THE LOGICAL PROBLEM OF INDUCTION

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Second, enlarged Edition

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by

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To

EINO KAILA

PREFACE TO THE REVISED EDITION

THIS book was first published in 1941 as a thesis for the doctor's degree in the University of Helsingfors. It appeared as volume III in the series Acta Philosophica Fennica. The book having been out of print for some time, I was invited by its present publishers, Basil Blackwell & Mott, Ltd., to prepare a new edition of it.

The book is essentially a discussion of the traditional problem of the Justification of Induction, sometimes also called the Problem of Hume. It examines some main attempts at a solution of this problem: the doctrine of synthetical judgments *a priori*, conventionalism, inductive logic in the tradition of Bacon and Mill, the approach to induction in terms of probability, and the pragmatist approach. All these attempts contribute something important to our understanding of the nature of induction. But in a certain respect they all fail to accomplish what they, often at least, claim to achieve. In a concluding chapter I try to show why this 'failure' on the part of any attempted justification of induction is inevitable, and why the view that it has catastrophic implications rests on a misunderstanding.

Since this book was first published, there has been a noticeable revival of interest in Inductive Logic. The new interest, however, has chiefly been in the *formal* aspects of the relation between premisses and conclusion in inductive arguments. The two main branches of this formal study may be called Elimination-Theory and Confirmation-Theory. The former is a further development of the tradition in inductive logic founded by Bacon. The latter is essentially a theory of inductive probability. In another work, called *A Treatise on Induction and Probability* (Routledge & Kegan Paul, London 1951) I have tried to contribute something to both these branches of formal study.

If I were now to write afresh on the Problem of Hume, I should probably write a very different book from this one. Not that the present work expresses opinions which conflict with my present views. But on many topics, here but lightly touched, I feel the need for a more profound discussion. This feeling attaches particularly to the treatment of synthetical judgments *a priori* in Chap. II, which I now think is superficial.

In order, however, not to attempt what I believe to be an unhappy compromise between the original set-up of the discussion and a new method of dealing with the problems, I have tried to interfere with the original text as little as possible — beyond correcting downright errors and adding references to more recent literature. Some sections I have preferred to re-write entirely rather than to revise. These sections are the following:

§3 of Chap. I, *Remarks about various usages of the term 'induction'*, has been re-written and considerably expanded by the inclusion, among other things, of some comments on Aristotle's doctrine of induction and on the mutual relation between generalization and inference from particulars to particulars (induction *versus* eduction).

§4 of Chap. IV, *The mechanism of elimination*, has been re-written so as to conform to the fuller account of the logic of induction by elimination which is given in my *Treatise on Induction and Probability* (Chaps. III & IV).

To Section 1 of Chap. V, I have appended a longish *Note on the role of Induction and Hypothesis in Science*. The urge to write it came from some recent attempts, which I consider unjustified, to decry the importance of inductive inference and of the epistemological and logical problems which it raises.

The whole of Chap. VI, *Formal Analysis of Inductive Probability*, is new. The old version of it was extremely weak and guilty of many errors. This chapter stands somewhat aside from the main theme of the book. I hope that, in its present shape, it could serve as an introduction to a more thorough study of an important province of contemporary formal Inductive Logic.

From Chap. VII of the old edition the first Section has been omitted. Its content is incorporated in Section 2 of the present Chap. VI.

§2 of Chap. VIII, *Reichenbach's Method of Correction*, has been re-written. The old version was neither accurate as an account nor fair as an appreciation of Reichenbach's opinions. To the same chapter has been added in the revised edition Section 3, *The goodness of inductive policies reconsidered*. Here the problem of the justification of induction is viewed from an angle somewhat different from that adopted in the rest of the book. Had I written a completely new book on induction, I should probably have made the point of view adopted in this section more pervading of the discussion throughout.

The Notes contain mainly historical material. I have no claim to scholarship in the history of learning or thought. But I have perhaps occasionally been able to give hints which may be of interest even to the professional historian of ideas.

I have tried to bring up to date the Bibliography of the 1941 edition. I have also added to it some items of an earlier date, and omitted from it others as irrelevant. The Bibliography primarily lists works and articles on the *epistemological* aspects of induction and probability. It cannot pretend to be complete, but I hope students will find it useful.

Dr. C. D. Broad, from whose kindness and knowledge I have much profited throughout my work in the field of induction and probability, has very kindly read the revised sections and contributed both linguistic corrections, and valuable comments on the subject-matter. I am most grateful for his assistance.

I dedicate this revised edition of my book to my first master in philosophy, as a token of gratitude for what he has taught me and of admiration for what he has done to encourage serious study in logic and philosophy in my country.

GEORG HENRIK VON WRIGHT

Helsingfors, Finland
May 1955

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CHAPTER I

INTRODUCTORY REMARKS ON INDUCTION

§1. *Inductive inference and the problem of induction.*

BY an inductive inference we mean roughly this: From the fact that something is true of a certain number of members of a class, we conclude that the same thing will be true of unknown members of that class also. If this conclusion applies to an unlimited number of unexamined members of the class, we say that the induction has led to the establishment of a generalization. As this is the most important type of inductive inference, induction is often defined as the process by which we proceed from particular to general, or from less general to more general propositions.[1]

It is, however, not necessary that an inductive inference should lead to a generalization. We may also extend the conclusion to a limited number of unknown members of the class, e.g. to the next member which turns up, thus proceeding from particulars to a new particular.[2] Both cases of inductive inference, that from particulars to universals and that from particulars to particulars, are covered by the definition of induction as reasoning from the known to the unknown.[3] This definition also includes induction as reasoning from the past to the future. It must be observed that this time-characteristic of inductive inference, which is sometimes mentioned in the definition of it, is of no essential importance, and that induction may also proceed from past cases to other unexamined instances belonging to the past.

Inductive inference, as is well known, plays an important role both in science and in every-day life. When the general aim of science is characterized by the words 'savoir pour prévoir', then science is conceived as a system of well-established inductions. Still more fundamental is the importance of induction as the basis of almost all our actions. When I assume that the same food that nourished me yesterday will nourish me today, or that if I put my hand into the fire it will hurt, I am making inductive inference. Also in many cases, where there is no question of conscious inference, we may be said to 'act inductively'.

But what are the criteria that an inductive inference is legitimate, i.e. how do we know that from what has been true of hitherto examined members of a class we can infer something as to other members also of the class? Is it possible, provided certain conditions are fulfilled, to prove that an inductive inference must necessarily be valid, and what are these conditions? Or, if this be not the case, is it then perhaps possible to determine which inductions are likely and which again are unlikely to be true, so that we shall be able to avoid 'bad' inductions and by keeping to the 'good' ones arrive at the truth at least in a majority of cases?

In all these questions the *validity* of inductive arguments is asked for, i.e. the logical nature of the relation that prevails between the evidence on which the induction is based as premisses, and the induced proposition itself as conclusion. The problem constituted by these questions we shall, therefore, call *the logical problem of induction*. With it can be contrasted the question of the factual *origin* of inductions from observations, i.e. of the psychological conditions that are essential for the discovery of invariances and laws in the flux of phenomena, and of the practical rules of scientific methodology, which can be abstracted from those conditions. This problem, which falls outside the scope of the present treatise, might be called *the psychological problem of induction*. It deserves mention that whereas philosophy of induction in England has been predominantly concerned with the former aspect of the problem, French authors on questions of induction have mostly taken interest in the latter.[5]

Historically the logical problem of induction has, ever since the appearance of David Hume's *Treatise on Human Nature* two hundred years ago, been closely tied up with the question about the *justification* of our use of inductive arguments. The use of induction, we are inclined to think, would not be rational, unless we can justify it by substituting for the mere belief that induction will lead to the truth some guarantees that, under given conditions and with specified limitations, it will *actually* do so. If such a justification cannot be given, the foundation of human knowledge about empirical reality seems to sink into a bog of irrationality, fatal for the superb edifice of natural laws and well-confirmed empirical rules based upon it. It is the chief aim of this treatise to clarify the muddles in the philosophy of induction, which originate from the 'sceptical' results of Hume. The logical problem of induction, therefore, as treated by us, will predominantly be viewed in its relation to the classical question about the justification of induction.

It might be suggested that there is a fundamental difference between the problem of justifying induction in cases where the inference involves a generalization, and the same problem in cases where the inference is extended only to a limited number of cases. This apparent difference is connected with the fact that inductions covering an unlimited range of instances are *unverifiable* in the sense that we can experience only a finite number of cases covered by them, whereas inductions extended solely to a limited number of new cases are at least 'in principle' verifiable, i.e. *all* the cases may fall within our experience and be verified separately. It is, however, important to note that the problem of induction as we are going to treat it has nothing to do with the possibility of finding a subsequent verification of inductive inferences, i.e. of verifying them by verifying *all* their single instances. The justification we are looking for is a justification of inductive inferences *before* their actual experiential verification, and this problem is essentially the same when the induction under consideration applies to the next member only of the class in question and when it applies to an infinite multitude of possible future members.[6]

In the following pages we shall mainly speak of inductions as generalizations, i.e. as extended to an unlimited number of instances, except where otherwise mentioned. Our first task will be to examine some different logical forms which inductive generalizations may assume.

§2. *Different forms of inductive generalizations.*

Our definition of an inductive generalization covers several different cases. In the simplest case we infer from the fact that all observed members of a class A have a property B that *all* unknown members of the class will have the same property. The generalization is thus of the form:[1]

$$(1) \qquad (x)\ [Ax \rightarrow Bx],$$

that is to say, it is true for every x, that if x is A, it is also B. If it is true as well that every B is A, the general implication takes the form of a general equivalence between Ax and Bx.

One of the predicates A and B, or both of them, may also have more than one argument. This is for example the case with the generalization: any two elements of the class A have the relation B to each other, which is of the form:

$$(2) \qquad (x)\ (y)\ [Ax\ \&\ Ay \rightarrow B\ (x, y)].$$

Evidently a great majority of natural laws in advanced sciences such as physics and astronomy concern the relations in which objects stand to each other and not the 'properties' of single objects. Inductions of the form (1) are therefore to be regarded as fairly 'primitive'.

The generalization may further be one about *ordered* sets of individuals. The symbolic expression of such an induction would for example be the following:

$$(3) \qquad (x)\ (y)\ [F\ (x, y) \rightarrow (Ax \rightarrow By)],$$

where F is the relation determining which of any x's and y's together form an ordered pair in this case. The important category of inductive generalizations, which are commonly known under the name of *causal laws*, are generalizations of this type.

A characteristic feature of the logical structure of generalizations of the kind (1) – (3) is that they are general implications or equivalences involving *only* the universal operator. Inductions of this type we call Universal Inductions or Universal Generalizations.[2] We shall in the next three chapters be dealing mainly with inductions of this kind, and when it is useful to state them in symbols we shall write them in the simplest form (1), except where otherwise is required.

With the Universal Inductions can be contrasted those generalizations in which we infer that something will be true not of *all* the members of a class, but of a certain proportion of them only. Inductions of this kind we shall call Statistical Inductions or Statistical Generalizations.[3] Such generalizations have recently come to play an increasingly important role in scientific inquiry, both in social sciences and mathematical physics. The question whether Statistical Generalizations are to be regarded merely as 'approximations' to the truth, being in principle replaceable by a system of Universal Inductions, or whether they represent 'ultimate' laws of nature, is one of the chief controversies in the philosophy of modern natural science.[4]

Statistical Generalizations speak about finite proportions of infinite multitudes of elements. The concept of a proportion which has a clear-cut meaning when applied to a finite number of enumerable elements or to a series of an infinite number of elements, given according to a rule determining the characteristics of each one of the members in the series, becomes problematic when we apply it, as is the case in Statistical Generalizations, to an unlimited multitude of what we may call 'empirically given' elements.[5] In order to show that the conception of a proportion has a definite meaning even in this last-mentioned case we shall examine the following symbolic expression which is equivalent to the statement that a proportion p of all elements which are A, are also B:

$$(4) \quad (E!q)\,(\varepsilon)\,(x_m)\,(Ex_n)\left[n > m \;\&\; \frac{\underset{i=1}{\overset{i=n}{N}}\; Ax_i \& Bx_i}{n} = p \pm \varepsilon\right].$$ [6]

In order to understand this formula, suppose first of all that we give each instance of A as it occurs an ordinal number: the first, the second, . . ., the n'th and so on. Then the expression (4) says that a proportion p of all the A's are B, if there exists one and only one real number p — between 0 and 1, including these two limits — such that for any element x_m there exists in the series a later element x_n, at which the ratio, of all elements which are both A and B to the total number n of examined elements of the class A, falls in the interval $p \pm \varepsilon$, where ε can be any amount however small. We say, in other words, that a proportion p of the elements which are A are also B, if over and over again it happens that, when the selection of A's is enlarged, the proportion of B's among them sooner or later becomes 'practically equal' to p. One further condition must be added, viz. that p is the only number for which this is true. We could imagine cases where more than one number had this property.[7] In such cases we should say that there is no definite proportion of A's which are B.[8]

The most interesting feature of this definition of a proportion is, for the purpose of our inquiry, that it is applicable also to series of elements, the characteristics of which are given empirically or extensionally, and not according to mathematical rule. For the formula shows that for this applicability it is only necessary that the elements of the class A must be *enumerable*, i.e. it must be possible to count them *as they occur* and to give to each one of them an ordinal number, thus ordering them in a series. This condition is fulfilled for all series or collections ('populations') of empirically given elements to which inductive generalizations apply. That this is the case follows from what we mean by 'empirically given'.

A proportion as defined by us is nothing but the limiting value of a certain relative frequency in an ever-increasing collection of elements.[9] It is very important to note that we can apply the concept of a limit (or proportion) to extensionally given series of elements without saying anything more about the way in which the characteristics are distributed in the series than is contained in the definition itself of the concept. The question of further properties of this distribution (randomness, irregularity, 'Nachwirkungsfreiheit' and so on) occurs first when we have to determine the connection between Statistical Generalizations and Probability-Laws, between propositions like 'a proportion p of all A's are B' and 'it is probable to degree p that an x, taken at random, which is A will also be B'.

We have already mentioned that inductive generalizations are unverifiable propositions in the sense that we can never verify more than a finite number of their instances. This unverifiability applied equally to Universal and to Statistical Generalizations. But in respect of falsifiability there is not a corresponding symmetry between the two types of generalization. A universal implication like (1) is falsified if a single proposition $Aa \,\&\, {\sim}Ba$ turns out to be true; i.e. if there is at least one individual which, although it is A, is not B, then it is obviously false to maintain that all A's are B. But if we had asserted, say, that 10 per cent of the A's were B, there is not a corresponding possibility of falsifying the statement. For even if it happened that the actual proportion of observed A's which are B deviates to any extent from 10 per cent, it is always conceivable that, by extending our observations to new A's, we finally arrive at a point where the observed proportion will equal 10 per cent. And as this might happen over and over again, after every deviation, the true proportion of A's which are B may after all be just this percentage.

