptual framework. We may again quote HANSON, as we did in ch. I: "Should someone claim that he has a good reason for abandoning a theory T, but can suggest no alternative to T, no other way to form concepts about the phenomena T covers, I deny that he has good reasons for abandoning T'."[191]

Now, what are the consequences of this view of confirmation and falsification for the analysis of singular empirical conditionals in general (compare ch. III, section 2)? Such an analysis should make it clear what the grounds are on which a decision about the truth or falsity of these statements is founded.

When we now try to do this, using

if something is water then it boils at 212 °F. (I)

as a law and taking

if this here is water then it boils at 212 °F. (II)

as an example of a singular empirical conditional, the meaning of the concept 'if-then' must be examined first. I want to propose the following: *the empirical concept 'if-then'*, i.e. the concept 'if-then' as it is used in empirical reasoning, *is a means for asserting a lawlike connection.*

The concept 'if-then' in general, may be used to express quite different things. It may be used to express a relation of entailment, or the making of a promise, or for threatening somebody, etc.

In all cases the concept 'if-then' serves to convey that the parts connected by it (whether or not these could truly be called statements) 'have something to do with each other'. In a singular empirical conditional this notion of 'having to do something with each other' can be explicated as 'connected in a lawlike manner within a certain conceptual system'. The empirical concept 'if-then' serves to tell us that the parts connected by it in an empirical conditional are lawlikely connected, if that conditional is true. This, now, is an intentional connection that cannot be rendered truth-functionally. The empirical concept 'if-then' may be said to have been designed to express such a lawlike connection.

Hence, a singular empirical conditional is true when the lawlike connection which is intended by it, and which is applied by it to a particular case, obtains; and it is false when the intended connection does not obtain. Based on such an analysis it is completely irrelevant whether the conditional happens to be expressed in the indicative or in the subjunctive mood.

Now (II) is true, because the lawlike connection intended by (II), and applied to 'this here', obtains since (I) is a law. But, e.g.,

if this here is water then it boils at 190°F.

is false, since the intended connection does not obtain: it is excluded by (I).

It is interesting to compare some remarks quoted previously about the analysis of counterfactuals with what has just been said: "the truth of a statement of this kind depends not upon the truth or falsity of the components but upon whether the intended connection obtains" (GOODMAN, quoted in ch. III, section 2, with respect to counterfactuals); "a counterfactual is true if a certain connection obtains between the antecedent and the consequent" (GOODMAN again, cf. note 116); "A subjunctive conditional is one such that we can know that the antecedent in some sense implies the consequent without knowing the truth-values of either" (CHISHOLM, note 116). All these remarks are valid for singular empirical conditionals in general, in which 'if-then' should be taken to express that 'intended connection', or that 'certain connection', or the 'in some sense implying', which has been identified as a lawlike connection, explicated in section 1 of this chapter.

But what could then be the role of experimental evidence, e.g., of the so-called positive and negative instances?

Let us consider different cases. Suppose the observation

$$Wa.B^{212}a \qquad \text{(III)}$$

to be true. We can infer from this

$$Wa \rightarrow B^{212}a$$

and which is thus also true. But we cannot infer (II) from the conjunction (III).

When we look upon this conjunction as the assertion of two isolated observational data, it does not have any confirmatory force as to the empirical conditional (II), let alone the law (I), but when we face it within the context of the theory in which (I) is taken up, taking care of the relevant circumstances as required by the law (I), we can say that our observational result is *in conformity with* the law (I); not that it confirms, but illustrates this law.

Suppose we now observe

$$\sim Wa \qquad \text{(IV)}$$

to be true. We can logically infer from this the following:

$$Wa \rightarrow B^{212}a$$
$$Wa \rightarrow B^{100}a$$
$$Wa \rightarrow \sim B^{212}a$$

etc.

These logical consequences are also true. But we cannot infer (II) from (IV). This piece of evidence has no confirmatory force at all, nor could it be regarded as an illustration of (I), because that law has not been applied here. (II) is true because the intended connection obtains, which is formulated in (I), and, e.g.,

if Wa then $B^{100}a$

is false, because the intended connection does not obtain, since it is excluded by (I), although the corresponding material implication, stated above, is true.

Suppose that we now observe

$Wa.\sim B^{212}a$

to be true. Taken in isolation, this piece of evidence again does not have any confirmatory or falsifying force. When it is taken in the context of the theory of boiling phenomena, which means that we are taking into account the relevant circumstances, and when this does not enable us to view the result as a normal case, we should say that the observational result is not in conformity with the law. It constitutes a genuine exception which leads to further theory formation. Of course, many 'abnormal' results of experiments are disqualified as mistakes, and this manoeuvre again stresses the normativity of the theory, but obstinate cases cannot be 'explained away' in this manner (cf. the bad teacher who continues to 'explain' his pupil's answers, deviating from his own, by saying that the pupil has not yet formed the appropriate concepts) and they must lead to further investigation. The result of our observation has been expressed in the concepts of our theory, but it is nevertheless 'systematically unintelligible'. Whether this result, i.e. this obstinate case, will at last turn out to be a falsification or a normal case within an extended conceptual framework can, in general not be decided in advance.

In this way we are back at the notions of confirmation and falsification with which we started our examination of the concept of confirmation. It should now be clear that, at the 'singular level', our concept 'if-then' has the following consequences: first, a singular empirical conditional, e.g. (II), cannot be inferred from a conjunction, i.c. (III), while a material implication, e.g. '$Wa \rightarrow B^{212}a$', does follow; second, an empirical conditional may be false, while the corresponding material implication is true.

This concept 'if-then', however, also has consequences at the 'universal level'. The statement

all gold cubes are smaller than 10 m^3,

taken as the formulation of an exceptionless repetition, can be reformulated as

$(x)(Gx \rightarrow Sx)$

(G = gold cube, S = smaller than 10 m^3)

but it is not equivalent to

if something is a gold cube then it is smaller than 10 m^3

because the latter is the statement of a regularity as a necessary connection, which has not been established.

On the other hand it is correct to say that from a law the corresponding all-statement can be inferred. We can for example infer from (I) that

all water boils at 212° F.

The transition from an all-statement to an empirical law, however, requires a process of theory formation by which a law is formed as the expression of a regularity being a necessary connection. In the same process the law is armed against negative instances.

We may, therefore, conclude that the distinction between accidental and lawlike universal statements can be reduced to this transition.

SAMENVATTING

De hedendaagse wetenschapsfilosofie ('philosophy of science') is voor verreweg het grootste deel ontwikkeld binnen een logisch positivistische wijsgerige context. Dit heeft met zich meegebracht, dat de problemen, die in de wetenschapsfilosofie aan de orde zijn, én de oplossingen die voor die problemen worden voorgesteld en acceptabel geacht, vrijwel steeds het stempel dragen van de wijsgerige uitgangspunten van het logisch positivisme. Het probleem van en de discussie rond het begrip 'empirische wet' vormen hiervan een treffend voorbeeld.

Men kan echter heel duidelijk een andere benaderingswijze in de wetenschapsfilosofie onderkennen, waarin van andere wijsgerige stellingen wordt uitgegaan. In verband hiermee wordt wel gesproken van een post-positivistische wetenschapsfilosofie.

In deze studie wordt getracht een analyse van het begrip 'empirische wet' te geven vanuit deze post-positivistische context. Voordat deze analyse ter hand kan worden genomen, moet eerst een korte schets van de logisch positivistische context worden gegeven en een uitwerking van de post-positivistische.

I

Uitgaande van de notie van directe of theorie-vrije waarneming, welke fundamenteel geacht moet worden voor het logisch positivisme, wordt een model geschetst van de logisch positivistische context in de vorm van een vijftal stellingen. Dit model, dat sterk simplificerend is en niet als het resultaat van een historische analyse mag worden gezien, dient als hulpmiddel bij het systematisch onderzoek, dat hier wordt ondernomen. Pas daarin krijgt dit model zijn betekenis.

De genoemde stellingen luiden:

i. Er is een waarnemer-onafhankelijke werkelijkheid, waarop de directe of theorie-vrije waarneming is betrokken.

ii. In die werkelijkheid bestaan geen noodzakelijke verbanden tussen de elementen. Alleen contingente, tijd-ruimtelijke relaties tussen die elementen worden erkend. De elementen zijn als zodanig onderling onafhankelijk.

iii. Waarnemingstermen zijn geïsoleerde of theorie-onafhankelijke betekeniseenheden en hun betekenis moet als zuiver extensioneel worden gekarakteriseerd. Alleen dan kunnen deze termen de dubbelrol spelen, waartoe ze in een logisch positivistische methodologie zijn voorbestemd, te weten beginpunten van betekenis en eindpunten van confirmatie zijn.

iv. Waarnemingsuitspraken hebben eveneens een geïsoleerde of theorie-onafhankelijke status. In het bijzonder geldt dit voor empirische (i.t.t. theoretische) wetten: hun waarheidswaarde en betekenis ligt vast onafhankelijk van de theorieën waarin ze eventueel zijn opgenomen.

v. De elementaire, extensionele logica vormt een adequaat middel voor kennistheoretisch en methodologisch onderzoek. Het spreken over intensionele bete-

kenis en intensionele verbanden is overbodig (soms: zinloos) binnen dit terrein van wijsgerig onderzoek; het extensionele kader is dan ook toereikend.

In het werk van o.a. HANSON, FEYERABEND en KUHN, en vooral in de dissertatie van GEURTS, wordt de notie van theorie-geladen waarneming tegenover die van een theorie-vrije waarneming gesteld.

Een tweetal consequenties hiervan is:

i. De empirische werkelijkheid en de elementen daaruit kunnen niet als waarnemer onafhankelijke grootheden worden gekwalificeerd. Als alle waarneming theorie-geladen is, dan is datgene wat wordt waargenomen ook steeds theoretisch mede bepaald.

ii. Elk empirisch begrip, hoe 'eenvoudig' ook, bezit betekenis door het intensioneel verbonden zijn met meer of minder andere empirische begrippen. Als zodanig kan elk begrip een theorie in miniatuur worden genoemd. Aangezien dan het empirische begrip niet als een geïsoleerde betekeniseenheid kan worden getypeerd, kan het ook niet meer de eerder genoemde dubbelrol spelen.

Het post-positivisme kan in eerste benadering door deze twee stellingen worden gekenmerkt. Een uitwerking hiervan richt zich primair op de notie van theorie-geladen waarneming en bouwt voort op een aantal gedachten, ontwikkeld door GEURTS. Waarneming van empirische verschijnselen is altijd waarneming op een bepaalde, gerichte wijze. Anders gezegd: waarneming is pas mogelijk als het een op de vereiste wijze theoretisch geladen waarneming is. Waarneming is steeds waarneming 'door de geschikte begrippen heen', waarbij die begrippen als miniatuur theorie fungeren.

Begripsvórming is dan ook primair een leren waarnemen; het is de vorming van die theorie die een bepaalde waarneming mogelijk maakt. Iemand, die een empirisch begrip heeft gevormd, heeft de (miniatuur) theorie gevormd waardoorheen een bepaalde vorm van waarnemen mogelijk is. Tegelijk echter heeft hij een stukje empirische werkelijkheid ontdekt waarvan hij eerder geen weet had. Van dit stukje werkelijkheid kan dan ook alleen maar op zinvolle wijze gezegd worden dat het bestaat, binnen de betreffende begripscontext.