Statistical Generalizations or assertions about proportions, therefore, are neither verifiable nor falsifiable propositions. This fact which simply follows from what we *mean* by saying that such and such a *proportion* of the elements in a class have a certain property, must not be considered alarming from an epistemological point of view.[10]

It must further be noted that from the logical structure of the formulae (1) — (4) it follows that a Universal Generalization is not a special or extreme case of a Statistical Generalization, that is to say a Statistical Generalization where the proportion in question is 1. For a proportion 1 or 100 per cent of the A's might be B, and there might still exist an infinite multitude of A's which are *not* B. Conversely, that 0 per cent of the A's are B is in no way incompatible with the existence of an infinite number of A's which are B.[11]

We have still to mention those inductive generalizations, which resemble Statistical Inductions in that their symbolic expressions contain both universal and existential operators, for which reason they are neither verifiable nor falsifiable propositions, but which nevertheless do not assert anything about proportions. For example: there exists a maximum velocity in nature — i.e. a velocity which is greater than all other velocities — or there exists a minimum quantity of energy. Of this type also is the following proposition: To every species of flower with honey there exists, for the purpose of fertilization, an insect which is able to reach the honey. This proposition may be said to have guided Darwin in his prediction of the existence of the insect which fertilizes *Angraeum sesquipetale*.[12]

The symbolic expression of this proposition would have the form:

$$(5) \qquad (x)\ \{Ax \rightarrow (Ey)\ [By \& R\ (x, y)]\}.$$

§3. *Remarks about various usages of the term 'induction'. Induction and eduction.*

The logician's term 'induction' is a translation of the Greek ἐπαγωγή which occurs in the logical works of Aristotle. It is noteworthy that Aristotle uses the term in three different ways.[1]

In the *Topics*, which is probably among the earliest of his logical writings, Aristotle defines[2] induction as 'a passage from individuals to universals'. As an example he gives[3] 'the argument that supposing the skilled pilot is the most effective, and likewise the skilled charioteer, then in general the skilled man is the best at his particular task'. Of this kind of induction he says[4] that it proceeds 'from the known to the unknown'.

In the *Prior Analytics* treatment of induction is linked with the theory of the syllogism. The account is not very clear.[5] Aristotle gives[6] an example which can be rendered[7] as follows: Man, the horse, and the mule are long-lived. But man, the horse, and the mule are all the bileless animals. Therefore all the bileless animals are long-lived. It is here essential that 'induction proceeds through an enumeration of all the cases'.[8]

In the *Posterior Analytics*, finally, induction is said[9] to impart new knowledge by 'exhibiting the universal as implicit in the clearly known particular'. In induction we abstract, through an act of intuition,[10] a general truth from considerations of a particular instance of it. It is essential to the Aristotelian doctrine that knowledge of particulars is possible only through sense-perception.[11]

Text-books on inductive logic usually mention only the two first kinds of inductive inference distinguished above. Induction which proceeds 'through an enumeration of all the cases' is usually called *complete*. It is more appropriately called *summary*[12] or *summative*[13] induction. Induction in the sense which Aristotle seems to contemplate in the *Topics* is traditionally called *incomplete*. It is also called *problematic.*[14] The best name for it seems to us to be *ampliative* induction.[15]

Induction in the sense of the *Posterior Analytics* has been called *abstractive* or *intuitive* induction.[16] It is a faculty of the intellect which is highly significant both for epistemology and metaphysics.

By *recursive* (or mathematical) induction one understands an argument of the following type: The first member of a series has a property A. It is shown that, *if* the n'th member has this property, then the $n+1$'th member has it too. From these two facts we conclude that *all* members of the series have the property A. This type of argument is of great importance in mathematics.[17] It seems first to have been consciously employed by James Bernoulli[18] and is sometimes also called Bernoullian induction.

Summative and recursive induction are both logically *conclusive* types of argument. It is of the essence of ampliative induction that it is *inconclusive*; that the argument is 'ampliative' means that its conclusion goes beyond ('transcends') its premisses, i.e. does not follow logically from them.

Ampliative induction, though in itself inconclusive, may nevertheless be turned into a conclusive argument, when supplemented by certain additional premisses.[19] Inductive inference, when exhibited in 'syllogistic' form, has been termed *demonstrative* induction.[20] The supplementary premisses are sometimes referred to under the name of Presuppositions of Induction.

An argument by summative induction can be given the following schematic form:

$$\begin{array}{l} A_1 \text{ and } \ldots \text{ and } A_n \text{ are all of them } B \\ A_1 \text{ and } \ldots \text{ and } A_n \text{ are all the } A\text{'s} \\ \hline \therefore \text{ All } A\text{'s are } B \end{array}$$

A and B are properties (classes). $A_1 \ldots A_n$ can be interpreted alternatively as classes or as individuals. The example from Aristotle, quoted above, answers to the first interpretation.

Summative induction is not, as has sometimes been said, a useless or trivial kind of argument. It often renders good service to 'the economy of thought' by summing up in a general formula the information contained in its premisses. Another relevant use of it occurs in mathematical proofs. In order to establish a proposition, we sometimes first 'resolve' it into a finite number of 'cases' to be considered separately. Thereupon we carry out the proof of the proposition for each case or show that the proof of some cases can be traced back to other cases which we have already settled. Finally we conclude by summative induction that the proposition has been established.[21]

The 'problematic' element in summative induction usually is in the second premiss, which says that the enumerated cases exhaust the scope of the generalization. Summative induction, however, presents no problem of justification *similar* to that of ampliative induction which we have described in section 1. But there is a superficial analogy between the two modes of induction which may be very misleading with regard to the problem of how ampliative induction is to be justified. In both cases we draw a general conclusion after the enumeration of a certain number of single instances. This suggests the same name, viz. that of *induction* and *inductive inference*, for the two processes. Once in possession of this common name we arrive at the further idea that, as reasoning from *every one* of the cases separately to *all* the cases leads to a *certain* conclusion, so reasoning from *some* only of the cases to *all* of them leads to something 'less' than certainty but still *resembling* it. This 'something' which resembles certainty without possessing its full power is then called *probability*.

The idea of induction as a sort of inference and of the relation between this kind of inference and probability thus has one of its roots in the apparent analogy between so-called complete and incomplete, summative and ampliative, induction.

In this book we are concerned exclusively with induction which is ampliative. Some authors, among them Mill,[22] even wish to restrict the term 'induction' to ampliative reasoning.

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As was already observed above (p. 1), ampliative reasoning deserving the name of induction need not aim at the establishing of general propositions. It may also conclude from some particular cases to some other particular cases. This type of reasoning has been called *eduction*.[23]

It is convenient to define the distinction between (generalizing) induction and eduction in such a way that inference from some instances of a class to *any finite number* of new instances of the class counts as eduction – even if this number of new instances should happen, with the old ones, to exhaust the class in question. It follows from this convention that (genuine) generalization always pertains to a *numerically unrestricted* ('potentially infinite') class of cases.

Mill, though recognizing inference from particulars to particulars,[24] was of the opinion that 'whenever, from a set of particular cases, we can legitimately draw any inference, we may legitimately make our inference a general one'.[25] This, we believe, expresses an important insight. The following considerations will perhaps serve to make this insight more explicit:

Let us assume that all *A*'s so far observed have been *B*. On the basis of this we are willing to assert that the next (or the next few) *A* which turns up will also be *B*. But we are *not* willing to assert the general proposition that all *A*'s are *B*. What could be the *reason* for this hesitation to generalize?

It seems off-hand plausible to think that hesitation to generalize must be due to fear that the *circumstances* under which the *A*'s so far have been observed might have been peculiar to the observed *A*'s and that a variation in accompanying circumstances will affect the occurrence of the property *B* in other *A*'s. In other words, we entertain a suspicion that some feature (or features) *C*, *other* than *A*, is to be held 'responsible' for the fact that all *A*'s so far observed have been *B*. If *C* is present in an *A*, then this *A* will be *B*. But if *C* is absent from an *A*, *B may* be absent too. Thus the eduction that the next *A* will be *B* gets its 'legitimacy' from our belief in a general proposition to the effect that all *A*'s which are *C* are also *B and* our belief that this *C*, whatever it may be, will accompany the next *A*.

The truth contained in Mill's dictum thus appears to be, that being 'backed' by a general truth is part of what we *mean* by a *legitimate* eduction. And this would serve to indicate that use of eduction is 'logically secondary' to generalization.[26]

In recent times Carnap[27] has emphasized the importance of ampliative inference other than universal inference, the importance of which he thinks has been overrated in traditional theory. Carnap's attitude is probably influenced by the fact that in his system of inductive logic general propositions (laws) always have a zero probability relative to their confirming evidence.[28] This fact, however, would seem to us to indicate a peculiar limitation in Carnap's theory rather than a limitation in the relevance of generalizing induction.[29] Classical theory of induction may have unduly neglected the study of eductive inferences, but it is even more obvious that Carnap underrates the practical importance and logical interest of generalization.

CHAPTER II

INDUCTION AND SYNTHETICAL JUDGMENTS *A PRIORI*

§1. *Justification* a priori *of induction.*

IN the next three chapters we shall be concerned with the possibility of justifying inductions with certainty, i.e. of proving the truth of inductive inferences either prior to the verification of any of their instances or after the verification of some of them. In the former case we speak of justification of induction *a priori*, in the latter of justification *a posteriori*. It must be noted that justification *a priori* does not exclude the possibility that instances of the induction have been recorded previously. The observation of such instances may even be a psychologically necessary condition for the detection of the justification. That the justification is *a priori* means, for the purpose of our investigation, only that factual instances play no role in the *proof* of the truth of the induction.

There is a *prima facie* presumption in favour of the possibility of justifying inductive inference by means of *a priori* arguments in certain typical cases. An instance is afforded by so-called causal laws. If we observe only that *A* is regularly followed by *B*, we cannot infer with certainty that the same will always be the case. But it sometimes happens that we can 'explain' the sequence from *A* to *B*, e.g. by discovering that *A* is causally connected with *B*. We began with the observation of a certain regularity, and found upon closer investigation a *reason* for it. This reason, we say, is a justification of the induction that the observed regularity will hold also for the future.

We shall first discuss this argument from causal connections. The results at which we arrive we shall automatically be able to extend to the problem as a whole of justifying induction *a priori*.

§2. *Hume's theory of causation.*

There are three fundamental types of inductive generalizations based upon alleged causal connections. That *A* is the cause of *B* may first of all imply that whenever *A* occurs it will be followed by *B*. In this case we say that *A* is a sufficient condition of *B*. But the causal relation may also entail that when *B* has happened it must always have been preceded by *A*. Here *A* is called a necessary condition of *B*. In the third place *A* may be both a sufficient and necessary condition of *B*. In the two first cases the inductive generalization, expressed in symbols, is a general implication; in the third case again it is a general equivalence.[1]

In order to see whether a causal connection between *A* and *B* justifies an inductive generalization of any of the above-mentioned types, we have to examine the logical structure of the relation between cause and effect. For the purpose of this investigation we can follow the lines of Hume. It must be noted that Hume, in speaking about cause and effect, chiefly had in mind relations where one term is a sufficient condition of the other.[2] This, however, does not in any way restrict the field of applicability of his theory of causation.

Hume's argument against the view that causal connection between *A* and *B* would involve some power or force making *B* a necessary consequent of *A*, or in other words that causal connection would justify induction, is best illustrated with his own well-known example of the billiard-balls.[3]

We commonly say that the impact of one moving ball against another stationary one is the cause of the second ball's movement. What is the content of this assertion? If I first consider a single case where this happens, all I actually experience, which is relevant to my assertion about cause and effect, is the movement of the first ball, its impact against the second one and finally the second ball's movement. In this we find no additional experience of a causal power, except perhaps in the purely psychological sense, which taken together with the experience of the movement of the first ball and the impact would assure us of the second ball's movement as an event which must necessarily and inevitably follow.[4]

Again, if I consider, not a single instance of the causal law, but a number of such instances, the situation remains fundamentally the same. In each case I experience the same succession of events, but even if the succession repeats itself a great number of times, this additional experience does not give us any further information as to what will happen next.[5] If the assertion that *A* is the cause of *B* is to imply that *A* will always be followed by *B*, the causal proposition is itself an instance of induction and in need of justification.

In examining the validity of the argument of Hume we have first to note two peculiarities of it which are open to criticism.

In the first place Hume's argument uses as a premiss his general empiristic thesis that any meaningful idea must be capable of reduction to certain definite impressions or sense-experiences.[6] As there is no impression corresponding to the idea of 'necessary connection' between cause and effect, we can safely conclude that such a connection does not actually exist. Of course we have not therewith denied the existence of the idea of such a connection. The origin of this idea is a very interesting psychological problem. Hume himself, as is well known, ascribed its origin to the force of habit.[7]

Secondly, Hume in discussing causality does not distinguish sharply the phenomenological aspect of the question from the physical one. It is, strictly speaking, not clear whether his analysis of causal connection applies to the way in which our impressions of the external objects are connected or to the connection between the physical objects themselves. In his criticism of causality, Hume speaks about external objects in a language of almost naive realism, and totally ignores the problem how the language of sensations is related to the language of things.[8] This in its turn, it might be maintained, makes his arguments seem more plausible than if he had sharply distinguished between the world of sensations and the world of things.

We have next to show that Hume's criticism can be reformulated in such a way that its two above-mentioned peculiarities become irrelevant to the essential points in his arguments. For this purpose we shall conceive of Hume's theory of causation not as a theory about 'matters of fact', but as an inquiry into the grammar of certain words. The question of how far this is in accordance with the intentions of Hume himself will be considered later.

Hume states the essence of his theory in the following words: 'There is no object, which implies the existence of any other if we consider these objects in themselves.'[9] We may re-state this formulation in the following way: From propositions asserting the existence of a certain object or the happening of a certain event,[10] can never follow propositions asserting the existence of another object or the happening of another event, different from the first object and the first event. In this formulation two words, viz. 'follow' and 'different', need further elucidation.[11]

When we say that the impact of the one billiard-ball against the other is something 'different' from the second ball's movement after the stroke, the word 'different' here has a twofold meaning. It means first of all that we experience the two events as distinct, separate happenings. This difference between them we call their psychological difference. But secondly, the events are different also in the sense that the proposition asserting the taking place of the second event is not entailed by, or cannot be deduced from the proposition asserting the taking place of the first event. That is their logical difference.

In the case of the billiard-balls, as in several other cases, logical and psychological difference are concomitant properties.[12] But it may also happen that two events, although they are psychologically different, are not different logically, i.e. the propositions asserting one of them may after all be entailed by the propositions asserting the other. This is not unlikely to be the case if the events are of a rather complicated structure. (In such a case we should be inclined to say that the difference between the events was only 'apparent'.) It must be observed that the arguments of Hume apply only to cases where there is at least logical difference between the causally related events. For if the events are not logically different, then there is actually a necessary connection between cause and effect, that is, 'necessary' in the sense of 'logically necessary'.

A proposition b is said to 'follow' from a proposition a, only if b can be deduced (derived) from *a by means of principles of logic alone.* If b follows from a, then the implication $a \rightarrow b$ and the equivalence $a \longleftrightarrow a \& b$ are logically necessary propositions.[13]

If we substitute these elucidations of what we mean by 'follow' and 'different' in our formulation above of the central point in Hume's theory, this formulation becomes *tautological.* For it then says, that from propositions asserting the existence of a certain object or the occurrence of a certain event, there never follow propositions asserting the existence of another object or the occurrence of another event, if the latter propositions do not follow from the former ones.

It is at once obvious that the validity of Hume's criticism of causality, when reformulated in this way, is independent of the two peculiarities which were mentioned as characteristic of Hume's theory in its 'original' form. For it is not affected by the truth or falsehood of his thesis about the way in which meaningful ideas are related to impressions, nor is it dependent upon whether we speak about sensations of objects or about the objects themselves.[14] It is also quite independent of the *relation* between the physical language and the sense-datum language. This is important, because from the way in which Hume himself formulates his theory together with his general thesis about the derivation of all meaningful ideas from sense-experiences it may appear as if in some way it were essential, for his criticism of causality, that propositions about physical objects were translatable into the language of sense-data. This, however, is not the case.

On the other hand it is easy to foresee that several objections will be made against our statement of the Humean theory of causation. It might be objected first that, although it overcomes certain difficulties present in the theory in its original formulation, it makes the theory itself wholly valueless, as the only thing that remains of it, when reformulated, is a tautology, a mere truism. Second, that surely Hume himself intended his theory to be something more than merely an inquiry into the grammar of certain words, for which reason our formulation omits all the philosophically interesting and controversial points about causality brought forward by Hume. And third, that our conception of the term 'follow' does not cover that kind of necessary connection which, in the opinion of the opponents to Hume's theory, exists between cause and effect.

Our answer to the first and second objections is this. We are not directly interested in the question as to whether our formulations cover the intentions of Hume himself as regards the nature of causal relationship. Nor do we wish to decide whether our theory omits the philosophically interesting points about causality, because it is tautologous. The only thing we wish to do is to show that from our reformulation of Hume's theory of causation can be demonstrated the impossibility of justifying inductions as truths *a priori* by reference to causal connections. As this may be truly said to have been one of the chief aims of Hume's own theory, we believe that our reformulation does more justice to the arguments of Hume than at first seems to be the case. We endeavour, moreover, to suggest that most of the so common outbursts about the absurdity of the Humean theory, about its fatal consequences for practical life, and about its destructive influence upon science, emanate from the failure to see that its importance is primarily *grammatical* and from the unfounded belief that Hume's criticism would in some way weaken the foundation of the actual world-order.[15]

If we could show that causal relationship does not justify induction, then we should have automatically disposed of the third objection against our statement of Hume's argument. For the kind of necessary connection referred to in this objection is just that alleged property of the causal relation which is supposed to justify induction.

We introduce the term 'analytical' to mean logically necessary and the term 'synthetical' to mean that which is neither logically necessary nor self-contradictory.[16] That *b* follows from *a* means, when the word 'follow' is defined as above, that *b* is an analytical, i.e. logically necessary, consequence of *a*. We can now re-state our formulation of Hume's argument in the following form: If the causal relation is synthetical, i.e. if the effect is not a logically necessary consequence of the cause, then the relation cannot be analytically valid. This formulation, obviously, is also a mere tautology. But as actually a great many causal laws, it seems, are formulated and applied as synthetical propositions, it follows that the causal relations with which we are concerned in many cases do not *as a matter of fact* possess analytical validity. This again is no truism, but an empirical proposition. Now the difficulty consists in seeing that if a causal law can actually be shown to lack logical necessity, then it cannot guarantee *a priori* the truth of the inductive generalization which it implies.

It is at this point that Hume, in our opinion, took a great step forward in comparison with his predecessors. The fact that reasoning from cause to effect, as mainly used in practical life as well as in scientific inquiry, is not reasoning involving logical necessity, was seen and pointed out long before Hume by a great many philosophers of various schools and periods. It can therefore be justly said that 'wäre dies der Kern und Inhalt seiner Lehre, so wäre er in der Tat an keinem Punkte über die antike Skepsis hinausgelangt'.[17] It is a most interesting fact which the ancient sceptics already clearly apprehended, that causal reasoning as an inference of one fact from another different one was never logically necessary, and their opinion in this matter was, according to themselves, derived from the teachings of the sophists.[18] During the Middle Ages this opinion was not unknown among scholastic philosophers.[19] Later Hobbes,[20] Malebranche,[21] and Leibniz[22] expressed similar views.

None of these philosophers, however, seems to have realized that if reasoning from cause to effect is void of logical necessity it cannot justify induction. In so far as the inductive problem occurred to their minds at all, they mostly concluded that induction must be justified by means of some rational principle of a not-tautological character. In other words they assumed, for the purpose of justifying induction, the existence of a kind of necessary connection other than the analytical one, a kind of necessity that was assumed to be a possible property of synthetical relations. This assumption is the kernel of the doctrine of synthetical judgments *a priori*. It is expressed very explicitly by Leibniz, who after having shown that there is no reasoning involving logical necessity which from the examination of single instances leads to a universal synthetical truth, says: 'Hinc jam patet, inductionem per se nihil producere . . . sine adminiculo propositionum non ab inductione, sed ratione universali pendentium.'[23]

Hume's greatest contribution to philosophy, we think, consists in having seen that this assumption is unjustified. And as was indicated above we want to show that his demonstration of this can be carried out also on the basis of our tautological reformulation of his argument.

Suppose somebody says that the event *B* is so connected with the event *A*, to which it stands in a synthetical and not an analytical relation, that when *A* has taken place *B* necessarily follows. What does this assertion convey? In the first place perhaps something like this: We feel perfectly sure that after the occurrence of *A*, *B* will follow within a certain interval of time. In so far as this statement is meant to be merely the expression of a psychological fact, which actually in most cases is inseparable from the use we make of a causal law, then it does not tell us anything as to whether *B* *really* will follow *A* or not. It is therefore obvious that reference to this psychological fact does not convey the whole meaning of the assertion that the connection between *A* and *B* is necessary. With this we want to say something *more*, something which at the same time will *justify* the belief or the conviction which we have that *B* must follow *A*.

It is not immediately clear what this 'more', i.e. this rational ground or whatever we like to call it, could be, but it is at any rate obvious that if the justification is intended to be a guarantee for the truth of the causal proposition *a priori*, one of its chief functions is to exclude the possibility of *A* occurring and not being followed by *B*. Suppose, however, that it were alleged that this had happened in spite of this 'more'. How is this to be dealt with? Shall we say that perhaps *B* after all was not in necessary connection with *A*? If we do this, then the causal proposition was itself a kind of induction and as such in need of justification. The only alternative to this again is that we 'save' the truth of the statement that *B* must follow *A* by saying either that *B* actually followed upon the occurrence of *A*, although for some reason it escaped notice, or that the event which took place first was only 'apparently', but not 'really', *A*.

About this 'saving' of the causal proposition the following must be observed. If we save the proposition by *supposing* that one of the two things which fall under the latter alternative be true, then this supposition may later be shown to be false. So we are thrown back on the first alternative, viz. that which made the causal law itself an induction. In order to avoid this we must do the 'saving' in such a way that the proposition that *B* *must* follow *A* within a certain interval of time is made the *standard* for the truth of the expression 'either *B* has escaped notice or *A* was not a "real" *A*'. And if this is done, the causal law becomes analytically valid, i.e. valid because of the way in which certain words are used.

Now it is obvious that if the proposition that *B* necessarily follows *A* is to be true *a priori*, then we must adopt the second of the two above alternatives. But then, as it has been shown, the proposition becomes analytical. This on the other hand contradicts the condition that the relation between *A* and *B* was synthetical. Therefore the assumption that we could justify the belief in a synthetical causal proposition by showing it to be true *a priori* is contradictory.

Thus the 'more than a mere psychological fact' that is contained in the assertion that *B* necessarily follows upon *A*, cannot, whatever this 'more' might be, under any circumstances guarantee the truth of this assertion *a priori*. On the other hand we must not overlook that the psychological fact has some bearing upon the problem of justifying induction. If, by introducing an assertion that a causal relation exists between *A* and *B*, we raise a law about an observed regularity to the higher rank of a causal law, this fact in itself stands as an expression of increased confidence in the proposition that *B* will follow *A*. (This stronger feeling of confidence or reliability, by the way, is *one* of the reasons why we speak of certain generalizations as 'laws of nature', as opposed to others as being mere 'general facts',[24] or why under certain circumstances we regard the reduction of an observed uniformity to another as an explanation of the first one.[25]) It is not altogether out of the way, moreover, to call this increased confidence a *justification* of our previous assumption that the observed regularity could be generalized. We have only to remember that *this* 'justification' does not tell us anything about the 'real' truth of the induction, but is solely an expression of our belief in this truth.

The above conclusion as to the impossibility of justifying induction by reference to causal relations which, although synthetical, were true *a priori*, has been reached by an analysis of the meaning of certain words and expressions. From the way in which the analysis has been pursued it is seen, as will later be shown in detail, that the result applies not only to the causal relation but to all the cases where it is alleged that the truth of a proposition, which is not logically necessary, could be guaranteed *a priori*.[26]

To our analysis the following objection is conceivable. How do we know that the meaning given by us to the analysed expressions is the 'true' one? Is it not plausible to assume that some of those philosophers who have objected to the arguments of Hume have used these expressions with a meaning different from ours, and that this meaning of theirs has enabled them truly to regard causal relatedness, for example, as a means of justifying the truth of inductive assumptions *a priori*?

The dispute about the 'true' meaning is futile. We do not pretend that certain expressions, as used by us, mean exactly the same as when used by other philosophers who have expressed different opinions about the matters here under discussion. By examining some types of argument which are intended to 'refute' those of Hume about causality we endeavour to show that this possible difference in the meaning of terms, does not, however, have any bearing upon the possible difference in opinion.

§3. *Kant and Hume.*

It was mentioned above that Hume in his analysis of causality did not distinguish sharply between the world of things and the world of sensations – between the physical and the phenomenal. Kant too, in his earlier writings does not seem to have been aware of the problems which arise when we begin to speak about things as being different from our sensations of them.[1] It is, moreover, interesting to note that as long as he had not become conscious of the importance of these problems, his view as to the possibility of establishing the *a priori* truth of any empirical proposition was in full accordance with the opinion of Hume.[2]

It is one of Kant's chief merits, however, that he saw with such extraordinary clarity the bundle of difficult philosophical questions which spring up when we pass from the realm of our private experiences – our sensations – to the realm of objective or rather, inter-subjective experience, i.e. to the realm of that which we have called physical objects and events. Kant employs the word 'Wahrnehmung' as opposed to 'Erfahrung' in roughly the same way as we employ the word 'phenomenal' as opposed to 'physical'.[3] The sensations are, so to speak, immediately given to us. The difficulty consists in seeing how we can acquire from them inter-subjective experience, or as Kant puts it, the problem is one of the 'Möglichkeit der Erfahrung'.[4]

It was not until Kant realized the significance of this problem that his attitude towards the possibility of synthetical judgments *a priori* underwent a radical change.[5] In his attempt to solve the problem he arrived at results which, in his own opinion, conflicted with the essential point in Hume's theory of causation.[6] It will be our task to show that Kant's results do not vitiate Hume's doctrine as reformulated above and that they consequently do not cause any modification in our view as to the possibility of justifying induction by means of *a priori* arguments.

According to Kant the passage from our subjective sensations to inter-subjectively valid judgments on nature is possible through a 'Synthesis', i.e. a rational process which transforms the 'material' given by the sensation into an inter-subjectively valid experience.[7] As an illustration of the way in which this 'Synthesis' operates, we will briefly examine the ideas laid down in Kant's famous Second Analogy of Experience, where he tries to prove the necessity of the Universal Law of Causation.[8] It seems plausible to re-state the 'points' in Kant's argument roughly as follows:

In speaking about cause and effect we usually presuppose the existence of an objective time-order, i.e. of inter-subjectively valid judgments about 'before' and 'after'. But how do we know that an objective time exists? Hume spoke about time as a sort of container in which events take place, one after the other. On this basis he showed that causality is merely a regular sequence in time and not a necessary connection between different events. Kant may be said, with Whitehead, to accuse Hume of 'an extraordinary naive assumption of time as pure succession'.[9] He attacks Hume's theory by showing that the assumption of an objective time *rests* upon the assumption *a priori* of the truth of causal laws. Time, in other words, presupposes causality.

The ideas of 'before' and 'after' have, in the first place, a purely 'subjective' meaning, as denoting certain phenomenological features of the way in which sensations succeed one another. When we say that *physical* objects or events stand to each other in the relation of 'before' and 'after', temporal relatedness means something entirely different. In order to see how the mind acquires the idea of an objective time, we have to examine more closely the nature of our experiences of successions among sensations.

I cast my eye on the wall of a house and view it from top to bottom. My sensations of the various parts of the wall are, in the phenomenological sense, temporally related. I also follow with my eye a vessel floating down a stream. Here again my sensations of the various positions of the vessel are temporally related. Nevertheless I am inclined, in the former case, to say that the various parts of the (physical) house exist simultaneously, whereas the various (physical) positions of the vessel exist successively. In the one case 'before' and 'after' in the succession of sensations correspond to 'before' and 'after' in the realm of things, in the other case not. How does the mind arrive at the notion of this difference?

We get the answer by pointing, with Kant, to a remarkable difference in the succession of sensations in the two cases. In the case of the house this succession can be *reversed*: instead of experiencing the parts of the wall from top to bottom, I might have experienced them in the opposite direction. But in the case of the vessel, the succession of sensations cannot be reversed. As Kant himself remarks: 'Ich sehe z.B. ein Schiff den Strom hinab treiben. Meine Wahrnehmung seiner Stelle unterhalb folgt auf die Wahrnehmung der Stelle desselben oberhalb dem Laufe des Flusses, und es ist unmöglich, dass in der Apprehension dieser Erscheinung das Schiff zuerst unterhalb, nachher aber oberhalb des Stromes wahrgenommen werden sollte. Die Ordnung in der Folge der Wahrnehmungen in der Apprehension ist hier also bestimmt, und an dieselbe ist die letztere gebunden. In dem vorigen Beispiele von einem Hause konnten meine Wahrnehmungen in der Apprehension von der Spitze desselben anfangen und beim Boden endigen, aber auch von unten anfangen und oben endigen, imgleichen rechts oder links das Mannigfaltige der empirischen Anschauung apprehendieren. In der Reihe dieser Wahrnehmungen war also keine bestimmte Ordnung.'[10]

It is on the existence of such *irreversible* series of successive sensations that the possibility of an objective time is founded. If there existed no irreversibility in the flux of sensations, it would not be possible to talk about 'before' and 'after' in any other than the phenomenological sense of the words.[11]

The statement that a series of sensations is irreversible is a law or rule determining the order in which sensations succeed one another. As ordering rules irreversibility-statements are, in the terminology of Kant, *causal laws*.[12] Thus he was entitled to say that the 'Synthesis' which, from the material given by the sensations, takes us to the idea of an objective time, is possible for the reason, that the succession of sensations is, in characteristic cases, governed by causal laws. This is the meaning of the above assertion that 'time presupposes causality'.

The 'Synthesis', giving to the sensations their 'Gegenständlichkeit', is always carried out in a manner analogous to that which leads to objective time from the time-sensations. In order that inter-subjective experience may be possible, i.e. in order that we may be able to speak about a world of things as opposed to a world of sensations, it is a necessary condition that there shall exist certain invariant relations or laws governing the stream of sensations. These invariances – these laws prevailing in the world of sensations – constitute the objective, physical or 'real' world. The physical is the invariance in the phenomenal.[13] Nature is the sum or the system of all the laws which regulate our subjective experiences.[14] If there were no invariance, no law and order in the realm of sensations, there would be no physical world and no inter-subjective experience.[15] Kant's 'Erfahrung' would thus not be possible.

Kant is not the first philosopher to have the idea that the physical is the invariance in the phenomenal. Leibniz had already formulated the idea clearly.[16] What is new in Kant is, above all, the idea of 'deducing' from the conception of inter-subjective experience itself a set of rules to which the invariances defining the physical world have to conform. With one of these rules we are already familiar, namely the Universal Law of Causation, the deduction of which has been outlined above.[17] It states that 'alles, was geschieht (anhebt zu sein), setzt etwas voraus, worauf es *nach einer Regel* folgt'.[18]

To the rules which are deduced from the general idea of 'Erfahrung' there correspond certain general concepts, called the categories.[19] The category corresponding to the Universal Law of Causation is that of Causality. The rules are said to subsume the experiential content of the sensations under the general concepts of the categories.[20]

According to Kant these rules, in conformity with which the subsumption takes place, are synthetical judgments *a priori*.[21] They are synthetical since they, apparently, assert something about the course of nature. And they are *a priori* since their truth is a necessary condition for the possibility of inter-subjective experience. They cannot be contradicted by experience, because experience itself presupposes them.