Nadere analyse laat zien, dat het empirische begrip, hoe 'eenvoudig' het ook moge lijken, steeds een wetmatig karakter bezit, waardoor het idee van het begrip als miniatuur theorie kan worden gepreciseerd. Dit wetmatig karakter is uiteraard niet gerechtvaardigd per se – het vereist empirische confirmatie, die niet als een confirmatie d.m.v. positieve gevallen kan worden omschreven. Een analoge wijze van confirmatie treffen we aan bij empirische wetten (zie onder V).

II

Op Goodman's z.g. 'new riddle of induction', dat in de discussie rond het begrip 'empirische wet' steeds een belangrijke rol speelt, wordt apart ingegaan. Als inductieprobleem heeft het enige consekwenties, die het onderzoek van het begrip 'empirische wet' uitermate bemoeilijken. Een analyse van het probleem leert echter, dat het als inductieprobleem niet formuleerbaar is, indien we tenminste Goodman's expositie serieus nemen: het is primair het probleem van de keuze tussen twee concurrerende theorieën.

Als het niet gaat om het onderzoek van verschijnselen van principieel statistische aard, zoals in Goodman's voorbeelden, dan kan men in het empirische begrip een wetmatige trek ontdekken, die we met een voorbeeld kunnen verduidelijken.

Onderdeel van de betekenis van het begrip 'groen' is:

> als iets groen is op tijdstip t_0, dan is het groen (zou het groen zijn; zou het groen geweest zijn) op enig ander tijdstip t_n, mits alle relevante omstandigheden gelijk zijn (gelijk zouden zijn; gelijk zouden zijn geweest).

Wanneer Goodman's probleem wordt geformuleerd als een conflict tussen een spreker van de 'gewone' groen-blauw-taal, L_1, en een spreker van de grauw-bloen-taal, L_2, dan blijkt, dat beide gebruik maken van een dergelijke wetmatige trek in hun eigen begrippen.

Gebruik makend van de door BARKER en ACHINSTEIN voorgestelde wijze van definiëren, moet dan worden geconcludeerd, dat het conflict tussen beide sprekers van begripsmatige aard is en zich afspeelt op het niveau van de singulaire beschrijvingen van wat ze waarnemen. Er is, met andere woorden, sprake van een conflict tussen twee begripssystemen of theorieën – de groen-blauw-theorie en de grauw-bloen-theorie – die in L_1 resp. L_2 kunnen worden uitgedrukt. Er is daarom geen sprake van een inductieprobleem, maar van het probleem van theoriekeuze. Beide sprekers nemen geheel verschillende dingen waar, omdat hun waarneming door verschillende theorieën heen plaatsvindt. Om vanuit zo'n conflictsituatie te komen tot een voor beide aanvaardbare theorie, spelen heel andere factoren een rol dan het feitelijk taalgebruik, waarop GOODMAN een beroep doet om zijn probleem, als inductieprobleem, tot een oplossing te brengen. Sterker, zo'n beroep op het gewone taalgebruik zal steeds leiden tot de keuze van de oude, vertrouwde theorie en biedt daarmee een nieuweling geen kans.

III

De regelmaatsopvatting van een empirische wet kan als volgt worden geformuleerd:

> een empirische wet is een ware, synthetische, universele uitspraak, die een regelmaat zonder uitzonderingen onder woorden brengt.

Het centrale probleem binnen deze opvatting is steeds de vraag hoe wetmatige universele uitspraken kunnen worden onderscheiden van accidentele universele uitspraken. En dát dit als het centrale probleem wordt gezien impliceert, dat begrippen als 'waar', 'synthetisch', 'regelmaat' en 'uitzondering' als voldoende duidelijk worden geaccepteerd.

Alle voorgestelde syntactisch-semantische criteria – een empirische wet is een universele uitspraak, die geen onelimineerbare individuconstanten bevat, die alleen zuiver kwalitatieve predicaten bevat, die een in principe niet-gesloten toepassingsbereik heeft – lijden schipbreuk.

Hoewel een wetmatige universele uitspraak corresponderende z.g. 'counterfactual conditionals' steunt, d.w.z. mede de basis vormt waarop de waarheidswaarde van zo'n 'conditional' kan worden vastgesteld, terwijl een accidentele uit-

spraak dat niet doet, kan dit feit toch ook niet als het gezochte criterium dienst doen. Een analyse van deze 'counterfactual conditionals' zelf leidt namelijk tot het stellen van twee onderling samenhangende vragen: i. Wat is een empirische wet? en ii. Wat moet onder relevante of normale omstandigheden worden verstaan? Nader onderzoek leidt er echter toe om het probleem van de 'counterfactual conditionals' in een veel breder verband te stellen: ook de analyse van 'gewone' empirische als-dan uitspraken ('singular indicative conditionals') – zoals die bij een voorspelling, verklaring of retrodictie worden gebruikt – loopt uit in de zojuist geformuleerde vragen. Het probleem van de 'counterfactual conditionals' is slechts een onderdeel van het probleem van de analyse van 'singular conditionals' in het algemeen. Dat de analyse van 'counterfactual conditionals' steeds als een apart probleem is gezien, is een gevolg van de opvatting als zouden 'gewone' empirische als–dan uitspraken adequaat zijn geanalyseerd binnen een extensioneel-logisch kader.

De laatste poging van de voorstanders van de regelmaatsopvatting, die wij hier onderzoeken, kan aldus worden geformuleerd: een universele uitspraak is wetmatig als hij een positie binnen een theorie bekleedt. Gegeven dit criterium, kan men recht doen aan de functie van een wet in het wetenschappelijk redeneren, i.e. in het doen van voorspellingen, het geven van verklaringen en het formuleren van 'counterfactual conditionals'.

Hoewel dit criterium, mits nader uitgewerkt, mijns inziens inderdaad kans van slagen biedt, is het binnen de logisch positivistische context waarin de regelmaatsopvatting wordt verdedigd, nauwelijks acceptabel. In de eerste plaats is de analyse van het begrip 'empirische wet' nu vervangen door een analyse van het begrip 'theorie', en in het bijzonder door de vraag wat het betekent dat een wet een positie bekleedt binnen zo'n theorie. Aan de vraag hoe de relatie is tussen een wet en de 'bijbehorende' relevante omstandigheden is in dit verband echter nog nauwelijks aandacht besteed en het is juist deze relatie, die opheldering zou moeten krijgen vanuit het gebonden zijn van een wet aan een theorie. In de tweede plaats en geheel afgezien van het eerste, is de theorie-binding in strijd met de basis-gedachte, dat een empirische wet in principe een theorie-onafhankelijke grootheid is, die een regelmaat zonder uitzonderingen formuleert. Het ontbreken van een analyse van dit begrip 'regelmaat', dat in feite als een intuïtief voldoende duidelijk begrip wordt gehanteerd, moet als één van de belangrijkste obstakels op weg naar de oplossing van het probleem van de empirische wet worden gezien. Het volledig voorbijzien van de essentiële rol, die uitzonderingen spelen – juist uitzonderingen leiden tot de formulering van relevante omstandigheden en daarmee tot de vorming van de theorie waarin een wet een positie bekleedt – is hiervan het gevolg.

IV

De noodzakelijkheidsopvatting kan in eerste benadering worden geformuleerd als:

een empirische wet brengt een noodzakelijk verband onder woorden.

Hierbij is dan de explicatie van het begrip 'noodzakelijk verband' het centrale

probleem geworden in de analyse van het begrip 'empirische wet'.

Wanneer dit verband wordt geïnterpreteerd als een logisch noodzakelijk verband – een empirische wet is een analytische uitspraak – dan werkt men weliswaar met een duidelijk begrip 'noodzakelijk', maar dan is tegelijk volkomen duidelijk, dat empirische wetten niet in déze zin een noodzakelijk verband uitdrukken. Aldus tenminste de tegenstanders van een dergelijke noodzakelijkheidsopvatting. Immers, zo vervolgen zij, dit zou in de eerste plaats betekenen, dat de negatie van een wet een contradictie zou opleveren – quod non – en in de tweede plaats, dat een wet nooit gefalsificeerd zou kunnen worden en dus geen enkele empirische inhoud zou bezitten, wat geheel onacceptabel is.

Toch is de verwerping van deze noodzakelijkheidsopvatting, op basis van een als volledig doorzichtig opgevat noodzakelijkheidsbegrip, niet overtuigend. Quine's aanval op het onderscheid analytisch-synthetisch wordt dan immers geheel genegeerd. En vooral de verdediging door GRICE en STRAWSON van dit onderscheid tegen QUINE, maakt een dergelijke verwerping dubieus. Wanneer we met Grice en Strawson het onderscheid in déze zin handhaven, dat binnen een begripssysteem of theorie wel van analytische en synthetische uitspraken kan worden gesproken, daarbij de mogelijkheid van verwerping van de theorie als geheel open latend – hetgeen betekent, dat we met Quine's belangrijkste punt van kritiek instemmen – dan moet ook de mogelijkheid, dat een empirische wet in déze zin analytisch zou kunnen zijn, opnieuw aan een onderzoek worden onderworpen. Dit onderzoek is deste belangrijker, omdat het kan worden verricht in aansluiting aan de vraag waarop we bij de regelmaatsopvatting uiteindelijk zijn gestuit: Wat betekent het dat een wet een positie bekleedt binnen een theorie? Bovendien wordt het belang van dit onderzoek nog benadrukt door het feit – en ook aanhangers van de regelmaatsopvatting vermelden dit feit – dat een universele uitspraak, als gevolg van voortgaande theorievorming, kennelijk de status van analytische uitspraak kan verkrijgen. Het dynamisch moment, waarop dan wordt gewezen, zal blijken van zeer groot belang te zijn.

Een analyse van het begrip 'noodzakelijk' als 'fysisch noodzakelijk' is met name door KNEALE en POPPER ondernomen. Een wet moet in hun ogen een noodzakelijkheidskarakter bezitten, omdat hij anders nooit de basis kan vormen voor het waar-zijn van een corresponderende 'counterfactual conditional'. Zelfs als we zouden weten, dat de uitspraak 'alle raven zijn zwart' (Kneale's voorbeeld) een feitelijke regelmaat zonder uitzonderingen formuleert, dan nog zou dit een accidentele uitspraak kunnen zijn, die geen basis biedt voor b.v. de uitspraak 'als deze raaf een antarctica-bewoner zou zijn, dan zou hij zwart zijn'. Het waar-zijn van de raven-uitspraak, m.a.w. het zwart-zijn van alle raven, kan een historisch toeval zijn: als het toevallig is, dat er nooit raven in poolstreken hebben gewoond, dan is het waar-zijn van 'alle raven zijn zwart' mogelijk afhankelijk van deze toevallige omstandigheid en in dat geval moet deze uitspraak accidenteel worden genoemd. In tegenstelling tot wat in genoemde 'counterfactual conditional' wordt gesteld, zou een antarctica-raaf waarschijnlijk wit zijn geweest.

Een universele uitspraak is pas een wet, wanneer zijn waarheid onafhankelijk

is van de toevallige begin-omstandigheden van onze actuele empirische werkelijkheid. Of, wanneer hij waar is in alle mogelijke werelden, die hoogstens van onze wereld verschillen voor wat betreft begin-omstandigheden. Hiermee stuiten we andermaal op de kwestie van de omstandigheden en het blijkt, dat de explicatie van het begrip 'noodzakelijk' in termen van 'mogelijke werelden' en 'begin-omstandigheden' nauwelijks enige verheldering biedt. Kneale's beroep op een intuïtief duidelijk noodzakelijkheidsbegrip doet dat evenmin.