The Universal Law of Causation therefore is, according to Kant, synthetical and *a priori*. How is this to be understood, and does the doctrine of Kant have any bearing upon the problem of Hume?

The answer to these questions can be got from studying again the arguments in the Second Analogy. The synthetical aspect of causality lies in the fact that there exist sequences of phenomena conforming to causal laws, or else, that causality prevails in this world *as far as our experience goes*. The aprioristic aspect consists again in that the prevalence of causality (for past and for future experience alike) can be used to *define* an objective time-order. Now the seductive element which makes us think of the Universal Law of Causation as being at the same time synthetical and *a priori*, seems to be this:

The actual possibility of inter-subjective knowledge indicates that time in the objective sense of the word exists as a matter of fact. From this follows, prior to any further experience, the unrestricted prevalence of causality, since causality was, so to speak, a defining characteristic of time.

In this reasoning, however, there is a serious fallacy. All we actually know is that *hitherto*, on account of certain uniformities of the way in which phenomena have occurred to us, it has been possible to arrange experiences in an inter-subjectively valid order of time, defined by these uniformities themselves. From this we may conclude that *if* uniformities of the same kind, i.e. causal uniformities or causality, are continuously going to exist, then it will *always* be possible to arrange experiences in this objective order. It does not, however, follow from the transcendental deduction of the Universal Law of Causation *that* uniformities of the kind mentioned are going to pervade also the realm of our coming sensations. That such will be the case, i.e. that the law of causation will be true, is an inductive generalization on the basis of what is hitherto known to us. This generalization is a synthetical proposition the truth of which has not been proved *a priori*.

The Universal Law of Causation, therefore, can *a priori* be made a necessary condition for the existence of an objective time, but the truth of it *qua* synthetical, i.e. as a proposition about the continuous existence of time, cannot be established in advance. The apparent possibility of doing so, by reference to time as a matter of fact, disappears if we consider that we are thus referring, not to the existence of *time in general*, but to the existence of *time up to a certain point*. And from this does not follow the unconditioned validity of causality, or in other words, the continued existence of time.

Analogous arguments apply to the other categories and to the judgments about the form of experience corresponding to them. In laying down defining criteria of inter-subjective experience these judgments are *a priori* and analytical. As generalizations about certain characteristic features of the phenomenal world they are again inductive, synthetical judgments, the truth of which cannot be proved *a priori*.

The foregoing reasoning also makes it clear why the transcendental deduction does not lead to a vitiation of Hume's results as to causality and the justification of induction. We know that certain inductions, among them causal laws, have held true hitherto, and that if henceforth no inductions or no inductions of a certain type are going to hold true also, then inter-subjective experience will not be possible any more. But that which would be necessary in order to justify induction, does not follow from this, viz. that causality and induction are *actually* going to hold also for the future, or, generally speaking, that inter-subjective experience is going to continue.

§4. *Kant and the application-problem.*

Let us suppose that we had established *a priori* the truth of the Universal Law of Causation, formulated as for example by Kant above.[1] The following question may now be asked: In what way, if at all, can the knowledge of this universal truth be applied to the establishment of actual causal connections between concrete events? Or, in other words, in what way can it provide us with a justification of specific inductions? This problem we shall call the application-problem of the Universal Law of Causation.

It is easy to see that the question asked can only be answered in one way, viz. that it is not possible by means of the Universal Law of Causation alone to establish general propositions about the actual course of events.[2] This seems to be admitted by Kant, when he says: 'Auf mehrere Gesetze aber als die, auf denen *eine Natur überhaupt* als Gesetzmässigkeit der Erscheinungen in Raum und Zeit beruht, reicht auch das reine Verstandesvermögen nicht zu, durch blosse Kategorien den Erscheinungen *a priori* Gesetze vorzuschreiben. Besondere Gesetze, weil sie empirisch bestimmte Erscheinungen betreffen, können davon *nicht vollständig abgeleitet* werden . . . Es muss Erfahrung dazu kommen, um die letztere überhaupt kennen zu lernen.'[3]

But what Kant in his *Kritik der reinen Vernunft* has overlooked is that, if the Universal Law of Causation does not help us to a justification of any single induction, then it does not provide us with a satisfactory solution of Hume's problem. If Kant's transcendental deduction does not enable us to guarantee the truth of any inductive generalization, then it leaves the logical problem of induction at precisely the same point where we had left it with Hume.[4]

On the other hand it seems obvious that Kant believed himself to have solved the problem of Hume, and never realized that the obstacle put to his theory by the application-problem deprived it in fact of any bearing upon the inductive problem.[5] In his later writings, however, he became aware of some difficulties on this point.

There are some very obscure passages in the *Prolegomena* where Kant tries to answer the question as to how the Universal Law of Causation is applied to concrete cases.[6] From these contexts one gets the impression that he believed the category of causation to be applicable to special cases in such a way that it could raise observed regularities to the higher level of universal and necessary laws. It is, however, uncertain what Kant here really means by 'necessity' and 'universality'. Does he mean simply the property of inter-subjective validity, belonging to physical propositions as opposed to phenomenological ones, which arises from the application of the category to the material given by the sensations, or is he thinking of necessity and universality in *that* sense which justifies induction? Some arguments could be put forward in favour of the first interpretation,[7] but it seems to us most likely that Kant himself did not clearly separate the two aspects from each other, and therefore can be said to have tried in some obscure way to include, even if he had the first one predominantly in mind, the second one also in his argumentation. This view is strengthened by the fact that Maimon, when he criticized Kant's treatment of the problem of Hume and showed that the Universal Law of Causation alone is not sufficient for the establishment of any single causal law, seems to have been inspired by those very examples by means of which Kant in the *Prolegomena* apparently tried to show just the opposite.[8]

Later, however, in the *Kritik der Urtheilskraft* Kant is quite clear as to the impossibility of deducing special laws of nature from the Universal Law of Causation.[9] For the establishment, therefore, of true inductions other principles than those deduced from the general idea of 'Erfahrung' are needed.[10] In his attempt to formulate these principles Kant approaches the rules and canons, like the principles of Uniformity of Nature or Limited Independent Variety, laid down in various attempts to establish a so-called 'inductive logic'.[11] With these attempts we shall be concerned later on. It is here sufficient to note that Kant explicitly regarded these principles as not being provable *a priori*, but merely as 'subjective'[12] assumptions.

§5. *The inductive problem in the school of Fries.*

Our result so far is then that Kant was not able to show the Universal Law of Causation to be synthetical and true *a priori*. Furthermore it has been proved that even if Kant had shown this, the difficulties raised by the application-problem would have deprived his result of all importance for the problem of Hume.

Jacob Friedrich Fries and his followers, both his immediate disciples and the adherents of the recent Neo-Friesian school, are of importance for the discussion of the present problem both on account of their ideas on the way in which synthetical judgments *a priori* are to be established, and because of the special attention given by them to the application-problem of those judgments and its bearing upon the justification of induction.

According to the Friesians the idea of deducing synthetical judgments *a priori* from the concept of inter-subjective experience, or from any other general principle, is fundamentally unsound. For either it makes those judgments analytical,[1] or it raises the further problem as to the truth of the principle from which the deduction is made.[2] If this principle is to be established by reference to a new idea we are involved in an infinite retrogression.[3] Therefore, as Fries rightly points out,[4] it is not possible by the transcendental method of Kant to establish any judgment as being both synthetical and *a priori*. This impossibility follows from the idea that those judgments must be *proved*, i.e. deduced from superior principles. Fries speaks of this idea as 'das rationalistische Vorurtheil'.[5]

Fries himself tried to establish synthetical judgments *a priori* by reference to a source of knowledge, called by him 'unmittelbare Erkenntnis'.[6] This immediate knowledge is a sort of mental fact, which is 'urtheilsmässig wiederholt'[7] in the form of synthetical judgments *a priori*. It is, in other words, a purely empirical, singular, and *a posteriori* detectable fact that we are in possession of general knowledge which is synthetical, in the sense that it contains information about what is going to happen, and *a priori* in the sense that we know it to be true generally prior to actual testing.[8] To ask for a *proof* of these judgments would be wholly to misunderstand the question, as our knowledge of them is simply *a fact*. As such they are to be regarded as starting-points for philosophical investigations and not as something which these investigations themselves ought to establish or justify.[9]

Even if the criticism of Fries and his followers rightly points to some defects in Kant's theory of synthetical judgments *a priori*, it ought not to be difficult to see that the new theory offered by the Friesians is no more successful as regards the establishment of these judgments than the rejected one. That such is the case can be shown by an example.

Suppose the judgment that all *A*'s are *B* to be synthetical and *a priori* according to the theory of Fries. Suppose further that somebody claims to have found an *A* which is not *B*. This, a Friesian would say, is impossible as we *know* that all *A*'s are *B*. But what does it mean that we 'know' this? It may mean, for example, that we *use* the predicates A and B in such a way that always when *A* is present we say that *B* is present also. If somebody claims to have found an *A* without *B*, we should tell him that either *A* was no 'real' *A*, or *A* was only 'apparently' lacking *B*. In this case, however, the proposition that all *A*'s are *B* would be analytical as it provides us with a *standard* for when the proposition 'either *A* is not a real *A*' or '*A* is only apparently lacking *B*' is true. Therefore this possibility must be ruled out.

But if, by 'knowing' that all *A*'s are *B*, we mean something else, whatever it may be, we cannot exclude the possibility of there being an *A* which 'genuinely', and not merely 'apparently' lacks *B*. This is seen by considering how the phrase 'apparently lacking *B*' may be defined. It can be defined as above, i.e. the predicate *B* is *used* in such a way that it is attributed to all *A*'s, for which reason every (real) *A* in which *B* is not as yet detected is said 'apparently' to lack *B*. This, however, is not the only possible, nor the most natural definition of the phrase. '*A* apparently lacks *B*' can also mean that it has often happened that *B* has first been detected in *A* upon closer investigation and that we suppose the case in question to be of this kind. Here again, however, the statement that *A* apparently lacks *B* is a hypothesis about what is going to happen after closer investigation of the circumstances, and it is not possible to forecast whether these investigations will actually lead to the detection of *B* or not. If we cannot decide this, however, we cannot decide whether *A* lacks *B* 'genuinely' or only 'apparently' so that if somebody claims to have found an *A* without *B*, we cannot decide in advance whether the case is one where *B* really is lacking, or whether it has merely escaped our attention so far. This again means that we cannot exclude the possibility that the judgment '*A* is not *B*' is true, or in other words that we cannot be sure *a priori* that all *A*'s are *B*.

If, therefore, 'knowing' that all *A*'s are *B* is to mean something which makes the proposition not analytical – which can perfectly well be the case – then the proposition can never at the same time be true *a priori*. Thus the Friesian way of establishing synthetical judgments *a priori* is as insufficient as that of Kant.

It has been mentioned that the Friesians have paid some attention to that problem which presented a serious obstacle to the doctrine of Kant, viz. the question of how to arrive at special inductions from the general principles established by *a priori* reasoning. Apelt's work *Die Theorie der Induction* illustrates the difficulties in which the theory of synthetical judgments *a priori* is necessarily involved when it has to show its applicability to concrete cases. We shall here give a short account of Apelt's ideas.

Pure induction, i.e. the examination of successive instances of, say, *A*'s which are *B*, is not able alone to assure us of the truth of the general proposition that all *A*'s are *B*.[10] But if we can deduce this inductive proposition from a system of general knowledge already known to us, it becomes a law, i.e. a generally and necessarily true proposition about nature.[11] From this it would appear that in the opinion of Apelt not only certain principles of a very general kind, such as the Universal Law of Causation, but also every special law of nature were a synthetical proposition *a priori*.[12] How is this to be understood?

The explanation seems to be something like the following: From a system of general principles alone it can be deduced that *if* there is any *A* then it must be *B*, but not that *A*'s actually exist. Thus for the establishment of the general proposition that all *A*'s are *B* induction is necessary in order to show that *A*'s being *B* actually *exist*, and deduction again is necessary in order to prove the universality of this fact.[13] Thus induction may be called the bridge which leads from the facts to the laws,[14] from the contingent experiences to the necessary truths.[15]

This interpretation of the theory is confirmed by the instances given by Apelt. He says for example that the planetary movement of Mars can be deduced from the general law of gravitation, but that the actual orbit of the planet has to be determined empirically.[16] As another instance of how induction works Apelt mentions Bradley's detection of the aberration of light which was made empirically and afterwards proved to be a consequence of general physical principles, this proof giving it the character of a general and necessary law.[17]

It is clear that this theory of induction is entirely based upon the assumption that induction from facts and deduction from general principles always give concordant results. There is one extremely interesting passage in Apelt's work where he tries by means of an example to show the impossibility of a contradiction between the principles and the facts. Daniel Bernoulli and Laplace stressed that the law that force and acceleration are proportional is no *a priori* truth, since it is conceivable that experience might show the force to be proportional, say, to the second power of the acceleration. Against this Apelt says:[18] 'Dies ist jedoch eine Irrung. Wenn ein Naturforscher einen Fall fände, bei welchem die beobachtete Grösse der Veränderung einer Bewegung mit der anderweit bereits bekannten Intensität der Kraft nicht übereinstimmte, so würde er das Gesetz $f = \frac{dv}{dt}$ nicht in Zweifel zeihen, sondern er würde vermuthen, dass ausser der zur Erklärung der Erscheinung angenommenen Kraft noch andere Kräfte mit im Spiele seien. Es lässt sich gar keine Beobachtung machen, die diesem Gesetze zuwider wäre, eben weil es nicht aus der Erfahrung folgt, sondern vor jeder bestimmten Erfahrung schon *a priori* feststeht.'

This is a most beautiful instance of how the old doctrine of synthetical judgments *a priori* approaches the doctrine, called conventionalism, which will be examined in the next chapter.[19] Here it is sufficient to note that if the impossibility of a contradiction between principles and facts is a consequence of the way in which we have to interpret the facts, then these principles become analytical. If for example an apparent contradiction between the observations and the law $f = \frac{dv}{dt}$ is to be interpreted as being due to the presence of hitherto unobserved forces, then the law is nothing but the definition which enables us to decide when we have to speak of unobserved forces and when not. For if the law is not made the definition of the presence of such forces, but only a 'symptom' or 'indicator' of them, then it is not *a priori* certain that these forces really are there and that it is not the law after all that is false. But if the truth of a principle is a truth guaranteed *per definitionem* then the principle is analytical.

The following we can say in conclusion about this theory of induction: The school of Fries is in advance of Kant in that it has seen that if synthetical judgments *a priori* are to help us to a solution of Hume's problem then it must be possible to deduce all laws of nature from these judgments, and thus make them too synthetical and *a priori.* The attempt to do this, however, meets with insurmountable difficulties when we come to the question of how to exclude the possibility that experience will contradict the *a priori* principles and laws. Out of this difficulty there are only two ways. Either we retain the aprioristic nature of the principles and laws, in which case they become analytical, or we retain their synthetical nature, in which case they can no longer be known to be true *a priori.* We have also seen that in the only case where one of the adherents of the Friesian theory faces this question, he apparently decides in favour of the former alternative, without, however, realizing that therewith he has also given up the doctrine about judgments being *both* synthetical *and a priori.* This decision of his also indicates the course which our further investigations as to the possibility of justifying induction by means of *a priori* arguments have to take. Before this, however, we have to show that certain other more recent theories, which are put forward in opposition to Hume's view about causation, also lead to difficulties which point in the same direction for their solution.