RESCHER introduceert echter een nieuw en m.i. belangrijk element in het onderzoek van de empirische wet. Een wet heeft inderdaad een noodzakelijkheidskarakter, dat zich vooral manifesteert in de mogelijkheid om 'counterfactual conditionals' waar of onwaar te noemen. Dit karakter heeft een wet echter niet op grond van waargenomen feiten, maar omdat het is aangebracht door de onderzoeker. Dit aanbrengen van noodzakelijkheid betekent in feite het toekennen aan een empirische generalisatie van een plaats in een voorhanden theorie. Het is het begripssysteem of de theorie waarin een empirische generalisatie wordt opgenomen, waaraan de wet zijn noodzakelijkheidskarakter ontleent.

RESCHER handhaaft echter het bestaan in een onafhankelijke realiteit van een waarnemer-onafhankelijke regelmaat, die eerst wordt uitgedrukt in een empirische generalisatie, waarop dán, later, het stempel van noodzakelijkheid wordt gedrukt door die generalisatie in een theoretisch kader in te passen. Juist hierdoor ontneemt hij zichzelf de mogelijkheid om een antwoord te geven op de vragen, die t.a.v. zijn theorie direct rijzen: Wat betekent het 'ingepast te worden in een theorie'? Waarom wordt een universele uitspraak op een gegeven moment ingepast en waarom juist dán?

Wanneer RESCHER niet zou zijn uitgegaan van het bestaan van waarnemer-onafhankelijke regelmatigheden, dan zou hij mogelijk hebben ontdekt, dat reeds het vaststellen van een regelmaat zelf een 'imputation' vereist, en dat de theorie-binding reeds vanaf dit moment aanwezig is.

V

In tegenstelling tot de regelmaatsopvatting stellen wij, dat een waargenomen regelmaat-zonder-uitzonderingen niét de ideaal-basis vormt waarop een empirische wet, door inductieve generalisatie, tot stand komt. Van een universele uitspraak die wél op deze wijze tot stand komt, en waarvan dus alleen positieve en geen negatieve gevallen bekend zijn, kan nooit worden beslist of hij wetmatig dan wel accidenteel is. Een dergelijke uitspraak moet, wanneer zich een negatief geval voordoet, als gefalsificeerd worden beschouwd: deze uitspraak is immers op geen enkele wijze gewapend tegen een negatief geval, dat dan ook slechts als falsificatie kan worden bestempeld.

In het wetenschappelijk onderzoek spelen negatieve gevallen echter een geheel andere en zelfs fundamentele rol. Veelal hebben we in dat onderzoek niet te doen met een 'exceptionless repetition', die het ideaal van de aanhanger van de regelmaatsopvatting is. In het onderzoek wordt desondanks een hypothese geformuleerd van de vorm:

alle A zijn B.

Dan zijn er vanaf het begin negatieve gevallen t.o.v. deze aangebrachte regelmaat aanwezig. Die negatieve gevallen krijgen niet een falsificerende functie, maar worden als uitzonderingen bestempeld. Dit laatste betekent, dat de hypothese als norm wordt gebruikt of als de formulering van een noodzakelijk verband. Om de negatieve gevallen als uitzonderingen te kunnen kwalificeren heeft men een dergelijke normering nodig. Erkent men dit niet, dan is de enige mogelijkheid om de negatieve gevallen, veelal vanaf het begin van het onderzoek aanwezig en soms pas later, als falsificaties te zien. Men komt dan óf überhaupt niet toe aan de formulering van een universele uitspraak, óf tot de verwerping van de uitspraak waarvan totnutoe alleen positieve gevallen bekend waren.

De hypothese is nu de uitdrukking van een regelmaat áls noodzakelijk verband, maar heeft in deze fase uiteraard een sterk ad hoc karakter. De verificatie van deze hypothese, en daarmee het tot stand komen van een wet, die een regelmaat als noodzakelijk verband uitdrukt, bestaat nu juist uit het doen verdwijnen van dit ad hoc karakter. En het moge duidelijk zijn, dat deze vorm van verificatie niet kan plaatsvinden op basis van positieve gevallen. Deze vorm van verificatie wordt gerealiseerd door middel van voortgaande theorievorming (hetzelfde geldt voor de verificatie van het wetmatig karakter van een empirisch begrip).

Stel dat iemand in een periode, waarin geen theorie over kookverschijnselen, laat staan een theorie over de structuur van vloeistoffen, voorhanden is, de volgende hypothese formuleert:

alle water kookt bij 212 °F.

Met deze hypothese als norm zal hij sommige waarnemingsresultaten als uitzondering kwalificeren. Zijn hypothese wordt nu geverifieerd, of als wet gevormd, wanneer hij er door voortgaande theorievorming in slaagt deze uitzonderingen te begrijpen als normale of verklaarbare verschijnselen. De ene uitzondering zal begrepen kunnen worden binnen een theorie over het verband tussen druk en kookpunt; de andere binnen een theorie over kookpuntsverhoging, ionisatie, etc.; een derde misschien binnen een theorie over isotopen, waarin het bestaan van b.v. de waterstofisotoop deuterium wordt gesteld.

Voortgaande theorievorming leidt aldus tot de vorming van een wet en in die voortgaande theorievorming kunnen de eerder bestaande uitzonderingen als normale gevallen worden begrepen, zodat het ad hoc karakter van de oorspronkelijke hypothese verdwijnt.

In een dergelijk proces komt een wet tot stand en tegelijkertijd betekent het de vorming van de theorie waarin die wet een bepaalde positie bekleedt. Binnen die theorie liggen nu tevens de relevante omstandigheden vast, die bij de gevormde wet behoren (b.v. voor 'alle water kookt bij 212 °F.', dat de druk 1 atm. moet zijn, dat de vloeistof niet met b.v. NaCl verontreinigd mag zijn, maar soms wel met AgCl, etc.).

Wanneer een wet op deze wijze is gevormd, dan is hij gewapend tegen verschillende negatieve gevallen. Het is nog sterker: die negatieve gevallen zijn nu, binnen het gevormde theoretisch systeem, even normaal als positieve gevallen (dat een hoeveelheid zuiver water op de top van de Mont Blanc bij een lagere

temperatuur dan 212°F. kookt, is precies even normaal als het koken bij 212°F. van een zuiver watermonster in een laboratorium in Wageningen).

Een wet brengt dus inderdaad een regelmaat onder woorden, maar deze regelmaat is niet een waarnemer-onafhankelijk gegeven; die regelmaat komt tot stand binnen en is pas waarneembaar door de theorie waarin de wet onlosmakelijk een positie bekleedt; een wet formuleert een regelmaat áls noodzakelijk verband.

De essentiële functie van de uitzondering, die de aanzet vormt tot voortgaande theorievorming en die daarmee in belangrijke mate de dynamiek van het onderzoek bepaalt (vgl. KUHN), hebben wij op deze wijze geprobeerd recht te doen.

Is het echter niet zo, dat nieuwe uitzonderingen steeds mogelijk blijven? En is het dán ook niet zo, dat het genoemde ad hoc karakter nooit geheel verdwijnt? In verband met deze vragen moet Kuhn's onderscheid tussen 'normal science' en 'revolutionary science' worden gemaakt. Normale wetenschapsbeoefening vindt plaats binnen een geaccepteerde theorie, die ons die wetenschapsbeoefening juist normaal doet noemen. In dit geval fungeert de theorie als norm voor de waarneming, als norm voor de omstandigheden, als kader waarbinnen de begrippen 'waar' en 'onwaar' worden gebruikt en als verklaringssysteem.

Revolutionair onderzoek wordt daarentegen juist gekenmerkt door de poging om tot de vorming van een alternatieve theorie te komen op grond van het optreden van nieuwe uitzonderingen, die men maar steeds niet kan gaan begrijpen binnen de 'oude' theorie. Met andere woorden, als men zich geconfronteerd ziet met een opstapeling van ad hoc hypotheses, waardoor die 'oude' theorie in discrediet geraakt.

Binnen deze vorm van onderzoek speelt juist de mogelijkheid van het optreden van nieuwe uitzonderingen een belangrijke rol en binnen dit onderzoek worden juist de vertrouwde normen of wetmatige verbanden ter discussie gesteld.

Tenslotte wordt, vanuit mijn omschrijving van het begrip empirische wet, een analyse gegeven van de begrippen 'regelmaat', 'noodzakelijkheid', 'confirmatie' en 'singular conditional'.

NOTES

1. See for a short exposition of these views R. S. WALTERS 'Laws of science and lawlike statements'. (18, vol. 4, p. 410)*
2. The term stems from Shapere's 68.
3. See Rescher's 66.
4. I tried to show this in 48, where I also sketched the five theses, which are formulated here.
5. In a recent article about lawfulness, RESCHER clearly states this in connection with regularities in nature: "No doubt nature is in various respects regular – it would take a bold act of rashness to deny that! And this regularity of nature in various respects is no doubt an ontological fact that would remain unaltered in the face of any hypothetical removal of rational minds from within its purview." (66, p. 192) What RESCHER states here about regularities is a fortiori valid for empirical data, I think. The opposition between an independent reality and the observer is quite clear.
6. E.g. REICHENBACH: "Although we emphasize the indispensability of synthetic connective operations, we should like to make it clear that we reject any kind of mysticism in the interpretation of these relations. We do not wish to say that physical necessity is due to invisible forces tying things together (. .). We agree with Hume that physical necessity is translatable into statements about repeated occurrences, including the prediction that the same combination will occur in the future, without exception. 'Physical necessity' is expressible in terms of 'always'." (62, p. 356)
7. E.g. HEMPEL: "all that a causal law asserts is that any event of a specified kind, i.e. any event having certain specified characteristics, is accompanied by another event which in turn has certain specified characteristics". (36, p. 253).
   Or: "It (i.e. a statement of universal form, H.K.) is a statement to the effect that whenever and wherever conditions of a specified kind F occur, then so will, always and without exception, certain conditions of another kind, G." (39, p. 54)
   And AYER: "Once we are rid of the confusion between logical and factual relations, what seems the obvious course is to hold that a proposition expresses a law of nature when it states what invariably happens." (2, p. 220)
   BRAITHWAITE: "Scientific laws will be taken as asserting no more (and no less) than the *de facto* generalizations which they include; the law that every hydrogen atom consists of one proton together with one electron will be interpreted as meaning that, as a matter of fact, every hydrogen atom, past, present and future, has this constitution." (9, p. 10)
   And again REICHENBACH: "Since repetition is all that distinguishes the causal law from a mere coincidence, the meaning of a causal relation consists in the statement of an exceptionless repetition – it is unnecessary to assume that it means more." (63, p. 158)
8. NAGEL: "(an experimental law) retains a meaning that can be formulated independently of the theory; and it is based on observational evidence that may enable the law to survive the eventual demise of the theory." (51, p. 86)
9. Cf. Blumberg's explication of an interpretation of the first-order predicate calculus, L_P: "an interpretation I of L_P (*a*) specifies a non-empty domain of individuals or objects D as the domain of interpretation, (*b*) assigns to each individual constant of L_P as its denotation an individual member of D, and (*c*) assigns to each *n*-place predicate constant of L_P a class of (if $n = 1$), or an *n*-ary relation among, the members of D." (18, vol. 5, p. 29)
10. "Now, if confirmation is to be feasible at all, this process of referring back to other predicates must terminate at some point. The reduction must finally come to predicates for which we can come to a confirmation directly, i.e. without reference to other predic-

* References are to the BIBLIOGRAPHY, p. 127.

ates. (. .) the observable predicates can be used as such a basis." (12, p. 64–5)

11. 68, p. 119.
12. 49, p. 102.
    And: "Since new paradigms are born from old ones, they ordinarily incorporate much of the vocabulary and apparatus, both conceptual and manipulative, that the traditional paradigm had previously employed. But they seldom employ these borrowed elements in quite the traditional way. Within the new paradigm, old terms, concepts, and experiments fall into new relationships one with the other. The inevitable result is what we must call, though the term is not quite right, a misunderstanding between the two competing schools." (ibid., p. 149)
13. "Introducing a new theory involves changes of outlook both with respect to the observable and with respect to the unobservable features of the world, and corresponding changes in the meanings of even the most 'fundamental' terms of the language employed. (. .) scientific theories are ways of looking at the world; and their adoption affects our general beliefs and expectations, and thereby also our experiences and our conception of reality." (23, p. 29)
14. 55, p. 425.
15. 34, p. 15 and 19.
    We may add that observation is not only "a systematic exposure of the senses to the world; it is also a way of thinking about the world, a way of forming conceptions." (ibid., p. 30)
    In 60, RUTH ANNA PUTNAM has drawn attention to the fact that HANSON did not make a sharp distinction between 'seeing' and 'observing' and that this obscures what he really means. In all cases when I quote HANSON, I have taken him to say something about observing. I agree with Ruth Anna Putnam's remark that "it is plausible to assume that he (i.e. Hanson, H.K.) meant, at least often, 'observe' (when he used 'see', H.K.). For some things which he says about seeing become true when they are taken as claims about observing." (60, p. 499)
16. "To a layman entering an atomic physics laboratory it would not be at all obvious that certain pairs of scintillations over here are connected with the presence of a nuclear reaction over there, and that they confirm a theory which explains why certain other scintillations near the reactor have the character they have. But to the physicist they count as the detection of neutrinos whose presence was suspected because of a loss of energy in certain decay processes in the reactor." (40, p. 18) MARY HESSE concludes with: "Each entity is observed in the ways appropriate to it". (ibid., p. 27)
    HANSON has also given this example and he ended up with the remark that "The layman must learn physics before he can see what the physicist sees." (34, p. 16)
17. GEURTS tried to analyse the notion of theory-loaded observation (although he does not like the term) in his dissertation 'Het ervaringsgegeven in de natuurwetenschappen' ('Experience in science'), Utrecht, 1971.
18. FEYERABEND: "Experimental evidence does not consist of facts pure and simple, but of facts analysed, modeled and manufactured according to some theory." (23, p. 50–1)
19. 24, p. 180. Somewhat further, FEYERABEND says: "The philosophies we have been discussing so far (i.e. versions of empiricism, H.K.) assumed that observation sentences are meaningful *per se*, that theories which have been separated from observation are not meaningful, and that such theories obtain their interpretation by being connected with some observation language that possesses a stable interpretation. According to the view I am advocating the meaning of observation sentences is determined by the theories with which they are connected". (ibid., p. 213)
    And in general: "the interpretation of an observation-language is determined by the theories which we use to explain what we observe, and it changes as soon as those theories change." (22, p. 163)
    Cf. POPPER: "not only the more abstract explanatory theories transcend experience, but even the most ordinary singular statements. For even ordinary singular statements are always *interpretations of 'the facts' in the light of theories*. (And the same holds for 'the

facts' of the case. They contain *universals*; and universals always entail a *law-like* behaviour.)" (55, p. 423)
Somewhere else POPPER remarks that "the customary distinction between '*observational terms*' (or '*non-theoretical terms*') and *theoretical terms* is mistaken, since all terms are theoretical to some degree, though some are more theoretical than others". (58, p. 119)

20. HANSON: "If seeing were just an optical-chemical process, then nothing we saw would ever be relevant to what we know (it would not constitute confirming evidence, H.K.), and nothing known could have significance for what we see. Visual life would be unintelligible; intellectual life would lack a visual aspect." (34, p. 26)
And MARY HESSE: "The conditions given for phenomenal statements are not sufficient for them to have scientific significance (e.g. in confirmation, H.K.), and if they are to have such significance, there must be connections of meaning between them at a higher than common-sense level, and therefore the condition of complete theoretical independence between them must be dropped." (40, p. 16)
21. CARNAP: "One of the most important distinctions between two types of laws in science is the distinction between what may be called (there is no generally accepted terminology for them) empirical laws and theoretical laws. Empirical laws are laws that can be confirmed directly by empirical observations. The term 'observable' is often used for any phenomenon that can be directly observed, so it can be said that empirical laws are laws about observables." (14, p. 225)
And NAGEL: "each descriptive term in an experimental law L has a meaning that is fixed by an overt observational or laboratory procedure." (51, p. 84)
22. 51, p. 85.
23. This example has originally been given by UBBINK in his lectures at Utrecht in 1966–1967. GEURTS has also mentioned it (26, p. 139), but neither of them worked it out. POPPER has given an analogous example (55, p. 421), but he did not work it out either.
24. "Perhaps the most striking feature of the normal research problems we have just encountered is how little they aim to produce major novelties, conceptual or phenomenal." (49, p. 35)
25. "If one asks why it has for so long been considered necessary to accept the comparatively unproblematic character of the observation predicates and statements in contrast to those of theories, there seem to be two replies, neither of which has been made very explicit. First, the pragmatic account of observability is only a cover for a much more deeply rooted belief, stemming from phenomenalism and the British empiricist tradition, that there are entities (and their properties) which are directly given in perception and that more or less well-defined areas of language directly describe these. (. .) Second, a half-conscious fear of vicious circularity has inhibited investigation of alternative accounts of the observation language. If we have no firm observation basis on which to stand, how can we begin to analyze the meaning and justification of theories that are erected upon observables? Worse, if observation itself is said to share the uncertainties of theories in any important degree, how can we avoid being sucked into a logical vortex in which we lose all contact with empirical evidence?" (42, p. 103)
26. GEURTS has also warned against such a separation: "It is correct to say that the observational datum is co-determined by the context through which it is observed, but one should be careful not to conceive this theoretical connotation itself as a 'datum' in its turn. The theoretical connotation we are talking about, represents the structuring activity; it brings about a pattern, if you like, thanks to which the observational datum is *constituted*. This theoretical connotation is a constitutive part of the datum in this sense, that it contributes to its formation." (26, p. 77; my translation)
27. See for a more detailed answer Geurts' criticism of the sense-datum theory, 26, p. 31–61.
28. 57, p. 47.
29. 55, p. 31.
30. Compare Popper's attack upon 'the primacy of repetitions': "Generally, similarity, and with it repetition, always presuppose the adoption of *a point of view*". (55, p. 421) It is, in my view, precisely this 'point of view' which comes to be known in concept formation

as a learning of how one should observe. The 'how' is the 'point of view', intended by Popper.

31. The element of 'ordering' is fundamental in Geurts' view: "Concept formation comes about in an ordering", as he expressed it. (26, p. 142; my translation) But he did not connect 'the learning how to observe' with 'the learning how to order'.
32. "The concept is a possibility to order, and it is as such only a position that can be occupied", as GEURTS stated in connection with the universal character of empirical concepts. (26, p. 151; my translation)
33. 33, p. 38.
34. This is also the way of defense of the 'dogma' by GRICE and STRAWSON: "The point of substance that QUINE is making, by this emphasis on revisability, is that there is no absolute necessity about the adoption or use of any conceptual scheme whatever (. .). But it is one thing to admit this, and quite another thing to say that there are no necessities within any conceptual scheme we adopt or use". (30, p.158) I shall come back to this later, in connection with the necessity view of empirical laws.
35. The difficulty of revising sociological concepts is clearly illustrated by NORBERT ELIAS in his 'Was ist Soziologie?'. The 'Wissenschaftstheorie', ELIAS opposes to is, by the way, a rather unrealistic one and I think that all of us would reject it.
36. The idea of the lawlikeness of a concept can be found at different places in Popper's 55: "By the word 'glass', for example, we denote physical bodies which exhibit a certain *law-like behaviour*". (55, p. 95)
    Or: "all universals are dispositional" (ibid., p. 424) in which 'universal' may be identified with my 'concept' and 'dispositional' with 'lawlike'.
    Cf.: "universal terms (. .) entail dispositions to behave in a law-like manner, so that they entail universal laws". (ibid., p. 425)
37. "my view of the matter, for what it is worth, is that there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process. My view may be expressed by saying that every discovery contains 'an irrational element', or 'a creative intuition' in Bergson's sense." (55, p. 32) In my opinion, however, POPPER has dismissed of an inquiry into concept formation too quickly. See also my remarks at the end of section 3.
38. 34, p. 30. Hanson's quotations of Bragg and Herschel are also very illustrative in connection with this point (ibid., p. 183–4) and in his 33 we get a clear illustration of concept formation as a learning to observe appropriately.
39. 33, p. 32.
40. 54, p. 22.
41. CARNAP mentioned both, normal instruments and normal circumstances, in connection with the confirmation of a full sentence of, e.g., the predicate 'red': "According to the explanation given, for example the predicate 'red' is observable for a person N possessing a normal colour sense (which requires a lawlike character of 'red' in my view, H.K.). For a suitable argument, namely a space-time-point c sufficiently near to N, say a spot on the table before N, N is able under suitable circumstances (and this again requires 'red' to be a lawlike concept, H.K.) – namely, if there is sufficient light at c – to come to a decision about the full sentence 'the spot c is red' after few observations namely by looking at the table." (12, p. 64)
    I have worked out the consequences of a concept being lawlike for the distinction between theoretical and observational terms in 48, in particular with respect to Carnap's view.
42. In the sense in which POPPER described it: "It is the simple rule that we are not to abandon the search for universal laws and for a coherent theoretical system, nor ever give up our attempts to explain causally any kind of event we can describe." (55, p. 61)
43. "it is most important to realize that the recourse to probability laws under such circumstances (i.e. the analysis of radiative phenomena, H.K.) is essentially different in aim from the familiar application of statistical considerations as practical means of accounting for the properties of mechanical systems of great structural complexity. In fact, in quantum physics we are presented not with intricacies of this kind, but with the inability of

the classical frame of concepts to comprise the peculiar feature of indivisability, or 'individuality', characterizing the elementary processes." (8, p. 203)
Again and again BOHR stressed this 'individuality' of atomic processes, which marks the essential difference between classical and modern physics: "a wholly new situation in physical science was created through the discovery of the universal quantum of action, which revealed an elementary feature of 'individuality' of atomic processes." (7, p. 313)
And: "in quantum mechanics, we are not dealing with an arbitrary renunciation of a more detailed analysis of atomic phenomena, but with a recognition that such an analysis is *in principle* excluded. The peculiar individuality of the quantum effects presents us, as regards the comprehension of well-defined evidence, with a novel situation unforeseen in classical physics and irreconcilable with conventional ideas suited for our orientation and adjustment to ordinary experience." (8, p. 235)
It is precisely this individuality, that determines quantum mechanics as a principally statistical theory: "In fact, the specification of a state of a physical system evidently cannot determine the choice between different individual processes of transition to other states, and an account of quantum effects must thus basically operate with the notion of the probabilities of occurrence of the different possible transition processes. We have here to do with a situation which is essentially different in character from the recourse to statistical methods in the practical dealing with complicated systems that are assumed to obey laws of classical mechanics." (7, p. 313)

44. "This novel feature (of individuality, H.K.) is not only entirely foreign to the classical theories of mechanics and electromagnetism, but it is even irreconcilable with the very idea of causality." (7, p. 313)
Or: "the impossibility of subdividing the individual quantum effects and of separating a behaviour of the objects from their interaction with the measuring instruments serving to define the conditions under which the phenomena appear implies an ambiguity in assigning conventional attributes to atomic objects which calls for a reconsideration of our attitude towards the problem of physical explanation. In this novel situation, even the old question of an ultimate determinacy of natural phenomena has lost its conceptional basis, and it is against this background that the viewpoint of complementarity presents itself as a rational generalization of the very ideal of causality." (7, p. 317)
And also: "Indeed the *finite interaction between object and measuring agencies* conditioned by the very existence of the quantum of action entails – because of the impossibility of controlling the reaction of the object on the measuring instruments, if these are to serve their purpose – the necessity of a final renunciation of the classical ideal of causality and a radical revision of our attitude towards the problem of physical reality." (8, p. 232–3)
In such a situation Mr. A's concept of change cannot be applied to quantum effects, because this would require a further analysis of these effects, which is forbidden by their individuality.