§6. *Some other theories of causation.*

Although the failure of the above attempts to solve the problem of Hume by reference to synthetical judgments *a priori* is quite commonly admitted nowadays, it does not follow that modern philosophy had unanimously accepted Hume's results. There are, on the contrary, a multitude of theories put forward against his. The most important ones are perhaps those of Whitehead on causal perception, of Meyerson on scientific explanation, and of Bradley and Bosanquet on concrete universals.

It might be said that all these theories have the idea in common that a right understanding of the nature of causal relationship requires a much more careful analysis of the concrete cases where such relationships are manifested than Hume gives.[1] And it is alleged that this closer examination of single instances of causal relationship will reveal to us the necessity which Hume denied the relation between cause and effect to possess.[2] 'Thus in the analysis of particular facts universal truths are discoverable.'[3] We shall next discuss the different meanings that this thesis might have, and its bearing on Hume's results.

The proposition that it is possible to arrive at general knowledge from singular facts or to anticipate an event *B* after the observation of the event *A* can mean all sorts of different things. It may first of all mean that *as a matter of fact* singular facts suggest to us general conclusions. This is no mere truism but a point of considerable importance, if we consider the following:

Hume says, on one occasion, that 'the effect is totally different from the cause . . . nor is there anything in the one to suggest the smallest hint of the other'.[4] This phrase interpreted in its most natural sense, is certainly false. Not only is there in general something in the cause which 'points' in direction to the effect and lets us anticipate it, but that such is the case is a most important and interesting feature of the world in which we live and the way in which we react to things. We are, in our daily life, constantly confronted with situations which are not very similar to situations with which we are familiar and in which we are compelled, without the aid of previous experience, to anticipate the right course of action. As a matter of fact we do this in a manner which clearly shows the untenability of Hume's view that the belief in causal connection arises as a mental habit produced by the repeated impressions of the same succession of events.[5] On this point Whitehead's theory about immediate causal perception gives a much better account of the way in which we arrive at the knowledge of causal laws than Hume's theory about the force of habit.[6]

If, therefore, the thesis that general knowledge is detectable in single facts is interpreted as above, it expresses an important truth. The manner, moreover, in which Whitehead treats it offers an explanation of how we as a matter of fact arrive at inductive knowledge, and this explanation is much more satisfactory than that given by Hume. But it is also clear that with this interpretation of the statement we do not obtain anything of relevance to the problem whether it is possible prior to verification to *guarantee the truth* of general propositions.

From this it follows that the above interpretation is not sufficient for the purpose of contradicting Hume's result as to the question of justifying induction.[7] But the same phrase can also be interpreted differently. That the single instance of an induction may contain general information can also mean that an analysis of the single case will show this to have such a constitution that the truth of the general proposition follows from it. Or, if it is a question of a causal law, the analysis may show the effect to be a necessary consequence of the cause.

This interpretation seems to fit in peculiarly well with Meyerson's theory of 'scientific explanation'. Expressed in our previous terminology, the essence of this could be formulated as follows: Hume maintained that the effect is something 'different' from the cause and can therefore not be a necessary consequence of it. We have seen that this is true if 'different' means 'logically different'. But difference may also mean psychological difference and of such cases Hume's statement need not necessarily be true. In other words, cause and effect may have a different appearance, but nevertheless upon closer investigation may be shown to be the *same* in the sense that the effect logically follows from the cause, or is contained in it. Now the theory of Meyerson seems to suggest that always where there is a question of real causal relationship, the effect upon closer investigation can be explained as a necessary consequence of the cause.[8] If this is possible the effect is said to be 'identical' with the cause.[9]

Now it is not our intention to show Meyerson's statement to be false. We shall, on the contrary, soon have occasion to show that it is true, if not for all, at least for a great many such cases where we speak about causal relationship. We only want to show that if the effect is identical with the cause, in the sense of being a logical consequence of it, then the causal law, i.e. the inductive proposition based upon this connection must be analytical.

The fact that a single *A* and a single *B* stand in necessary causal relationship to each other can be generalized to the statement that when *A* occurs it will always be followed by *B*, if the *A* which occurs repeatedly really is *the same A* in all the cases.[10] Sameness here of course does not mean spatio-temporal identity but sameness in the sense of having in common all the characteristics relevant to the causal property. Under such circumstances the inductive proposition is true, because if there were an *A* apparently not followed by *B*, we can safely conclude that this *A* although it may have a certain resemblance to the previous *A*'s is not of the same kind as those *A*'s which should produce *B*. If it 'really' were the same then it would produce *B*.[11] Sameness in other words has been defined so as to include the property of having the occurrence of *B* as a logical consequence. On the other hand, if we do not define sameness in this way, which is perfectly well conceivable, we cannot be sure in advance of the truth of the inductive proposition. And if we define sameness so that we can be sure of this then the proposition becomes analytical. It is this because it follows from the definition of two *A*'s being 'the same'.

Thus if the theory of Meyerson is to lead to the establishment of general causal propositions *a priori* known to be true then these propositions must be analytical. This is a consequence of his theory of which Meyerson himself does not seem to have been aware.[12]

The circumstance which forced the theory of Meyerson to make inductive propositions analytical if their truth is to be guaranteed *a priori* was the difficulties presented by the definition of sameness. It seems to us plausible to say that the theory of causation put forward by Bradley and Bosanquet is an attempt to meet this difficulty with the explicit purpose in mind of making the inductive propositions, based upon causal relationship and known to be true *a priori*, synthetical and not analytical.[13]

This is done in the theory of 'concrete universals'. To suppose *A* to be a concrete universal or, in the terminology of Mill, a natural kind,[14] is to suppose that although any instance of *A* may have an unlimited variety and multitude of properties, these properties are bound up with each other in such a way that the repetition of a few of them brings with it the repetition of the whole, probably infinite, number of properties belonging to the universal.[15] Thus in our example above it seems unnecessary for the purpose of establishing the truth of the proposition that *A* will always be succeeded by *B* to define sameness in a way which makes the proposition analytical, but it is sufficient to assume that the *A*'s are instances of the same natural kind. If it is once shown, as is supposed to have been the case, that one instance of the universal has the power of producing *B* then any instance of it will have the same power. And in order to know that two *A*'s belong to the same natural kind it is necessary only to know that they have a limited number of properties, say *XYZ*, in common. As soon as *XYZ* is repeated in *A*, *A* will be followed by *B*.

Let us assume this theory to be true in so far as that concrete universals really exist. There remains, nevertheless, a difficulty which none of the supporters of the theory seems to have noted. How is it possible to know that the properties *XYZ* are sufficient for the determination of the objects which belong to the same natural kind? Suppose that *A* once produced *B* and that another *A*, having at least *XYZ* in common with the previous one does not produce *B*. Then we must assume that *XYZ* was not sufficient for the determination of the members of the kind. We should say for example that a fourth characteristic is needed for this determination, and that the absence of this in the second case prevented *B* from following. If this characteristic is supposed to be *T* and it again happens that an *A* which has *XYZT* in common with the first one does not produce *B*, we have to look for a fifth characteristic and so on. In other words, if we want *a priori* to be sure that *A* will always be followed by *B*, we have to define the universal which guarantees this truth in such a way that if an *A* is not followed by *B* then there must exist some ultimate property of the universal, not yet observed or taken into account, which is not possessed by the *A* in question. But in this case the truth of the inductive proposition that *A* will always be followed by *B* becomes analytical. If the theory of concrete universals was intended to establish the proposition as synthetical and true *a priori*, it has failed to do so.

We may thus conclude that in whatever way we interpret the thesis that singular facts provide us with general information, we cannot overthrow Hume's results as to the impossibility of guaranteeing *a priori* the truth of inductive propositions *qua* synthetical. Either an inductive proposition remains synthetical, and in this case it is not possible to decide anything as to its truth *a priori*, or we can guarantee its truth, in which case the proposition becomes analytical. If we lay emphasis primarily upon the guaranteeing of the truth, then we are driven to accept the alternative that justifiable inductions are analytical sentences. This consequence, as was shown above, also applies to the older doctrines of synthetical judgments *a priori*.

§7. *General remarks about synthetical judgments* a priori.

The reader should have observed above that when we have had to show the inefficiency of a certain philosophical theory to guarantee *a priori* the truth of a general synthetical proposition, we have always made use of one and the same 'technique'. Finally, we must say a few words about the justification of this 'technique', and about the general significance of the results at which we arrive by its aid.

The method used by us has been, generally speaking, as follows: Suppose that the proposition 'all *A*'s are *B*' is said to be synthetical and *a priori*. Suppose also that somebody maintains that there is an *A* which is not *B*. How is such a situation to be judged?

The possibility that the latter of the two assertions about *A* and *B* is true can be excluded, it seems, in two ways. Either we use the terms *A* and *B* in such a way that the phrase '*A* is not *B*' would contradict that use; or, if this is not done, the proposition is shown to be false for *some other reason*, (e.g. careless observation, a mistake in the records, a deliberate lie or anything). But in order to guarantee *a priori* that some such reason will be present we must determine exactly what is to be *called* a reason against the existence of an *A* which is not *B*, in such a way that the presence of such a reason follows in *any* situation where it is maintained that an *A* is not *B*.

In either case the truth of the proposition that all *A*'s are *B* follows from the use or the definition of certain terms.[1] Consequently the proposition, if its truth is to be guaranteed *a priori*, must be analytical.

The whole reasoning hangs upon the assumption that it is possible to deny a synthetical judgment *a priori*. It is uncertain how far this assumption would be in explicit accordance with known theories about such judgments. In general it is not possible from the contexts in which those theories are expounded to find a clear answer to this question. Kastil, in discussing different theories of synthetical judgments *a priori*, assumes it to be concordant at least with some of them to give an affirmative answer to the question whether such a denial is possible or not.[2] But it must be observed that even if the answer, as given by any philosopher, were in the negative, it would have only an apparent bearing upon the method of reasoning employed by us. This is seen in the following way:

The word 'possible', in this context, may mean several different things. It is plausible to assume that some philosopher would like to employ the word in such a sense that the negation of a synthetical judgment *a priori* becomes, in his terminology, 'impossible'. But when we say that such a negation is 'possible', we simply intend to say that *as a matter of fact* it might happen that somebody asserts a proposition of a form contradictory to the form of the general proposition, and *this* possibility cannot be denied by anybody.

We can thus end this chapter with the conclusion that *the only way to guarantee* a priori *the truth of general propositions is to make them analytical.* In the next chapter we shall see what bearing this result has upon the question of justifying induction.

CHAPTER III

CONVENTIONALISM AND THE INDUCTIVE PROBLEM

§1. *The way in which conventions enter into inductive investigations. Some examples.*

We know from chemistry that the melting-point of phosphorus is 44° C. We have obviously arrived at this result in what may be termed an inductive way, i.e. we have melted different pieces of a substance known to us under the name of phosphorus and found that they all, suppose the experiment to have been carefully performed, melted at the same temperature of 44° C. From this fact we generalize that *all* pieces of phosphorus will melt at 44° C or, as we also express it, that the melting-point of phosphorus is 44° C. This inductive generalization is of the form $(x)\ [Ax \rightarrow Bx]$, if A is the property of being phosphorus and B the property of melting at 44° C.

In considering what justified our making this generalization, it immediately occurs to us that the justification is not solely in the experimental facts as such, but that the clue to it essentially lies in the multitude of circumstances and qualifications which determine the correctness and the significance of the experiments. If somebody, for example, had made a number of haphazard experiments under strange conditions and based a generalization upon his results we should not have attached much weight to it, even if the number of experiments were great and all had led to concordant results.[1] Let us therefore consider the essentials of what we call a correct experiment.

The fundamental condition is that we have reliable criteria which enable us to decide when it is with a piece of phosphorus that we are dealing and when not. It is quite conceivable that we are not able to enumerate exactly the criteria used by us in our experiments, and a scientist in the first place would hardly bother about such an enumeration. But certainly we have relied upon *some* criteria, as we have chosen a definite substance and not at random for the experiments. Let us assume these criteria to have been K, L, M, e.g. macroscopic properties such as colour, smell, taste, etc. We shall for the present be concerned only with these criteria, and shall assume all other things about the conditions of the experiment to be settled.

Suppose then that we find a substance having the characteristics K, L, M, which does not melt at 44° C. Does this fact imply that our previous generalization as to the melting-point of phosphorus is falsified?

Obviously what has happened *could* be regarded as a falsification of the law. But there is also another way left open which, in the practice of science under similar circumstances, is very often resorted to. We simply declare that the last examined substance cannot have been a piece of phosphorus at all. The property of melting at 44° C, which originally was an observed 'empirical' property of substances, already known to us under the name of phosphorus, is thus made a *standard* for what may be called phosphorus and what may not. If this is done the generalization that all pieces of phosphorus melt at 44° C can never be falsified, i.e. is absolutely true under any circumstances whatsoever.

Here we have a case of an 'inductively' established generalization being absolutely true. If we consider wherein the reconcilability of absolute truth and induction consists, we find that it has its root in the fact that the word 'phosphorus' has been used during the course of the investigation in what may be termed a quasi-ambiguous way.

At the outset we used it to denote a substance characterized by a number of properties with which we were already familiar — a certain colour, smell, taste, macroscopical structure and so on. In enunciating the law about the melting-point we were in the first place enunciating an empirical discovery,[2] viz. that the substances with the properties mentioned were found to have a further property — the melting-point — in common. In so far as the generalization 'all pieces of phosphorus melt at 44° C' is to mean that, whenever in the future we find a substance with the first-mentioned properties, it will exhibit the further discovered property too, then this generalization is a hypothesis which later experience may confirm or refute.

Although the word 'phosphorus' was used at the outset for substances exhibiting the properties mentioned, it is by no means certain that we, even at the beginning, wished to *define* phosphorus as a substance exhibiting these characteristics.[3] We simply asserted that these substances were phosphorus, as though phosphorus were something fixed and given, about whose definition we never need to bother. A certain co-existence of a number of easily observable properties has made us familiar with something called phosphorus, and which of these properties are defining or fundamental ones and which again empirical and accidental, is a question which never before occurred to us. The first time that we were confronted with it was perhaps in the above situation, where the properties *K*, *L*, *M*, which we used to regard as criteria of phosphorus, are present, but a further property which hitherto always accompanied them is absent.

Now the question occurs: what then *is* phosphorus? Is the substance now under examination phosphorus or not? In such a situation it is quite conceivable that we should renounce every pretension of regarding the properties *K*, *L*, *M* as the 'true' criteria for what may be called phosphorus and should find the experimentally discovered property, especially if it is exactly measurable and sharply distinguishable from other characteristics, more convenient for this purpose. But at the same time we make the inductive generalization true *per definitionem*, i.e. absolutely true as being an analytical proposition.[4]

On the other hand the fact that we in this way 'save' the truth of the inductively established proposition by making it analytical, does *not* necessarily imply that we announce the melting-point itself as a defining property of phosphorus. This certainly is a possibility close at hand, but there is also another way open.

In making our decision as to whether an instance of *K*, *L*, *M* not melting at 44° C is or is not a falsification of the law of the melting-point, we have regard to a multitude of circumstances. It is very plausible to assume that among those 'circumstances' are to be found assumptions which are themselves inductive. For example, we may suppose that if a substance with *K*, *L*, *M* does not melt at 44° C then it differs from phosphorus also in other properties,[5] in, say, its microphysical structure, and this difference is the 'cause' of some substances with *K*, *L*, *M* melting at the temperature in question and others not melting. The 'probability' which we attach to assumptions of this kind will influence our decision.[6]

Thus we can decide to regard the law about the melting-point as true, not because the melting-point itself defines phosphorus, but because it indicates the presence of another property that explains why phosphorus melts at exactly this temperature. Here it is important to observe that even if we do not know of any such property, or if every property that is assumed to cause the characteristic melting-point of phosphorus is shown later not to be the cause looked for, it may nevertheless be plausible, on what we know, for example, of other substances and their melting-points, to *postulate* its existence.