45. EINSTEIN: "What does not satisfy me in that theory (i.e. statistical quantum theory, H.K.) from the standpoint of principle, is its attitude towards that which appears to me to be the programmatic aim of all physics: the complete description of any (individual) real situation (as it supposedly exists irrespective of any act of observation or substantiation) (. .) for such a complete description there is no room in the conceptual framework of statistical quantum theory". (20, p. 668)
And: "For me, however, the expectation that the adequate formulation of the universal laws involves the use of *all* conceptual elements which are necessary for a complete description, is more natural (. .). Assuming the success of efforts to accomplish a complete physical description, the statistical quantum theory would, within the framework of future physics, take an approximately analogous position to the statistical mechanics within the framework of classical mechanics. I am rather firmly convinced that the development of theoretical physics will be of this type; but the path will be lengthy and difficult". (ibid., p. 672)
Cf. 19, where EINSTEIN has also defended the thesis of the incompleteness of quantum mechanics and the possibility of a further analysis of quantum phenomena. In Einstein's

view, the classical concept of change, as it is used by our Mr. A, remains applicable in principle to quantum effects.

46. In his 'Studies in the logic of explanation' HEMPEL used the universal statement 'all apples in basket *b* are red' as an example of an accidental statement, which cannot be used as a premise in his model of a D-N-explanation. (36, p. 226)
47. 46, p. 123. His example runs as follows: "The fact, if it is a fact, that no ravens have lived in very snowy regions may be only an accident of history, and so too the fact, if it is a fact, that there has never been a raven that was not black." (ibid., p. 123)
KNEALE wanted to stress the possibility that 'all ravens are black' is the expression of an historical accident on a cosmic scale, so that it is not a lawlike, but an accidental universal. Cf. chapter IV, section 2.
48. See 35, especially sections 3 and 6. HARRÉ introduced a 'hypothetical mechanism' as a 'candidate for reality'. An 'ontological experiment' may prove this mechanism to be the real one and in this way its ad hocness, as I would call it, can be removed.
49. CARNAP: "To be real in the scientific sense means to be an element of the framework; hence this concept cannot be meaningfully applied to the framework itself." (11, p. 210–1)
The internal question, i.e. the question about the existence of certain entities within a framework, is a genuine theoretical question, but the external question of the choice of that framework is a practical question: "The acceptance or rejection of abstract linguistic forms (. .) will finally be decided by their efficiency as instruments, the ratio of the results achieved to the amount and complexity of the efforts required." (ibid., p. 228)
The difference between Carnap's internal-external distinction and mine lies in the fact that in my view the choice of a theory is not at all as non-obligatory as CARNAP takes it to be, unless he takes 'linguistic framework' in a purely syntactical sense, but this is hardly compatible with what he says in his 'Testability and meaning', where he offers an epistemological – as opposed to pragmatic – argument to choose the thing-language: "According to these considerations, it seems to be preferable to choose the primitive predicates from the predicates of kind 1, i.e. of the observable thing-predicates. These are the only intersubjectively observable predicates." (12, p. 80)
This cannot be said to be a practical reason of the kind quoted above. The choice of a theory or a conceptual system is made on the basis of empirical evidence, the predominant point being whether it enables us to understand or explain what can be observed through that theory. The instrumental efficiency is a derived aspect.
50. 38, p. 11.
51. 28, p. 74.
52. 28, p. 74.
53. "We have so far neither any answer nor any promising clue to an answer to the question what distinguishes lawlike or confirmable hypotheses from accidental or non-confirmable ones; and what may at first have seemed a minor technical difficulty has taken on the stature of a major obstacle to the development of a satisfactory theory of confirmation. It is this problem that I call the new riddle of induction." (28, p. 80–1)
54. "True enough, if we start with 'blue' and 'green', then 'grue' and 'bleen' will be explained in terms of 'blue' and 'green' and a temporal term. But equally truly, if we start with 'grue' and 'bleen', then 'blue' and 'green' will be explained in terms of 'grue' and 'bleen' and a temporal term (. .). Thus qualitativeness is an entirely relative matter and does not by itself establish any dichotomy of predicates." (28, p. 79–80)
55. 69, p. 128.
56. Goodman's remark that "The reason why only the right predicates happen so luckily to have become well entrenched is just that the well entrenched predicates have thereby become the right ones." (28, p. 98) cannot be taken as an adequate answer, of course.
57. BARKER and ACHINSTEIN proposed the following definition of 'grue': "(it) applies to a thing at a given time if and only if either the thing is then green and the time is prior to t_0, or the thing is blue and the time is not prior to t_0". (4, p. 511)
See for Blackburn's formulation 5, p. 140.

GOODMAN criticised the proposal of BARKER and ACHINSTEIN in 29, but he did not object to their definition.

58. 41, p. 15, under B.
59. 41, p. 16.
60. 28, p. 74–5.
61. Compare, e.g., SMALL, who stated that, in order to uphold the symmetry, we have to suppose that there are human beings "who have a quite astonishing ability to look at a thing and say straight off whether it is grue or bleen without first determining whether it is blue or green and what their temporal location is." (70, p. 548)
    But SMALL forgets that Green must have the same astonishing ability in Grue's view in order to establish that a thing is green.
62. 75, p. 388–9.
63. Compare for the argument in the text N. RESCHER 'Hypothetical Reasoning', Amsterdam, 1965. RESCHER has made use of this kind of reasoning in 65 and 66, where he considered the lawlikeness of laws, not that of concepts.
64. "Plainly 'green', as a veteran of earlier and many more projections than 'grue', has the more impressive biography. The predicate 'green', we may say, is much better *entrenched* than the predicate 'grue'." (28, p. 94)
65. In 44 KAHANE has drawn attention to the fact that one must appeal to the meanings of the predicates in question in order to make sense of the notion of conflict between those predicates: "one might claim that 'all emeralds are green' and 'all emeralds are grue' conflict because if something is grue after time t_0 then it is blue, and it follows from the meanings of the terms 'blue' and 'green' that if somewhing is blue it cannot be green. My own personal inclination is to accept this line of reasoning, but for many philosophers it is forbidden. In particular, Goodman cannot accept it, since he denies the validity of the synthetic-analytic distinction, and it is only be means of that distinction that one can know without inductive evidence that if something is blue it cannot be green." (44, p. 380–1)
    I agree with KAHANE, but I have not paid much attention to this point in my examination.
66. 41, p. 25.
67. "Unless Grue has some such long and non-trivial story available Goodman's puzzle in practice is spurious." (41, p. 23)
68. 41, p. 25.
69. NAGEL, 51, p. 85.
70. SCHEFFLER proposed the following explanation of this theory-independence of experimental laws: "The main proposal I should like to make here (. .) is that we reconstrue the possible meaning-independence of experimental laws, and indeed of observational formulations generally, in terms of reference rather than sense or connotation. Experimental or observational laws, for example, formulate relationships of one or another sort among the classes of elements denoted by their constituent terms. The denotations or references of these terms in specific cases can, as we have earlier stressed, be determined independently of a characterization of their respective senses. (. .) Now it follows from such a view of the matter that the absorption of experimental or observational laws into different theoretical frameworks is compatiblew ith their constancy of referential interpretation." (67, p. 61–2)
    It may also be clear from this quotation, that the terms in an experimental law, as a theory-independent statement, have a purely extensional meaning, i.e. they are the names of classes.
71. CARNAP: "no sharp line can be drawn accross this continuum (of terms, H.K.); it is a matter of degree (. .). Individual authors will draw the line where it is most convenient, depending on their points of view, and there is no reason why they should not have this privilege." (14, p. 226)
    And: "One of the most important distinctions between two types of laws in science is the distinction between what may be called empirical laws and theoretical laws. (. .) empirical laws are laws about observables." (ibid., p. 225)

NAGEL: "the distinction between experimental laws and theories is not a sharp one, and (. .) no precisely formulated criterion is available for identifying the statements to be classified under these rubrics" (51, p. 106), but "Despite the admitted vagueness of the distinction under discussion, we shall see that it is an important one." (ibid., p. 83)

72. 63, p. 157.
73. 2, p. 220.
74. 39, p. 54.
75. 2, p. 220.
76. GOODMAN: "For convenience, I shall use the term 'lawlike' for sentences that, whether they are true or not, satisfy the other requirements in the definition of law. A law is thus a sentence that is both lawlike and true". (28, p. 22)
77. HEMPEL: "What distinguishes genuine laws from accidental generalizations? This intriguing problem has been intensively discussed in recent years." (39, p. 55–6)
Or: "The characterization of laws as true lawlike sentences raises the important and intriguing problem of giving a clear characterization of lawlike sentences without, in turn, using the concept of law. This problem has proved to be highly recalcitrant". (37, p. 338)
And GOODMAN: "to define this distinction is a decilate matter" (28, p. 38) and "We have so far neither any answer nor any promising clue to an answer to the question what distinguishes lawlike or confirmable hypotheses from accidental or non-confirmable ones". (ibid., p. 80)
78. STEGMÜLLER has given a detailed survey of these attempts (71, p. 300–314) and NAGEL has discussed them in 51, ch. 4.
79. NAGEL: "the accidental universal contains designations for a particular individual object and for a definite date or temporal period, while the nomological universal does not." (51, p. 57)
GOODMAN: "The most popular way of attacking the problem takes its cue from the fact that accidental hypotheses seem typically to involve some spatial or temporal restriction, or reference to some particular individual. (. .) Complete generality is thus very often supposed to be a sufficient condition of lawlikeness; but to define this complete generality is by no means easy." (28, p. 77)
80. PAP: "Let us call expressions by which we designate particular objects, times, or places *individual constants*, and predicates by means of which we talk about repeatable qualities or relations and that are not defined in terms of individual constant *purely general*. And an individual constant will be said to occur essentially in a statement p if it occurs in p and p is not translatable without change of meaning into a statement in which it does not occur. As a first approximation one might then define a lawlike generalization as a synthetic universal statement in which no individual constants occur essentially." (53, p. 293)
HEMPEL: "a fundamental lawlike sentence must be of universal form and must contain no essential – i.e. uneliminable – occurrences of designations for particular objects." (36, p. 268)
81. REICHENBACH: "An *individual-term* is a term which is defined with reference to a certain space-time region, or which can be so defined without change of meaning. The term can be a proper name or a definite description." (64, p. 32)
Then: "A synthetic statement is *universal* if it cannot be written in a reduced form which contains an individual-term." (ibid., p. 33)
Now, for p to be a fundamental lawlike statement, "The statement p must be universal." (ibid., p. 40)
82. According to GOODMAN, 'all grass is green' may be equivalently reformulated as 'all grass in London and elsewhere is green' and his conclusion that "to exclude all hypotheses that have *some* equivalent containing such a term is to exclude everything" (28, p. 78) is correct in his context, but cannot be directed against the amended criterion in my text.
AYER does not speak about uneliminable constants either if he says that "as Professor