This possibility is important for the following reason. If we, as a matter of actual fact, had to save the truth of a general proposition, such as that about the melting-point of phosphorus, by saying that a substance which does not melt at the temperature in question cannot be phosphorus, then *we should almost certainly not want to say anything decisive about the definition of phosphorus.* We would rather say something like this: Perhaps the melting-point can be used for the purpose of defining phosphorus, but perhaps also there will be found some 'deeper' quality of the substance which will explain why *phosphorus* melts at just 44° C. But irrespective of which alternative finally is to be chosen, we wish under all circumstances to adhere to the norm that phosphorus melts at 44° C. Whether it is because the temperature *defines* phosphorus, or whether it is because it only *indicates* some true criterion of that substance, is a question not as yet considered, and one that need not be settled in this connection.[7]

We shall next consider another case, slightly different from the former one, which gives a new illustration of the way in which conventions enter into inductive investigations. As an example we shall take the famous instance of the billiard-balls.

We have observed that the impact of one billiard-ball against another is, so far as our experience goes, followed by the movement of the second ball. From the observed fact we might conclude that the impact of the first ball is the cause of the second ball's movement, implying that whenever the first ball strikes the second the latter will move, which again is an inductive law or generalization. What of its justification? Is it really possible that the law, which seems to us so obvious, could be false; that one day it might happen that, although one ball strikes another, the second one is left unmoved? Or is there something to exclude this possibility *a priori*?

Let us suppose, in order to find the answers to these questions, that it has actually happened that a ball is struck by another but is left unmoved. We should then under no circumstances immediately say that the previously enunciated law has been falsified. Instead of this we should investigate the more closely the circumstances under which the impact has taken place in order to find an 'explanation' of what happened, i.e. to show generally speaking, that what happened was in accordance with some general law operating against the law which we were in the first place considering. Suppose, for example, that we find that the second ball was fixed to the table and could not move at all. This would justify us in saying that the law was not false, but that the cause could not operate because of the presence of a counteracting cause, the ball being fixed to the table.

All this may seem extremely trivial. In fact it is of fundamental importance. It has shown us that the law, as originally enunciated, was still *incomplete* in its formulation, that instead of saying that whenever one ball strikes another the second one will move, we intended to say that whenever one ball strikes another the second ball will move, *provided certain circumstances are present, and certain conditions fulfilled.* Of these further conditions and qualifications there are obviously a great number. They specify, first of all, the quality of the material used in the experiment: one ball must not be of iron, the other of paper; the impact must have a certain minimum force; the surface over which the balls move must be of such and such a kind. And further they exclude the possibility of counteracting causes: the ball must not be fixed to the table; it must not be acted upon by forces of a certain kind, exceeding a maximum amount, and so on.

Thus the inductive generalization, which we intended to formulate on the basis of what experience has taught us about the effect which the impact of one billiard-ball against another has produced in the past, is in fact far more complicated than its usual enunciation indicates. For practical purposes, however, it will generally be sufficient to state the law in the simplest formulation, perhaps with a very few qualifications, because the further additions to it are either concerned solely with exceptional circumstances, which very seldom need be taken into account, or they are such that the conditions laid down in them are so trivial and 'self-evident' that their fulfilment is taken for granted without special mention. Besides this, the leaving out of the additional qualifications seems to be merely a matter of convenience and quite harmless from an epistemological point of view. At the back of our minds we have the idea that although the law 'in practice' is left incomplete in its formulation, it is always 'theoretically' possible to formulate it in full, if needed. And, it is added, if really *all* relevant circumstances are taken into account, *then* the law certainly will hold for the case in question.[8]

This idea, however, ought to be more closely examined. Let us ask the following question: How would it be possible to know that *all* the conditions necessary for the formulation in full of the law have been taken into account? This is possible in more than one way. We may, for instance, decide after the enumeration of a certain definite number of conditions that *all* 'relevant' circumstances have been taken into account. If then the impact is not followed by the supposed effect, we must speak of a falsification of the inductive law. But it is not certain that this way would recommend itself as being plausible or in conformity with the actual practice of science. The decision as to when all relevant circumstances have been taken into account would always retain an air of arbitrariness, which in actual scientific procedure we wish to avoid.

Our attitude to the question as to the presence or not of all relevant circumstances in a given enumeration of conditions is therefore usually as follows: Perhaps all relevant circumstances have been enumerated, perhaps not; this is a question on which future experience will give us elucidation. It is conceivable moreover that, depending upon a number of circumstances of which several are themselves inductive experiences and assumptions, we regard it as highly desirable to make *the truth of the generalization* about the billiard-balls itself the *standard* for deciding when all conditions necessary for the validity of the experiment are fulfilled. In other words, as soon as the ball is struck, but left unmoved, we say that there must still exist some condition, 'relevant' to the truth of the law, which has not yet been taken into account, and which is absent in this case. Thus the truth of the induction serves as the norm which guides us in our search for new qualifications to be added for the purpose of getting a complete and exhaustive formulation of the law aimed at in making the generalization.

The above examples are meant to illustrate the two typical ways in which conventions may be introduced in inductive lines of thought. It must finally be observed that the two ways do not in general occur separately, but are usually *both* present in connection with *the same* induction. As will be remembered, we supposed in the example of the melting-point of phosphorus that all conditions as to the correctness of the experiment (apart from those as to the true criteria of phosphorus) were settled. Actually in settling these conditions we should be confronted with problems such as this: under what conditions should the measurement of the temperature take place in order to be correct? This question again resembles that of the presence or not of counteracting causes in the instance of the billiard-balls. Here also conventions similar to those in the last-mentioned instance may enter, and so the two ways are combined.

It is thus found that conventions play a fundamental role in investigations which rightly and truly are called inductive. Generalizations from experience get part of their strength and convincing power from the analytically binding force of conventions about the use of certain words and expressions. But what bearing has this upon the inductive problem as such?

§2. *Conventionalism as an 'elimination' of the inductive problem.*

The importance of conventionalistic or analytical steps of thought in inductive investigations is already implicitly contained in the doctrines of Mill and Whewell on induction. For example the role played by possible 'counteracting causes' in causal inductions has been noted by both the authors.[1] When Whewell constantly emphasizes that the process of induction has something to do with the formation of concepts and that with every scientific induction there is introduced a new idea, he moves along lines of thought which are not always very far from the ideas which were illustrated above in the example of the melting-point of phosphorus.[2] Bacon had mentioned induction as an operation by which concepts are defined.[3] Jevons[4] and Mach[5] stress the connection between induction and the classification of natural phenomena, and Sigwart[6] and Broad[7] give good examples of how induction is used for the formation of scientific concepts. Mach also pointed out a certain resemblance between the inductive procedure which results in the definition of a concept and the mathematical method called recursive induction.[8] Induction as a step in the formation of concepts is also related to the Aristotelian 'intuitive' induction.[9]

The first philosopher who clearly stated the *general* importance of conventions for the foundations of science and for inductive investigations was Poincaré.[10] He did not, however, use conventionalism for the purpose of offering a general theory of induction.[11] This has been done by certain other philosophers, who think that conventionalistic lines of thought when developed to a certain extreme would lead to an *elimination* of the inductive problem. The argument is roughly the following.[12]

The problem of induction, as put forward by Hume and as dealt with by most philosophers after his time, has its origin in a misconception of the nature of scientific truth.[13] It is an over-simplification, not in accordance with the real use of scientific propositions, to regard every generalization at which we arrive by induction as being purely synthetical. On the contrary, in so far as induction can claim to reach absolute truth it is because there has taken place a transition from synthetical to analytical, from well-established and well-confirmed empirical generalizations to linguistic conventions, which obtain their unrestricted validity from being analytical and tautologous. As this transition from synthetical to analytical is sometimes difficult to perceive, is very seldom explicitly formulated, and is often hidden in the ambiguous forms of language, we easily arrive at the mistaken idea of something which is at the same time synthetical and necessarily true.[14] From the attempt to reconcile these contradictory attributes of one and the same sentence originates the inductive problem in its 'classical' form. When we have seen that this attempt was undertaken on the basis of a misunderstanding the whole problem disappears, is eliminated. To justify induction is not to show how propositions which are synthetical can also be known to be true for unexamined instances, but to show how universal and necessary truth originates from synthetical propositions changing their nature into analytical.

There is much to be said for the view held by certain philosophers that the whole of science, even that part of it which is based upon induction, is not a system of general synthetical propositions, but of statements which are analytically true. When in this system a general proposition is discarded, it is not because it has been falsified, i.e. contradicted by an experiential proposition, but because enlarged experience has recommended the employment of some new mode of scientific expression.[15] Thus this view of scientific truth accounts also for the phenomenon of 'falsification' and need not presuppose a 'system' of science that has been rendered unchangeable once for all. And as it seems at the same time to eliminate the problem of induction, which occurs with the 'usual' view of the nature of scientific truth, it gets a further air of plausibility from this.

We shall call the view that general truth in science is always analytical, radical conventionalism. A typical representative of this view is Le Roy.[16] Ideas similar to those of Le Roy have in recent years been expounded by Ajdukiewicz.[17] Radical conventionalism, with explicit reference to induction has been developed by Schuppe,[18] Cornelius,[19] and Dingler.[20] The conventionalism of Dingler is peculiar in that he prescribes further conditions for the conventions which make up the bulk of exact science, as, for example, that geometry must be Euclidean and mechanics Newtonian.[21]

It is not our intention to discuss here the merits and demerits of the respective systems of conventionalism. We shall only show that it is not possible to eliminate the inductive problem as a whole (even in science) by taking a radically conventionalistic view of the system of natural laws.

§3. *Conventionalism and prediction.*

Suppose that we decide to regard every general proposition, established by induction, as true *per conventionem*, i.e. either as part of a definition or as an incomplete proposition, the completion of which is guided by its truth, or as a combination of both these cases. Then it would be possible to co-ordinate with these analytical propositions (one or more) synthetical general propositions after the following pattern:

Take the sentence 'the melting-point of phosphorus is 44° C' as enunciating a defining property of phosphorus. To this analytical proposition we thereupon co-ordinate the following general synthetical proposition: 'if a substance has all the defining properties of phosphorus, except perhaps the melting-point which is still unexamined, then the substance is phosphorus.' Or if, as certainly would be more in accordance with actual scientific practice, the defining properties of phosphorus are not explicitly enunciated: 'if a substance has such and such properties, which, irrespective of the further question whether they are defining properties or empirical ones, are regarded as reliable criteria of phosphorus, then the substance will melt at 44° C'. Again, in the case of the proposition concerning the billiard-balls, we co-ordinate the following synthetical sentence: 'if we know only that such and such conditions are fulfilled when we make the experiment, then the impact of one ball against another will be followed by the movement of the second one'.

The mere fact that we are able to do the co-ordination is, of course, trivial. But the further fact that, in a great number of cases, we use the co-ordinations to stress the reliability ascribed by us to certain inductive generalizations has a deep bearing upon the problem of induction.[1]

In order to understand this, consider the following alternative:

What if we were *never* justified in co-ordinating to the analytical propositions such synthetical propositions? This would imply that it would be impossible to make any reliable predictions in science since an analytic proposition in itself never justifies predictions. From the mere knowledge that *A*, *B*, and *C* define a substance it does not follow that we can regard, say, the presence of *A* and *B* as a reasonable basis for predicting the presence of *C*. Only if *A* and *B* are reliable 'signs' of *C* are we justified in co-ordinating to the definition the synthetical proposition that if a substance has the properties *A* and *B* then it has also the property *C*. Therefore, the fact that this coördination actually takes place in a great number of cases is nothing but an expression of the other fact that we regard reliable predictions as possible in science. This, by the way, is what makes science important and useful.

But here we are immediately confronted with a new question. How do we know that the synthetical propositions co-ordinated to the analytical ones really *are* reliable? (What has been said, above, implies that they are *regarded* as such, but this is of course no proof that they actually *are* so.) Or, in other words, how do we know that science can be used in future for predictions, not simply that it has, as a matter of fact, been possible to use it for this purpose?

These questions are nothing but the re-occurrence of the inductive problem within the conventionalistic conception of science itself. We cannot settle the questions by making the co-ordinated synthetical propositions themselves analytical.[2] For then we could again create new co-ordinations of synthetical propositions, and the inductive problem would only have been pushed to a new level. Nor can we dismiss the question by saying that, according to radical conventionalism, only analytical propositions are formulated as laws of science, and that consequently the above-mentioned co-ordinated synthetical propositions do not belong to the system of science. What matters is not whether we pretend that the co-ordinated propositions are laws of nature or not, or even whether they are formulated at all, but solely the *fact* that we regard certain predictions as reliable and act accordingly. This fact remains even within the system of radical conventionalism and its 'justification' constitutes the inductive problem.[3]

§4. *Conventionalism and the justification of induction.*

We have thus seen that conventionalism, even in its most radical form, does not eliminate the inductive problem. But, nevertheless, conventionalistic points of view contribute to a clarification of important aspects of the problem.

(1) We are now aware of the important fact that the general question about a 'justification of induction' covers not one, but (at least) two different things. One is this: how can we prove an inductive generalization to be true? The other is: how can we prove that such a generalization is a reliable basis for making predictions? There is a strong temptation to regard an answer to the former question as also an answer to the latter, and this tendency explains why we are liable to overlook the difference between the two aspects of the problem of how induction is to be justified. For if I have proved that it is true that all *A*'s are *B*, have I then not also proved that any prediction of *B* on the basis of *A* will be true?

As we know, there really is a means of proving that all *A*'s are

B, viz., by making the proposition true *per conventionem*. Thus it is possible to justify induction, *if* by 'justification' we mean only a proof of the absolute truth of the inductive proposition. But this 'justification' does not tell us anything as to the reliability of predictions. For if all *A*'s are *B per conventionem*, then to say that an *A* will be *B* is not to *predict* anything about *A*, but simply to state a tautologous fact about it. Therefore, if the first question contained in the problem of the justification of induction is answered by resorting to conventionalism, then the second question is still left open.

We have said that if it is true *per conventionem* that all *A*'s are *B* then there is no further question of 'predictions' of *B* on the basis of *A*. This, however, does not exclude the same verbal mode of expression 'all *A*'s are *B*' from being used in future for predicting *B* on the basis of properties of the *A*'s, *other* than *B*. But the reliability of those predictions is, of course, not to the slightest degree increased by the fact that the proposition 'all *A*'s are *B*' is made absolutely true *per conventionem*.[1]

(2) Those who emphasize that conventionalism eliminates the inductive problem seem to have been so impressed by the fact that it is possible to account for the absolute validity which we sometimes attribute to inductive generalizations, that they overlook the fact that it is not this alone which we had in mind when we demanded a justification of induction.[2] The general propositions, according to which predictions are made, still remain to be justified. But in the justification demanded for *them*, we seem to be content with something 'less' than absolute truth. Scarcely anybody would pretend that predictions, even when based upon the safest inductions, might not fail sometimes. We are satisfied in knowing that they are highly 'probable' at any rate. Thus conventionalism may be said to be able to dispose of the *element of absolute truth* contained in induction, and what then remains to be accounted for is the *element of probability* which is attached to the inductive predictions.

(3) The idea that conventionalism could eliminate the inductive problem, however, originates from more than a mere failure to see that to justify induction means not only to establish the truth of general propositions, but also to give rational grounds for the reliability of predictions. There is a deeper reason, why conventionalism seems to dispose of the inductive problem *as a whole*.