Nelson Goodman has pointed out, generalizations of law can themselves be expressed in such a way that they contain a reference to particular individuals, or to specific places and times." (2, p. 227)

83. 62, p. 361.
84. 36, p. 267.
85. NAGEL: "a distinction is introduced between 'fundamental' and 'derivative' lawlike statements. Ignoring fine points, a universal conditional is said to be fundamental if it contains no individual names (or 'individual constants') and all its predicates are purely qualitative; a universal conditional is said to be derivative if it is a logical consequence of some set of fundamental lawlike statements". (51, p. 57)
    And PAP: "a *fundamental* lawlike statement (is a) synthetic universal statement in which no individual constants occur essentially, and (. .) a *derivative* lawlike statement (is) one that is deducible from a fundamental lawlike statement though it contains individual constants essentially." (53, p. 293)
86. Cf. NAGEL, 51, p. 58, note 12. See also 53, p. 294.
87. See his 'Postcript' to 36.
88. See 50.
89. 51, p. 58.
90. Cf. GOODMAN, 28, p. 20; HEMPEL, 36, p. 268; NAGEL, 51, p. 59.
91. GOODMAN: "The next step, therefore, has been to consider ruling out predicates of certain kinds. A syntactically universal hypothesis is lawlike, the proposal runs, if its predicates are 'purely qualitative' or 'non-positional'." (28, p. 78)
92. HEMPEL: "the idea suggests itself of permitting a predicate in a fundamental lawlike sentence only if it is purely universal, or, as we shall say, purely qualitative, in character; in other words, if a statement of its meaning does not require reference to any one particular object or spatio-temporal location." (36, p. 268)
    POPPER also pointed to this distinction between, in his terminology, individual and universal concepts: "An individual concept is a concept in the definition of which proper names (or equivalent signs) are indispensible. If any reference to proper names can be completely eliminated, then the concept is a universal concept." (55, p. 66)
93. HEMPEL: "The stipulation just proposed (cf. note 92, H.K.) suffers, however, from the vagueness of the concept of purely qualitative predicate. The question whether indication of the meaning of a given predicate in English does or does not require reference to some specific object does not always permit of an unequivocal answer since English as a natural language does not provide explicit definitions or other clear explications of meaning for its terms." (36, p. 269)
    And GOODMAN: "The claim appears to be rather that at least in the case of a simple enough predicate we can readily determine by direct inspection of its meaning whether or not it is purely qualitative. But even aside from obscurities in the notion of 'the meaning' of a predicate, this claim seems to me wrong. I simply do not know how to tell whether a predicate is qualitative or positional, except perhaps by completely begging the question at issue and asking whether the predicate is 'well-behaved'– that is, whether simple syntactically universal hypotheses applying it are lawlike." (28, p. 79)
94. 61, p.22. Quine's allergy for intensions can be found in many of his articles e.g. 'On what there is', 'The problem of meaning in linguistics' and 'Two dogmas of empiricism', all contained in his 'From a logical point of view'.
95. 27, p.4,
96. "That there is such a distinction to be drawn at all is an unempirical dogma of empiricists, a metaphysical article of faith." (61, p. 37)
97. NAGEL: "let us call a universal whose scope of predication is not restricted to objects falling into a fixed spatial region or a particular period of time an 'unrestricted universal'. It is plausible to require lawlike statements to be unrestricted universals." (51, p. 59)
    STRAWSON: "It is customary to devide empirical general statements into restricted and unrestricted generalizations; or into generalizations about closed classes and generalizations about open classes; or, we may put it, into general reports and forecasts on the

one hand, and laws on the other." (73, p. 198)

98. HEMPEL: "Surely a lawlike sentence must not be *logically* limited to a finite number of instances: it must not be logically equivalent to a finite conjunction of singular sentences, or, briefly, it must be of *essentially generalized form*." (37, p. 340)
99. POPPER: "We can distinguish two kinds of universal synthetic statement: the 'strictly universal' and the 'numerically universal'. It is the *strictly universal statements* which I have had in mind so far when speaking of universal statements – of theories or natural laws. The other kind, the numerically universal statements, are in fact equivalent to certain singular statements, or to conjunctions of singular statements, and they will be classed as singular statements here. (. .) Formal logic (including symbolic logic), which is concerned only with the theory of deduction, treats these two statements alike as universal statements ('formal' or 'general' implications)." (55, p. 62)
100. NAGEL: "It must also be noted that, though a universal conditional is unrestricted, its scope of predication may actually be finite. On the other hand, though the scope is finite, the fact that it is finite must not be inferrible from the term in the universal conditional which formulates the scope of predication, and must therefore be established on the basis of independent empirical evidence." (51, p. 59)
101. 51, p. 59.
102. 50, p. 139.
103. 50, p. 137.
104. Cf. PAP: "We have not succeeded in capturing any *formal* characteristic of lawlike hypotheses that would reliably distinguish them from accidental statements." (53, p. 301) And POPPER: "there may be *true, strictly universal statements* which have an accidental character rather than the character of true universal laws of nature. Accordingly, the characterization of laws of nature as strictly universal statements is logically insufficient and intuitively inadequate." (55, p. 428)
105. NAGEL: "This *prima facie* difference between accidental and nomic universality can be briefly summarized by the formula: A universal of law 'supports' a subjunctive conditional, while an accidental universal does not." (51, p. 52)
AYER has given the following example: "If it is a law of nature that the planets move in elliptical orbits, then it must not only be true that the actual planets move in elliptical orbits; it must also be true that if anything were a planet it would move in an elliptical orbit." (2, p. 229)
CHISHOLM: "Both law and nonlaw statements may be expressed in the general form, 'For every x, if x is S, x is P'. Law statements, unlike nonlaw statements, seem to warrant inference to statements of the form 'If *a*, which is not S, *were* S, *a* would be P' and 'For every x, if x *were* S, x would be P'." (16, p. 97)
And finally RESCHER: "In accepting 'all A's are B's' as a law, we have to be prepared to accept the conditional 'If x were an A, then x would be a B'. It is preeminently this element of hypothetical force that distinguishes a genuinely lawful generalization from an accidental generalization like 'all coins in my pocket weigh less than one half ounce.' ". (66, p. 179)
HEMPEL suggested that GOODMAN also wanted to make the distinction via the support to counterfactuals: "Goodman has pointed out a characteristic that distinguishes laws from such nonlaws: The former can, whereas the latter cannot, sustain counterfactual and subjunctive conditional statements." (37, p. 339)
This, however, suggests too much. GOODMAN posed the question: "Is there some way of so distinguishing laws from non-laws, among true universal statements of the kind in question, that laws will be the principles that will sustain counterfactual conditionals?" (28, p. 20) and he then seeks a criterion.
106. NAGEL: "What has come to be called the 'problem of counterfactuals' is the problem of making explicit the logical structure of such statements and of analyzing the grounds upon which their truth or falsity may be decided." (51, p. 71)
107. "we need to invoke the concept 'lawlike generalization' in order to explain how a counterfactual conditional can be asserted with warrant; hence it would be running around in a

circle to define a lawlike generalization as a universal statement that warrants a counterfactual conditional." (53, p. 291)

108. GOODMAN: "The analysis of counterfactual conditionals is no fussy little grammatical exercise. Indeed, if we lack the means for interpreting counterfactual conditionals, we can hardly claim to have any adequate philosophy of science. A satisfactory definition of scientific law, a satisfactory theory of confirmation or of disposition terms would solve a large part of the problem of counterfactuals. Conversely, a solution of the problem of counterfactuals would give us the answer to critical questions about law, confirmation, and the meaning of potentiality." (28, p. 3)
And CHISHOLM: "Given a method of treating the subjunctive, it may then be possible to throw light, not only upon disposition predicates, but also upon such notions as 'law', 'cause', 'physical necessity', etc." (15, p. 297)

109. For example HAMPSHIRE: "The difference is that the subjunctive form *explicitly implies* that the condition specified is contrary to fact, the antecedent unfulfilled, while the indicative has no such explicit implication; it leaves it open." (32, p. 11)
And BRAITHWAITE: "The assertion of a subjunctive hypothetical is similar to that of an indicative hypothetical except that the assertion includes an assertion that p (i.e. the antecedent, H.K). is false (indicated by the use of the subjunctive mood or by some other device)". (9, p. 316)
NAGEL examined two examples and then stated: "In both conditionals, the antecedent and consequent clauses describe suppositions presumably known to be false." (51, p. 71)
GOODMAN explicitly confined himself to conditionals of which we know the antecedent to be contrary to fact. (28, p. 4)

110. 15, p. 302.

111. Cf. GOODMAN: "Considered as truth-functional compounds, all counterfactuals are of course true, since their antecedents are false." (28, p. 4)
And Nagel's example: "C' (i.e. the statement 'if the length of pendulum a had been shortened to one-fourth of its actual length, its period would have been half its actual period') is not rendered by the statement: 'The length of a was not shortened to a fourth of its actual length and if the length of a was shortened to one-fourth of its present length then its period was half its present period'. The proposed translation is unsatisfactory, because, since the antecedent clause of the indicative conditional is false, it follows by the rules of formal logic that if the length of a was shortened to a fourth of its present length, its period was *not* half its present period – a conclusion certainly not acceptable to anyone who asserts C'". (51, p. 71)

112. See 43.

113. 51, p. 68ff.

114. 9, p. 295ff.

115. 51, p. 71.

116. 28, p. 5. Cf.: "A counterfactual is true if a certain connection obtains between the antecedent and the consequent. But as is obvious from examples already given, the consequent seldom follows from the antecedent by logic alone." (ibid., p. 7–8)
And CHISHOLM: "A subjunctive conditional is one such that we can know that the antecedent in some sense implies the consequent without knowing the truth-values of either." (15, p. 295)

117. NAGEL has given a clear formulation: "a counterfactual can be interpreted as an implicit *metalinguistic* statement (i.e. a statement about *other* statements, and in particular about the logical relations of these other statements) asserting that the indicative form of its consequent clause follows logically from the indicative form of its antecedent clause, when the latter is conjoined with some law and the requisite initial conditions for the law." (51, p. 72)

118. "The first major problem is to define relevant conditions: to specify what sentences are meant to be taken in conjunction with an antecedent as a basis for inferring the consequent. But even after the particular relevant conditions are specified, the connection

obtaining will not ordinarily be a logical one. The principle that permits inference of 'That match lights' from 'That match is scratched. That match is dry enough. Enough oxygen is present. Etc.' is not a law of logic but what we call a natural or physical or causal law. The second major problem concerns the definition of such laws." (28, p. 8–9)

CHISHOLM also pointed to these two problems: "Assuming that the distinction between law statements and nonlaw statements is available to us, I shall now make some informal remarks which I hope will throw light upon the ordinary use of these conditionals." (16, p. 101) and somewhat later: "The peculiar problem of interpreting ordinary counterfactual statements is that of specifying which, among the statements the asserter believes, he intends to *exclude* from his presuppositions." (ibid., p. 103)

119. 51, p. 72.

120. See HAMPSHIRE 32 and RESCHER 65.
RESCHER: "First we have the nomological counterfactuals. These do not pose any distinctively logical difficulties, although they may (and I think do) generate real problems for the proper understanding of the concept of law. Secondly we have the purely hypothetical counterfactuals." (65, p. 195)

121. ANDERSON: "Several recent articles have stated or implied that the true subjunctive conditional sentences entail the denial of their antecedents. (. .) It will be the purpose of this note to establish conclusively that this view is untenable." (1, p. 35)
Cf. also D'ALESSIO, who wanted to give "further evidence against the widespread view that the grammatical distinction between subjunctive and indicative conditionals is associated with a logical distinction between two types of statement, viz., counterfactual and noncounterfactual." (17, p. 306)
CHISHOLM remarked already in his first article that "Many contrary-to-fact conditionals are not expressed in the subjunctive mood and many conditionals which are expressed in this mood are not actually contrary-to-fact." (15, p. 289)
At last AYERS: "What is it about a conditional statement which makes it 'counterfactual' or 'unfulfilled'? Certainly not any feature of the statement itself or its expression. Whether a conditional is fulfilled is like the question whether or not a factual statement is true: the answer depends on something quite outside the meaning, form or category of the statement or the sentence by which it is expressed. An empirical statement cannot by itself give us reason for saying that it is true or that it is false, since the assertion that it is either is one which must be checked against the facts. Nor can we read off from the mood or from any other feature of an hypothetical statement whether or not the antecedent or the consequent is fulfilled, whether or not it is counterfactual. For this again has to be checked against the facts." (3, p. 349)

122. See 72.

123. GOODMAN: "As a first approximation then, we might say that a law is a true sentence used for making predictions." (28, p. 20) and hence "A general statement is lawlike if and only if it is acceptable prior to the determination of all its instances." (ibid., p. 22) and somewhat later: "A sentence is lawlike if its acceptance does not depend upon the determination of any given instance." (ibid., p. 23)
NAGEL: "To call a statement a law is to assign a certain function to it, and thereby to say in effect that the evidence on which it is based is assumed not to constitute the total scope of its predication." (51, p. 63)
And STRAWSON: "All we need to add to our characterization of natural laws (i.e. that they are unrestricted generalizations, H.K) is the requirement that such evidence as this (i.e. evidence that there will be no more members, and that there never were more than the limited number of which observations have been recorded) shall not be an essential part of our grounds for accepting them." (73, p. 199–200)

124. STEGMÜLLER: "die Brauchbarkeit dieses Kriteriums (hängt) von der Wahrheit einer recht unplausiblen empirischen Hypothese ab, nämlich von der Annahme, das die Endlichkeitshypothese des Universums falsch ist. Unter dieser *Endlichkeitshypothese* verstehen wir dabei die Aussage, dasz das Universum eine endliche Zeitdauer besitzt, von endlicher

räumlicher Erstreckung ist und eine endliche Anzahl von Elementarpartikeln enthält. Ist die auf Grund heutiger physikalischer Erkenntnisse als fundiert zu betrachtende Endlichkeitsannahme richtig, so gibt es nach diesem Kriterium keine Gesetze. (. .) *die Tatsache allein, dasz dieses Kriterium von der Wahrheit einer physikalischen Hypothese abhängt, spricht gegen das Kriterium.*" (71, p. 305)
HEMPEL has also drawn attention to this point: "It would be excessive, however, to deny the status of fundamental lawlike sentence to all statements which, in effect, make an assertion about a finite class of objects only, for that would rule out also a sentence such as 'All robins' eggs are greenish-blue', since presumably the class of all robins' eggs – past, present and future – is finite." (36, p. 267)

125. Compare, e.g., STRAWSON who stated, after he has given his characterization of a law (cf. note 123): "Evidence which is both permissible and adequate to establish a law will then be adequate to establish also the related unfulfilled conditionals." (73, p. 200)
126. GOODMAN has given the following example: "That a given piece of copper conducts electricity increases the credibility of statements asserting that other pieces of copper conduct electricity, and thus confirms the hypothesis that all copper conducts electricity. But the fact that a given man now in this room is a third son does not increase the credibility of statements asserting that other men now in this room are third sons, and so does not confirm the hypothesis that all men now in this room are third sons." (28, p. 73)
127. 28, p. 73.
128. GOODMAN has made this clear enough with respect to the confirmability of a statement and LAUTER also pointed to the fact that an explication of the concept of law requires a solution of the problem of confirmation: "a theory of original nomological statements without an explanation of their inductive confirmation is unable to explain the difference between accidental universals and laws". (50, p. 139–40)
129. See 46 for the exposition of the raven-example. Cf. also 45, p. 75ff.
130. KNEALE: "The fact, if it is a fact, that no ravens have lived in very snowy regions may be only an accident of history, and so too the fact, if it is a fact, that there has never been a raven that was not black. But to say this is just to say that, even if (*per impossibile*) we could know the second fact, we should still not be entitled to assert such a contrary-to-fact conditional as 'If some inhabitants of snowy regions were ravens, they would be black'." (46, p. 123)
131. "Although we may not use any modal word such as 'must' or 'necessarily', we assume that our pronouncement (i.e. 'all F is G', H.K.) commits us not only to asserting that everything which actually has been or will be F has been or will be G, but also to asserting that if anything which is not as a matter of fact F were F it would also be G. This, I thought, was sufficient to show the inadequacy of a Humean account of natural laws." (47, p. 97)
And elsewhere: "an unfulfilled hypothetical proposition cannot be derived from a proposition which is concerned only with the actual." (45, p. 75)
132. 28, p. 82.
133. GOODMAN: "Somewhat like Kant, we are saying that inductive validity depends not only upon what is presented but also upon *how it is organized* (my italics, H.K.); but the organization we point to is effected by the use of language and is not attributed to anything inevitable or immutable in the nature of human cognition." (28, p. 96–7)
134. BRAITHWAITE: "If a reason can be given for the blackness of all ravens by exhibiting such a scientific system, this generalization will be regarded as lawlike." (9, p. 304)
NAGEL: "one may refuse to label S (i.e. 'all ravens are black', H.K.) as a law, on the ground that only statements for which indirect evidence is available (so that statements must occupy a certain logical position in the corpus of our knowledge) can claim title to this label." (51, p. 69)
135. 51, p. 70.
136. NAGEL: "The evidence on the strength of which a statement L is called a law can be distinguished as either 'direct' or 'indirect'. (a) It may be 'direct' evidence, in the familiar sense that it consists of instances falling into the scope of predication of L (. .) (b) The

evidence for L may be 'indirect' in two senses. It may happen that L is jointly derivable with other laws L_1, L_2, etc., from some more general law (or laws) M, so that the direct evidence for these other laws counts as (indirect) evidence for L. (. .) However, the evidence for L may be 'indirect' in the somewhat different sense that L can be combined with a variety of special assumptions to yield other laws each possessing a distinctive scope of predication, so that the direct evidence for these derivative laws counts as 'indirect' evidence for L." (51, p. 64–5)

PAP: "A universal statement is lawlike to the degree that it is indirectly confirmed by instances that directly confirm more general hypotheses from which it follows or less general statements that follow from it (. .). It is thus the concatenation of inductions by means of unifying, comprehensive generalizations that confers lawlike character on statements of restricted generality as well as on statements of unrestricted generality." (53, p. 302)

BRAITHWAITE: "The condition for an established hypothesis *h* being *lawlike* (i.e. being, if true, a natural law) will then be that the hypothesis either occurs in an established scientific deductive system as a higher-level hypothesis containing theoretical concepts or that it occurs in an established scientific deductive system as a deduction from higher-level hypotheses which are supported by empirical evidence which is not direct evidence for *h* itself." (9, p. 301–2)

137. GOODMAN has given a different example. For the statement 'all copper conducts electricity' indirect evidence is available that is direct evidence for 'all iron conducts electricity', via the higher-level statement 'all metals conduct electricity' (H). But the statement 'all iron things and all things on my desk conduct electricity' (K) does not put us in a position to accept the direct evidence for 'all iron conducts electricity' as indirect evidence for 'all things on my desk conduct electricity'. "Wherein lies the difference? (. .) Clearly the important difference here is that evidence for a statement affirming that one of the classes covered by H has the property in question increases the credibility of any statement affirming that another such class has this property; while nothing of the sort holds true with respect to K. But this is only to say that H is lawlike and K is not. We are faced anew with the very problem we are trying to solve: the problem of distinguishing between lawlike and accidental hypotheses." (28, p. 77)
138. Cf. LAUTER: "a complete analysis of the concept 'original nomological statement' must wait upon an analysis of what it is for a sentence to be part of an 'accepted scientific theory'. The prospects for the latter analysis are just as remote as those for a direct analysis of 'scientific law'." (50, p. 142)
139. 51, p. 69–70.
140. 51, p. 72.
141. "If this necessity is interpreted, as it has been, as a form of *logical* necessity, the meaning of 'necessary' in this sense is quite transparent; and indeed a systematic and generally accepted analysis of such necessity is provided by logical theory." (51, p. 52)
142. NAGEL: "none of the statements generally labeled as laws in the various positive sciences are in point of fact logically necessary, since their formal denials are demonstrably not self-contradictory. Accordingly, proponents of the view under discussion must either reject all these statements as not cases of 'genuine' laws (and so maintain that no laws have yet been discovered in any empirical science), or reject the proofs that these statements are not logically necessary (and so challenge the validity of established techniques of logical proof). Neither horn of the dilemma is inviting." (51, p. 53)
143. AYER: "if we want our generalizations to have empirical content, they cannot be logically secure; if we make them logically secure, we rob them of their empirical content. The relations which hold between things, or events, or properties, cannot be both factual and logical." (2, p. 219)

CARNAP: "It is true that we have obtained certainty (in the statements of logic and mathematics, H.K.), but we have paid for it a very high price. The price is that statements of logic and mathematics do not tell us anything about the world." (10, p. 10)

And: "Logical statements are true under all conceivable circumstances; thus their truth

is independent of the contingent facts of the world. On the other hand, it follows that these statements do not say anything about the world and thus have no factual content." (13, p. 25)

Cf. the rhetorical questions of HAMLYN: "How is it possible for a statement both to be about something and to elucidate the concepts involved?" (31, p. 108) and of WAISMANN: "if this item of knowledge (i.e. 'I see with my eyes', H.K.) is *based on experience*, how can it be *necessary*? And if it *was* necessary how could it tell us anything about experience?" (76, vol. 10, p. 117)

144. AYER: "It would be characteristic of such systems (of logically true statements, H.K.) that no experience could falsify them, but their security might be sterile. What would take the place of their being falsified would be the discovery that they had no empirical applications." (2, p. 217)
145. QUINE, 61, p. 22–3.
146. Cf. his 74.
147. Cf. Nagel's examples in 51. p. 54 and p. 55, footnote 8.
148. Ayer's example is this: "If it suits us to regard heavy water as a species of water, then we must not make it necessary that water consists of H_2O. Otherwise, we may. We are free to settle the matter whichever way we please." (2, p. 216)
149. 53, p. 252.
150. See 61 and also White's 77. Compare Goodman's remark: "the notion of a necessary connection of ideas, or of an absolutely analytic statement, is no longer sacrosanct. Some, like Quine and White, have fortrightly attacked the notion; others, like myself, have simply discarded it; and still others have begun to feel acutely uncomfortable about it." (28, p. 60, footnote 1)
151. "The unit of empirical significance is the whole of science." (61, p. 42) and "As an empiricist I continue to think of the conceptual scheme of science as a tool, ultimately, for predicting future experience in the light of past experience." (ibid., p. 44)

Statements in isolation cannot admit of confirmation or information: "The dogma of reductionism survives in the supposition that each statement, taken in isolation from its fellows, can admit of confirmation or information at all." (ibid., p. 41)