Language as it is actually used, both in everyday life and in science, is so constituted that in most cases it is not settled whether a given proposition is 'really' synthetical or analytical, nor which criteria of a certain object are defining criteria, and which again are empirical. For this reason it is not usually immediately clear whether a general proposition, as used by us, is 'really' used *qua* analytical, i.e. as a linguistic standard for interpreting facts, or *qua* synthetical, i.e. as a means of predicting experiences.

Therefore, if I have arrived by induction at a general law, I need not immediately decide as to its analytical or synthetical nature. I may for instance, to begin with, use it for the purpose of making predictions, and may regard every successful prediction as a confirmation of the law. Not until I am confronted with a situation in which a prediction fails to hold — and this may actually *never* happen — do I have to consider whether the law has been falsified or whether it is more plausible to 'save' its truth by 'explaining away' the failure of the prediction. As both alternatives are always *possible* I need never fear that experience will *compel*[3] me to withdraw the inductive generalization once established. It is always *in our power* to decide whether a law has been falsified or not, and this fact, which conventionalism reveals to us, explains the feeling of unshakable validity which we sometimes attach to those inductions which, on the other hand, are themselves 'confirmed' by successful predictions.[4]

(4) It must also be observed that if in the case of an apparent 'falsification' of a law, we decide to make the law analytical, this usually happens because there exists some empirical invariance or uniformity which, in spite of the exception to it in this case, is regarded as 'strong' enough to justify the introduction of the convention.[5] The convention, so to speak, serves the purpose of strengthening an already assumed empirical law; it adds absolute validity to something which is already in itself 'almost' absolutely true. Psychologically, therefore, the transition from the synthetical to the analytical which takes place when a convention is used to 'justify' induction may mean only a very slight increase in the *feeling of confidence* which we associate with the proposition, and this fact may obscure the fundamental change in the logical nature of the generalization which is introduced by the convention. So the inductive proposition, which has been transformed into an analytical

one, retains an empirical 'flavour' which gives to the conventionalistic decision an air of being concerned not only with the future use of words, but with future facts as well.

(5) There is a tendency, very much furthered by the introduction of exact symbols and notations, to regard the bulk of human knowledge as expressible in a definite set of propositions.[6] As was seen from the above discussion of the example of the billiard-balls, the idea underlying this tendency is to some extent delusive. It leads us to regard as pure knowledge about facts, knowledge which actually gets part of its firmness from verbal circumstances. The question of the justification of induction must be understood against the background of language, as an expression of knowledge, being not only ambiguous and 'unsettled', but also in a certain sense *inexhaustive*.[7] The importance of this inexhaustiveness of language for the problem of induction is revealed to us by the conventionalistic points of view from which the question was discussed above.

CHAPTER IV

INDUCTIVE LOGIC

§1. *Justification* a posteriori *of induction.*

In the previous chapters it was shown that the only way to guarantee *a priori* the truth of inductive generalizations is to make them analytical. And from this it followed that we cannot justify *a priori* predictions from inductive laws.

The idea of a justification *a priori* of induction was to make the proof of inductive propositions independent of the empirical testing of instances of those propositions. With this idea can be contrasted that of proving inductive truths with the aid of verified instances of the generalizations. The justification of induction is thus *a posteriori*.[1]

Of attempts to justify induction *a posteriori* there are two fundamentally different types.

Of the one type are the theories of induction which contrast the process leading from singular facts to inductive laws with the process of deducing those facts from the laws, the latter of these two 'inverse' processes being regarded as the *justification* of the 'inductive leap' made in the former. The logical element in induction is thus — deduction. The inductive philosophy of Whewell is the most noteworthy representative of this type. Of the other type are the attempts *to formalize the very process of generalizing from given data*, i.e. to make inductive propositions follow from singular instances according to given rules. Of this type is the theory of induction of Bacon and Mill.

Theories of the second type thus aim at the creation of a *logic of induction*, 'parallel' to the other main branch of formal study, viz. deductive logic. This, however, must not mislead us to the idea that the *logic* involved in inductive reasoning is, in any circumstances, of a different kind from the logic used in what is commonly known under the name of syllogistic or deductive reasoning. It is important to observe from the very beginning that the *logic* of all known attempts at a so-called logic of induction is exactly the same as the logic of that process of thought which is called deductive reasoning, even if this fact is sometimes obscured by a misleading terminology.[2]

In the following sections we shall examine the above two types of 'classical' attempts at an inductive logic. Here, as in the preceding two chapters, we are concerned only with a justification of induction leading to certainty. Considerations about the probability of propositions are still outside the sphere of our investigation.

§2. *Induction and discovery. Induction and deduction as inverse operations.*

According to a well-known definition induction is 'the operation of discovering and proving general propositions'.[1] In this definition the two fundamentally different aspects, viz., that of discovering a general proposition and that of proving it, are parallelized in a way which has been fatal for the philosophy of induction. A careful separation of them on the other hand contributes much to a clarification of the ideas about induction and its justification.

The problem of how to discover a generalization from a set of particular data is related to the question which, in the logic of Jevons, is called the inverse (or inductive) problem.[2] This problem, again, could be re-stated in the terminology of modern symbolic logic roughly as follows: Given a certain number of propositions, a, b, c . . ., construct a truth-function in the form of an equivalence which is true for certain given combinations, and for them only, of truth-values of those propositions. Assume for example that the propositions are a and b and that the equivalence is to be true for all possible combinations of truth-values of the propositions, except the case when a is true and b false. Then a truth-function fulfilling these conditions is $a \equiv a \& b$.

Given the truth-function, any of the propositions asserting one of the prescribed combinations of truth-values (in our example the propositions $a \& b$, $\sim a \& \sim b$, $\sim a \& b$) can be *deduced* from it. Given the latter propositions, the construction of the truth-function again becomes the *inverse* of this deduction. In the opinion of Jevons the sole way to perform this inverse operation was to 'guess' at the truth-function.[3] We *suppose* the truth-function fulfilling the conditions in question to be T; by deducing the particular data from T we *verify* (or falsify) this supposition.

The analogy between this and induction is obvious.[4] In the case of an induction we have also a set of particular data the 'law' for which we are in search of, i.e. a proposition from which these particular data are deducible. The invention of this law cannot usually be performed 'mechanically' but is the outcome of skilful guessing, guided by scientific 'intuition'.[5] As soon as any law, L, has been guessed at, we can test the result of the guessing by trying to deduce the given data from the law. If the deduction can be carried through, then the supposition that L is a law of the kind we look for has been verified.[6]

The process just outlined, by which we establish inductive propositions, is the typical process of discovery. It must be observed that this process does not accompany every case of induction. When from the fact that such and such A's are B, I infer that all A's are B, there does not occur any 'discovery', in the sense that I guess at a law from which the observed particulars are subsequently deduced. The law, so to speak, follows 'directly' from the given data.

On the other hand it is clear that such cases as the last-mentioned are of a rather 'primitive' kind, and that the many beautiful instances of induction which science affords us are generally peculiar on account of the element of discovery which they contain. This is the case in every instance of quantitative induction in which empirical measurement has provided us with a set of corresponding values of the variables, and we wish to detect a law or function for this correspondence. A sub-class of these instances again is that in which the values are pictured in a diagram and we look for a curve connecting them.

Kepler's discovery of the planetary path of Mars is a good example of quantitative induction of the last kind. Observation had informed him of the position of the planet in various points of its path, and from this information the path itself was to be induced. We know that Kepler, after having first rejected no less than nineteen assumptions as to the true path, discovered the law which agreed with the observations, i.e. from which the observed positions could be deduced.[7]

The process of discovery accompanying induction is of interest also from a psychological point of view. It introduces order and perspicuity in a multitude of previously disconnected facts, and it facilitates our handling of the given information. It concentrates, in the handy formulation of a law, a mass of knowledge which before had to be summed up in elaborate records of observations. Inductive discovery, in other words, is an important step towards the ideal 'economy of thought'.[8]

Induction as an operation inverse to deduction bears a certain resemblance to the well-known way of making geometrical constructions on given data, which is sometimes called the analytical method.[9] This method consists in that we suppose the problem to have been solved, and then deduce the given data from the solution. By tracing the thread of deductive steps in the opposite direction we thereupon carry out the construction.[10]

The idea that induction as a logical method is analogous to this way of reasoning in mathematics is very old. It is contained already in Zabarella's account of a logic of induction.[11] Galileo expounds the same idea in his account of the resolutive method which, according to him, described the way in which mathematical laws of nature are discovered. This method consists in a certain happening being resolved – analysed – into components, each one of which is supposed to obey some comparatively simple mathematical law. It is then shown that when the respective values of the components of the event in question are calculated separately under these suppositions and the components are then put together, their 'resultant' approximates to the actual happening. Galileo regarded the coincidence of the calculated and the actual course as the 'verification' of the discovered law.[12] The method is illustrated, for example, in his discovery of the mathematical paths of falling bodies. Ideas very similar to these on the nature of the scientific method are expressed by Leibniz.[13]

The inductive logic, or logic of discovery, expounded by Whewell must be understood wholly against the background of the above idea of induction and deduction as inverse operations and the analogy between induction and the analytical method in mathematics.[14]

According to Whewell the logic of induction is 'the analysis of doctrines inductively obtained into their constituent facts, and the arrangement of them in such a form that the conclusiveness of the induction may be distinctly seen'.[15] The 'Inductive Tables'[16] in which this function of the inductive logic is performed are tables giving a hierarchy of propositions, beginning with particular data and ascending from them to laws of greater and greater generality. Each ascending step is 'a leap which is out of reach of method',[17] i.e. the more general proposition is not deducible from the less general ones, whereas the descending steps form a chain of successive deductions. These deductions, says Whewell, 'are the criterion of inductive truth, in the same sense in which syllogistic demonstration is the criterion of necessary truth'.[18] The general propositions are thus *discovered* by induction and *proved* by deduction.[19]

Now the following question must be asked: Does this scheme of an inductive logic, foreshadowed in the description of the scientific method given by two of the greatest geniuses of European thought and developed to systematic strictness by Whewell, give a justification of induction?

The question ought not to be answered off-hand. The answer depends upon what we expect a justification of induction to be. It is not unplausible to assume that when, in advanced sciences, we ask for a 'justification' of the inductions made, we primarily have in mind a proof that the known data follow from the assumed laws. This may be the case because, as has already been said, inductions in advanced sciences, like astronomy and mathematical physics, do not usually follow from the data as a matter of course but are the products of 'discovery', i.e. of a sort of 'scientific guessing',[20] and must therefore afterwards be shown to fit the facts.

Snell detected the well-known law for the refraction of light, $\frac{\sin\alpha}{\sin\beta}=k$. Before one had this law one had to look for each pair of corresponding angles from tables. When Snell hit upon the law he certainly could not see at once that it really fitted each of the recorded pair of angles. The proof that each pair of the tables really followed from the law, was the justification of his discovery. Whether for this justification already recorded facts were used or whether the law was tested on new facts was in the first place a point of minor importance.

If, therefore, by a justification of induction we mean a proof that from the inductive proposition follow the data upon which the induction was made, then the scheme of an inductive logic outlined by Whewell provides us with the justification sought for.[21] The fact that in advanced sciences the justification of induction, in which we are primarily interested, may be a justification in this sense, has as a rule been overlooked by inductive logicians simply because they have chiefly confined their attention to those 'primitive' types of inductive inference where the general proposition follows as a matter of course from the given data.[22] Whewell was an exception to this rule, and that is why his philosophy of discovery partly gives a much better account of scientific induction than do other 'classical' treatments of the inductive problem.

But Whewell, on the other hand, overlooked the significance of another aspect of induction. This aspect, the importance of which again becomes more readily apparent if we confine our attention to those 'primitive' inductions where the element of discovery disappears, is that of *generalization*. It is stressed with great acuteness by Mill in his polemics against Whewell.[23]

If the discovery of the law from which given data follow is an induction, then it must be possible to deduce from it also data other than those already given.[24] Kepler's discovery that the available observation-points of the path of Mars were situated on an ellipse was, as such, no induction. It was made an induction by the further assumption that this ellipse would give to us the path of the planet also between the observed points, and make it possible for us to calculate Mars's position for any future time.[25] This further assumption which gives to the discovery its inductive character is a generalization.

But the truth of the generalization, involved in the discovery, cannot be proved in Whewell's scheme of an inductive logic.[26] And, consequently, if by the justification of induction we mean a proof that the general proposition to which induction has led us is true, then this kind of inductive logic does not justify induction.

We have therefore seen that the question whether Whewell had been able to justify induction or not is answered affirmatively or negatively depending upon what kind of 'justification' we look for. From the same considerations it also follows that the kindred question whether a law of nature can be verified or not has a twofold meaning, and therefore can be answered as well in the negative as in the affirmative. If by 'verification' we mean a proof that the supposed law really is a *law* for the given data, i.e. that the data can be deduced from it, then it is possible to verify it. But if 'verification' is to mean a proof that the law as a generalization is *true*, then we do not as yet know of a corresponding way of verifying it. Now we must not forget that actually the term 'verification' has been used by several philosophers and scientists so as to cover primarily the first case. We thus understand how they arrived at the opinion that laws of nature were verifiable. But it must be added that those philosophers as a rule overlooked that this kind of verification was not a verification of the law as a general proposition, but of the supposition that the law fitted the given facts. They therefore forgot that the problem of justifying induction has a further aspect which has not yet been dealt with.

If we consider why the inductive logic of Whewell does not justify induction in the sense of proving the truth of general propositions, we immediately detect that this is because, although the given data follow from the law, the converse of this, apparently, does not hold true, i.e. the law does not follow from the data. It is, therefore, the task of an inductive logic, which has to prove the truth of the inductive generalizations themselves, to show that under certain circumstances we are entitled to infer the truth of the law from the data. Can this task really be accomplished?

§3. *The idea and aim of induction by elimination.*

The idea that the process of generalizing from given data could be formalized, i.e. that the generalization could be inferred from the data according to fixed rules, seems to have its origin in the following observation:

We cannot establish the truth of an inductive generalization merely by collecting a huge number of instances confirming it. This kind of induction, *inductio per enumerationem simplicem*, '*puerile quiddam est*',[1] to use the words of Bacon, as it is a task constantly '*periculo ab instantia contradictoria exponitur*',[2] i.e. because the number of verifying instances as such cannot eliminate the possibility of a falsifying case. On the other hand it looks as though the examination of only a very few cases were sometimes sufficient for the establishment of an unshakable generalization. This evidently must be because the cases, besides being instances of the generalization, show some other characteristic features which are relevant to the validity and legitimacy of the inductive inference made from them. It is the business of an inductive logic to give a general scheme of these features which the given data must possess in order to serve as a valid basis for a generalization.[3]

The inductive method, described in the classical attempts to formalize the process of generalizing and in contrast with *enumerative induction*, we shall call *induction by elimination*.[4] It is our intention in this discussion to examine the eliminative method from the viewpoint of modern symbolic logic. This examination will not only enable us exactly to estimate the value of this method for the problem of justifying induction, but will also lead to interesting discoveries about the logical nature of eliminative induction in general. These discoveries, we think, are a testimony of the value of logistics when used as a means of analysing and re-interpreting 'classical' doctrines and ideas on philosophical questions.

The task of induction, if we confine our attention only to Universal Inductions about one-place predicates,[5] could be described as that of connecting two characteristics (properties, predicates), A and B, by universal implication (or universal equivalence).