152. 61, p. 43.
153. See 30.
154. GRICE and STRAWSON: "Where such a shift in the sense of the words is a necessary condition of the change in truth-value, then the adherent of the distinction will say that the form of words in question changes from expressing an analytic statement to expressing a synthetic statement." (30, p. 157)
155. 51, p. 55.
156. BLANSHARD: "Granted that some things are related through a necessity linking their qualities, and that some events are related through the necessity implicit in causation, is there any ground for holding that all things and events are interrelated necessarily? Yes, there is impressive ground" (6, p. 472), because "It is part of reasonableness to accept a conclusion, even when undemonstrable, if it makes sense of things, and no alternative does." (ibid., p. 471)

And EWING, quoted by NAGEL: "The cause logically entails the effect in such a way that it would be in principle possible, with sufficient insight, to see what kind of effect must follow from examination of the cause alone without having learnt by previous experience what were the effects of similar causes." (51, p. 53)

And: "C (a cause, H.K.) may perfectly well entail E without our being able to see that it does so, and we may have general grounds for assuming the presence of a logical necessity which we cannot grasp ourselves, or at least see that this assumption is really presupposed in all our scientific reasoning." (21, p. 167)

157. We shall restrict ourselves to KNEALE, but see also W.E. Johnson's 'Logic' (I, ch. 11 and III, ch. 1) and G.H. von Wright's 'Logical Studies' (pp. 144–162).
158. For the exposition of this example, see 45, p. 75 ff.
159. "When we say that A-ness necessitates B-ness, we mean that it is impossible for an A-

thing not to be B. Our idea of necessitation is, therefore, the notion of the boundary to possibility." (45, p. 78)

160. 45, p. 80.
Cf. also: "Instead of trying to reduce necessity to universality we should, I think, take the notion of necessitation as fundamental." (47, p. 101–2)
It is interesting that HEMPEL also introduced a kind of necessity which can be interpreted as a boundary of possibilities: "whether a statement of universal form counts as a law will depend in part upon the scientific theories accepted at the time. This is not to say that 'empirical generalizations' – statements of universal form that are empirically well confirmed but have no basis in theory – never qualify as laws: Galileo's, Kepler's, and Boyle's laws, for example, were accepted as such before they received theoretical grounding. The relevance of theory is rather this: a statement of universal form, whether empirically confirmed or as yet untested, will qualify as a law if it is implied by an accepted theory; but even if it is empirically well confirmed and presumably true in fact, it will not qualify as a law if it rules out certain hypothetical occurrences (such as the fusion of two gold bodies with a resulting mass of more than 100,000 kilograms, in the case of our generalization H, i.e. 'All bodies of pure gold have a mass of less than 100,000 kilograms') which an accepted theory qualifies as possible." (39, p. 57–8)
But this again remains an incidental remark and is not systematically elaborated.

161. See for this discussion Popper's 55 (par. 14 and new appendix X), 56 and 59; and Kneale's 46 and 47.

162. For a discussion of essentialism, see 58.

163. POPPER: "Thus natural laws set certain limits to what is possible. (. .) in fact, when I said, in several places in my book, that natural laws *forbid* certain events to happen, or that they have the character of *prohibitions*, I gave expression to the same intuitive idea. And I think it is quite possible and perhaps even useful to speak of 'natural necessity' or of 'physical necessity', in order to describe this character of natural laws, and of their logical consequences." (55, p. 428)

164. 55, p. 433.

165. 55, p. 427–8.

166. KNEALE: "The important thing in Popper's new definition, and what makes it acceptable to me, is just that it connects the notion of natural law with that of validity for states of affairs other than the actual." (47, p. 99)

167. KNEALE: "all worlds that differ from our world, if at all, only in initial conditions and these must clearly be naturally possible worlds with instances of the same attributes and relations as we find exemplified in our actual world." (47, p. 101)
NERLICH and SUCHTING describe these worlds as 'all worlds which instantiate the properties and relations of our world but which may differ from our world with respect to initial conditions." (52, p. 234)

168. "What we mean is 'all worlds which have the same structure – or the same natural laws – as our own world'." (55, p. 435)

169. See 52.

170. 59, p. 321.

171. POPPER says this in an addendum of 1968 to the Xth new appendix of 55, and he continues with: "I may perhaps sum up my position by saying that, while theories and the problems connected with their truth are all-important, words and the problems connected with their meaning are unimportant. For this reason I am not really very interested in either the definition or in the definability of 'natural necessity'; though I am interested in the fact (for I believe that it is a fact) that the idea is not meaningless." (55, p. 441)
I do not think that this remark is a very good one either. Of course, a quarrel about words is not very interesting, but it has much of a purely ad hoc escape to qualify the attempts for clarifying a concept (i.e. 'physical necessity') as a quarrel about words.

172. POPPER: "Nevertheless, the phrase (in the definition) 'all worlds which differ (if at all) from our world only with respect to initial conditions' undoubtedly contains implicitly the idea of laws of nature. (. .) (so it) may be said to be circular." (55, p. 435)

173. 55, p. 435.
174. See 66.
175. "Lawfulness manifests itself in two related ways: *nomic necessity* and *hypothetical force*. Nomic necessity introduces the element of *must*, of inevitability. (. .) This nomic necessity manifests itself most strikingly in the context of hypothetical suppositions – especially counterfactual hypotheses. (. .) It is preeminently this element of hypothetical force that distinguishes a genuinely lawful generalization from an accidental generalization like 'All coins in my pocket weigh less than one half ounce'." (66, p. 179)
176. 66, p. 185.
177. RESCHER: "For no matter what the structure of a generalization might be, or how well established it is by the known data, its acceptance as a law demands some accommodation of it within the 'system' of knowledge. Any 'law' occupies a place that is more or less fundamental within the general architectonic of our knowledge about the world – its epistemic status is a matter not only of *its own* form and *its own* evidential support, but *its placement within the woof and warp of the fabric comprising it together with other cognate laws of nature*." (66, p. 186)
178. 66, p. 187.
179. RESCHER: "Our view of the matter agrees with Hume's that lawfulness is not an observable characteristic of nature, and it agrees with Kant that it is a matter of man's projection. But we do not regard this projection as the result of the (in suitable circumstances) inevitable working of the psychological faculty-structure of the human mind. Rather, we regard it as a matter of *warranted decision*, a deliberate man-made imputation effected in the setting of a particular conceptual scheme regarding the nature of explanatory understanding. We thus arrive at a position that is Kantian with a difference. Kant finds the source of lawfulness in the way in which the mind inherently works. We find its source in the conceptual schemata that we in fact deploy for explanatory purposes: As we see it, lawfulness demands an imputational step made in the context of a certain concept of explanation. Both these divergent views agree, however, in making lawfulness fundamentally mind-dependent." (66, p. 189–90)
180. "At this point, however, the distinction between laws and regularities becomes important. No doubt nature is in various respects regular – it would take a bold act of rashness to deny that! And this regularity of nature in various respects is no doubt an ontological fact that would remain unaltered in the face of any hypothetical removal of rational minds from within its purview. But the idea of a law involves – as we saw – more than just factual regularity as such, since lawfulness is bound up with nomic necessity and hypothetical force. To say that these factors do not represent objective facts but result from man-made imputations is not to gainsay the objective reality of regularities in nature." (66, p. 192)
And: "Does it follow from this position that if there were no men – or rather no rational minds – that there would be no laws? (. .) The answer (. .), I believe, must be: Yes." (ibid., p. 190)
181. "Like rules, laws state how things 'must be', yet unlike most familiar rules laws admit no exceptions, but are always 'obeyed'." (66, p. 178)
182. We find the same failure in Geurts' treatment of lawlike statements: first of all, we have, in his view, a synthetic universal statement, asserting a contingent regularity, be it, in his view, not a regularity in an observer-independent reality. Then the scientist makes it analytic by decision, and hence lawlike. But the presupposition of there being synthetic universal statements, asserting an observed regularity, as the basis of a law, is untenable. See 26, pp. 183–190.
183. 57, p. 44–5.
184. RESCHER: "The appropriateness of such epistemic commitment revolves about questions of the type: 'To what extent is the 'law' at issue justifiably regarded as immune to rejection in the face of hypothetical considerations?' 'How should this generalization fare if (*per improbabile*) a choice were forced upon us between it and other laws we also accept?' 'How critical is it that the law be true – how serious a matter would it be were

the law to prove false?'" (66, p. 186)

185. "None of these claims (i.e. 'No object can move faster than light', 'There cannot be a perpetuum mobile', 'A temperature registration of less than –273 °C. is impossible') state *mere* matters of fact. Each essentially involves the conceptual principles of entire physical theories. Similarly the proposition: 'One must interact with microparticles to learn about them'. The negation of this is none the less physically unintelligible, as with the claims above." (33, p. 78)
And: "It is at this moment inconceivable – i.e. 'systematically unintelligible' – that we should ever encounter a *perpetuum mobile*, or accelerate particles faster than *c*." (ibid., p. 83)

186. Compare once again GRICE and STRAWSON: "If we can make sense of the idea that the same form of words, taken in one way (or bearing one sense), may express something true, and taken in another way (or bearing another sense), may express something false, then we can make sense of the idea of conceptual revision. And if we can make sense of this idea, then we can perfectly well preserve the distinction between the analytic and the synthetic, while conceding to Quine the revisability-in-principle of everything we say." (30, p. 157)

187. QUINE is right when he rejects the view that an analytic statement is one in which 'the linguistic component is all that matters'. He says: "it is nonsense, and the root of much nonsense, to speak of a linguistic component and a factual component in the truth of any individual statement." (61, p. 42)

188. This opposition has also been accepted by FISK, who speaks of a necessary connection in nature to account for the fact that in nature only a few of a great many possibilities are realized: "It is often supposed that the stable patterns required are simply regularities. One of the main points to be made here will be that mere regularities are inferior to necessary connections in doing the job that must be done for data to serve as inductive support. The problem is not that data fail to have a chance of being representative of the way things are if there are only regularities in the universe, and no necessary connections. It is rather that, without necessary connections, things could happen in so many ways that it would be unreasonable to suppose they happen in one of the relatively few ways in which regularties are preserved. A denial of necessity makes unreasonable an assumption of regularities. (. .) only if there are a certain number of unspecified necessary connections believed to hold in nature is it consistent to hold that given data support a given hypothesis." (25, p. 385)
Such a necessity should be taken to be "a real necessity – a *de re* necessity – and is not a propositional necessity, a *de dicto* necessity." (ibid., p. 389)

189. Cf. HANSON 33, p. 26ff. HANSON has clearly seen the importance of necessary connections for scientific investigation.

190. These cases also constitute examples of Harré's so-called ontological experiment, 35, p. 40.

191. 33, p. 32.

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CURRICULUM VITAE

Op 15 oktober 1939 ben ik te Helmond geboren. In 1958 slaagde ik voor het eindexamen H.B.S.-B aan het Marnix College te Ede.

Na het vervullen van mijn dienstplicht begon ik in 1960 mijn studie in de scheikunde aan de Rijksuniversiteit te Utrecht.

Na het kandidaatsexsamen koos ik als hoofdvak de wijsbegeerte van de natuurwetenschappen en als bijvakken theoretische organische chemie en systematische filosofie.

In 1967 voltooide ik mijn studie, waarna ik tot 1 september 1971 als medewerker verbonden was aan de Centrale Interfaculteit van de Rijksuniversiteit te Utrecht. Sinds september 1971 ben ik medewerker bij de afdeling wijsbegeerte van de Landbouwhogeschool te Wageningen.