This task, first of all, presents two different aspects, according to whether the characteristics are properties of the same individual (object, event)[6] or whether they belong to different individuals. In the former case the general implication which we want to establish is of the form:

$$(1) \qquad (x)\ [Ax \rightarrow Bx],$$

in the second case again of the form:

$$(2) \qquad (x)\ (y)\ [F\ (x, y) \rightarrow (Ax \rightarrow By)],$$

where F is a function, determining the pairs of corresponding x's and y's.[7]

If in (2) the function F correlates the x's and y's as individuals succeeding each other in time, we shall call the generalization a Causal Law. Universal implications and equivalences of the form (1) and those of the form (2), which are not Causal Laws, might in accordance with a classical terminology be called Uniformities of Co-existence. The most conspicuous difference between Bacon's and Mill's respective systems of inductive logic is that the former treats of Uniformities of Co-existence, the latter causal uniformities.

But the task of connecting two characteristics by universal implication also presents two different aspects in another sense. Either I take one of the characteristics as 'given' and look for another characteristic, which it universally *implies*. Or, I look for a characteristic by which the 'given' one is itself universally *implied*. The former case is that of finding a *necessary condition* of a given characteristic, the second that of finding a *sufficient condition*. The connecting of two characteristics by universal equivalence is again the establishing of a necessary *and* sufficient condition of a given property.[8] We shall, therefore, call the characteristic, which is 'given', the *conditioned* one, and the characteristic we are looking for, the *conditioning* one.[9]

Now the idea of eliminative induction could be described shortly as follows: Suppose I look for a conditioning property of a given conditioned property, say A. As a rule there will be a number of *concurrent hypotheses* as to this conditioning property. Is it the characteristic B or C or D or any other which is connected with A by universal implication? This question, obviously, we cannot answer by collecting a great number of instances confirming one or other of these possible general implications, i.e. by resorting to enumerative induction. Because if we do so, then we can never, in spite of the confirmations, eliminate the possibility that not the confirmed hypothesis but some other is the true one. On the other hand we know that we need only find one single instance where one of these possible general implications does not hold in order to *invalidate* and consequently also *eliminate* one of the concurrent hypotheses as to the necessary or sufficient condition of which we are in search. It is this important *asymmetry* in the possibilities of verifying and falsifying Universal Generalizations which is at the root of the idea of an inductive method, proceeding from elimination of concurrent hypotheses.

Francis Bacon, who gave the first substantially correct description of eliminative induction, saw the advantage of the eliminative method over enumerative induction in that the former was a method enabling us to reach *absolute certainty*.[10] This association between absolute certainty and induction by elimination is typical also of most later treatments of that method. For a critical examination of the idea that eliminative induction could attain absolute certainty it is, however, important to observe that the word 'certainty' in this connection may mean no less than three different things. The failure to separate these different meanings from one another has been the cause of much confusion and misunderstanding.

In the first place absolute certainty may mean that under certain circumstances we can prove a general proposition to be the *only* generalization which is in accordance with certain data.

In the second place absolute certainty may mean that the inductive method provides us with the *premisses* from which the generalizations themselves can be deduced according to logical rules. From this it does not follow that the generalizations, reached by the eliminative method, must be *true*. The eliminative method would take us to this conclusion only if we had the additional knowledge that the premisses which this method provides are themselves true. That the conditions for this conclusion are fulfilled is the third meaning of the phrase that eliminative induction leads to absolute certainty.

It would not be inappropriate to say of the eliminative method that it *justifies* induction if it could reach certainty in *any* of these three senses. But it is also clear that only if it does so in the third sense, do we get that kind of justification in which we are primarily interested here, viz. that which excludes the occurrence of a contradictory instance to the law established by induction. Bacon explicitly attributed this power to the eliminative method when he said that it 'ex aliquibus generaliter concludat ita ut instantiam contradictoriam inveniri non posse demonstretur'.[11]

It follows from the logical nature of the eliminative method, as described above, that elimination as such only informs us of the *falsehood* of certain hypotheses. From this information in itself nothing can ever be concluded as to the (conditional or unconditional) *truth* of some not-eliminated hypothesis. Pure elimination, therefore, at least cannot attain certainty in the second and third of the above senses.

It is, however, not certain *a priori* that elimination in itself can attain even certainty in the first sense. For this it is a minimum requirement that it is logically possible, by elimination alone, to invalidate all concurrent hypotheses *except one* as to a necessary or sufficient condition of a given characteristic. It will be our next task to inquire whether the logical mechanism of elimination really can achieve this last aim without the aid of some general postulates about the nature of the universe.

§4. *The mechanism of elimination.*

Necessary Conditions. We begin with a description of how the logical mechanism of elimination works when we are looking for a necessary condition of a given characteristic A.

By a *positive instance* of the conditioned property A we mean any individual x, of which this property can be truly predicated.

We consider a (finite) set $x, y, z, \ldots$ of positive instances of the conditioned property. To the individual x there answers a set of properties $X_1, X_2 \ldots$ (other than A), which can be truly predicated of this individual.[1] Similarly, there is a set $Y_1, Y_2 \ldots$ of properties of y, a set $Z_1, Z_2 \ldots$ of properties of z, etc. The properties in each set are assumed to be *logically independent* of each other. This means that no one of the properties is such that its presence in the individual logically follows from the presence of some (or all) of the other properties in the individual.[2]

These sets of properties we call *basic sets of initially possible necessary conditions of A*.

By the *positive analogy* between a number of sets of properties we mean the (set of) properties, which are common members of *all* the sets. The (set of) properties again which are members of *some but not all* the sets of properties are said to constitute the *negative analogy* between the sets.[3]

It is clear that, if a property P belongs to the negative analogy between the basic sets of initially possible necessary conditions of A, then it cannot (actually) be a necessary condition of A. For, from the definition of the negative analogy it follows that there exists at least one positive instance of the conditioned property A, in which P is lacking. And anything which is absent in the presence of A cannot be a necessary condition of A. Or, in other words, if a property belongs to the negative analogy between the basic sets, then the supposition that it would be universally implied by the conditioned property A is *invalidated* and consequently also *eliminated* from the class of alternative hypotheses as to the necessary condition of A.

It follows from this that each *increase* in the negative analogy mentioned, and conversely each *decrease* in the corresponding positive analogy, effects the elimination of some possible hypothesis as to the necessary condition of the conditioned property A. Therefore, the object of applying the eliminative method of induction to the search for a necessary condition of A is to narrow, as far as can be done, the positive analogy between some basic sets of initially possible necessary conditions of the property. This is done by adding new positive instances to those already considered, which *differ* from the latter in as many properties as possible. Or to use a classical terminology, it is done by 'varying the circumstances' under which A occurs. According to the way in which this variation is affected, we say that we use eliminative induction as a method of *observation* or as a method of *experimentation.*

It is conceivable that the positive analogy between the basic sets might finally consist of one property only. This, of course, does not mean that use of eliminative induction will *actually,* sooner or later, in every case lead to a situation in which all properties save one are eliminated from the positive analogy. If we are left with more than one property in the positive analogy, this may be either because we have not yet 'varied the circumstances' to the utmost, *or* because the conditioned property actually has *more than one* necessary condition among the members of the basic sets. The latter alternative is known from traditional logic of induction under the name of 'Plurality of Causes'. It is to be observed that there is no answer to the question, which of these two alternatives is true, other than the answer which future experience alone can give after continued recourse to eliminative induction.

Let us, however, suppose that we actually were left with only one property in the positive analogy. From this fact it cannot be concluded that this remaining property is also the only remaining possible necessary condition of A — not even in the 'realm', so to say, of the properties in the basic sets. For, we have still to consider the possibility known by the name of 'Complexity of Causes',[4] i.e. the possibility that, although no single member of the basic set is a necessary condition of A, a *disjunction* of two (or more) such members is a necessary condition of A.[5]

That the disjunction of two properties, say B and C, is a necessary condition of A means that whenever A is present, then B *or* C is present. In symbols: $(x)\ [Ax \rightarrow Bx \vee Cx]$. Necessary conditions of the disjunctive form are no mere 'theoretical possibilities' but familiar from the practice of science.[6] Thus, e.g., in order to bring about a variation in the volume of a gas, it is necessary either to vary the pressure, to which the gas is subject, *or* to vary its temperature. Variation in pressure *or* temperature is thus a necessary condition of variation in volume.

It might here be suggested that the eliminative method should be applied also to the properties which can be constructed by forming all the possible disjunctions of logically independent properties which are members of some of the basic sets.

Actually, an application of the eliminative method to such 'disjunction-properties' is possible.[7] But the resort to elimination is here subject to an important limitation in its logical powers. This is seen from the following considerations:

It is clear that if a property P is a necessary condition of A, then the disjunction of P and *any* property is also a necessary condition of A. For, that P is a necessary condition of A means that in all positive instances of A the property P is present, and of all instances where P is present it is trivially true that P *or* P' is present.

Consequently, if elimination has left us with the property P as a possible necessary condition of A, then it has also left us with all disjunctions of properties, containing P as a constituent, as such possible conditions.

From the above it follows that *if* all the 'disjunction-properties' mentioned with the exception of one have been eliminated, i.e. excluded from the possibility of being necessary conditions of A, then this one remaining property must necessarily be the disjunction of as many logically independent properties as there are in all the basic sets considered together.

The relevance of this peculiarity of the eliminative method, when applied to possible complex conditions, to that method's power of attaining certainty will be estimated later.

Sufficient Conditions. We proceed next to an examination of the mechanism of elimination in cases where we are looking for a sufficient condition of a given property A.

Sufficient and necessary conditions are interdefinable. If the presence of A is sufficient for the presence of B, then the absence of A is necessary for the absence of B, and *vice versa*. In symbols: $(x)\ [Ax \rightarrow Bx] \equiv (x)[\sim Bx \rightarrow \sim Ax]$. If oxygen is a necessary condition of life, then the absence of oxygen is sufficient to extinguish life, and conversely.

It follows from this that to ascertain the sufficient conditions of a given property A is equivalent to the task of ascertaining the necessary conditions of the property $\sim A$ (not-A). And this means that the same method of elimination as was described above for necessary conditions can be applied ('negatively' or 'inversely', so to speak) to the search of sufficient conditions.[8]

There is, however, a 'typical' case of the search of sufficient conditions, to which a different canon of elimination is applicable. In this case we are interested, not in the sufficient conditions 'as such' of a given property, but in the sufficient conditions of the property among the properties of a given positive instance of it.[9]

In this case we compare a given positive instance x of the conditioned property A with a (finite) *set of negative instances* $y, z, \ldots$ of A. (By a negative instance of A we mean any individual, of which A can be truly denied.)

To the positive instance x there answers a set of logically independent properties $X_1, X_2 \ldots$ which can be truly predicated of x.[10] We call it *a basic set of possible sufficient conditions* of A.

To the negative instances $y, z \ldots$ also answer sets $Y_1, Y_2 \ldots$ and $Z_1, Z_2 \ldots$ etc. of properties which can be *truly* predicated of the individuals. We call these sets of properties *basic sets of not-possible sufficient conditions* of A. (It is clear that any property which occurs in a negative instance of A cannot be a sufficient condition of A.)

We form *the logical sum* of the basic sets of not-possible sufficient conditions of A. That is to say: we form a set consisting of each property that occurs in *at least one* of the above sets of not-possible sufficient conditions.

With this sum-set, thus formed, we compare the one basic set of possible sufficient conditions of A. It is obvious that if a property belongs to the positive analogy between these two sets, then it is eliminated as a possible sufficient condition of A. For, from the fact that it belongs to this analogy, it follows *per definitionem* that at least one instance exists in which the property in question is present but in which A is lacking. The property mentioned, in other words, though present with A in x, cannot *universally imply* A.

For this reason each *increase* in the positive analogy mentioned entails the elimination of some alternative hypothesis as to the sufficient condition of A among properties of x. It is the purpose of the eliminative method to increase this positive analogy to the utmost. This is done by taking for examination new negative instances which *agree* in as many properties as possible with the one positive instance x of A which we have examined.

Obviously, the elimination of all properties *except one* from the basic set of possible sufficient conditions, is logically possible. But here, as in the case where we looked for a necessary condition, we can never *a priori* exclude 'Plurality of Causes', i.e. the possibility that A has not one but several sufficient conditions among members of this basic set.

Suppose, however, that we had actually eliminated all properties except one from the basic set. Then the question of possible 'Complexity of Causes' arises. This means that, although no single member of the basic set of possible sufficient conditions of A is actually such a condition, nevertheless a *conjunction* of two (or more) such members is a sufficient condition of A.[11]

That the conjunction of two properties, say B and C, is a sufficient condition of A means that whenever B *and* C are both present (but not necessarily when one of them is present), then A is present too. In symbols: $(x)\ [Bx\&Cx\rightarrow Ax]$. Sufficient conditions of a conjunctive form are certainly a commonplace in the practice of science.[12] Their importance to the logical study of induction was (vaguely) recognized by Mill.[13]

It is possible to apply the same canon of elimination also to the properties which are constructed by forming all the possible conjunctions of any two, three, etc. members of the basic set of possible sufficient conditions.[14] But the use of elimination among such 'conjunction-properties' is subject to a limitation, analogous to the one described above for necessary conditions.

It is clear that if a property P is a sufficient condition of A, then the conjunction of P and *any* property is also a sufficient condition of A. For, that P is a sufficient condition of A means that in all positive instances of P A is present, and of all instances where P *and* P' are present it is trivially true that they are positive instances of P. Thus, if elimination has left us with the property P as a possible sufficient condition of A, then it has also left us with all conjunctions of properties, of which P is a constituent, as such possible conditions.

From the above it follows that *if* all the 'conjunction-properties' mentioned with the exception of one have been eliminated, i.e. excluded from the possibility of being sufficient conditions of A, then this one remaining possibility will necessarily be the conjunction of *all* the properties in the basic set of initially possible sufficient conditions of A.

Our description of induction by elimination, when used for the purpose of ascertaining necessary conditions, roughly answers to Mill's Method of Agreement, and our description of the eliminative method, when used for the purpose of ascertaining sufficient conditions of a given property in a given positive instance of it, roughly corresponds to Mill's Method of Difference. For the purpose of ascertaining necessary-and-sufficient conditions canons of elimination may be used which roughly answer to Mill's Joint Method.[15]

Throughout the above discussion we have assumed that the generalization or law, of which we are in search, is of the type [1] of the preceding Section, i.e. of the type that the conditioned and the conditioning properties are attributes of the same individual. The description of the mechanism of elimination can without difficulty be extended so as to apply to generalizations of the type [2] as well. We have only to let the basic set of properties answer, not to (positive or negative) instances of the conditioned property itself, but to instances correlated through some relation F to instances of the conditioned property. This modification in the determination of the basic sets is altogether inessential to the way in which the eliminative mechanism functions.[16]

Conclusions. It was mentioned at the end of Section 3 that if elimination, as such, without the aid of any general postulates or assumptions as to the nature of the universe, were to attain certainty in any of the various senses in which 'certainty' has been attributed to this method of induction, then it should be logically possible to effect, in any given case, the elimination of all alternative hypotheses *except one* as to a necessary or a sufficient condition of a characteristic. Examination has taught us that the two sub-methods of induction by elimination, roughly corresponding to the classical methods of Agreement and Difference, *are both equally efficacious in the attainment of this aim.* And to both of them applies the following limitation in their power of reaching this aim:

Although it is logically possible to achieve an elimination of all initially possible conditions except one, relative to given basic sets of logically independent properties, this 'ideal' elimination must always lead to the result that the only remaining possible condition is of 'maximal complexity', i.e. either a disjunction or a conjunction of as many properties as there are logically independent members in the basic sets of initially possible conditions. This important restriction on the 'direction' of the eliminative process makes pure elimination less valuable, as a means of finding the only possible true hypothesis as to a necessary or sufficient condition of a given characteristic, than it might seem at first sight to be. It would have been more valuable, if the 'direction' of elimination had been 'free', i.e. if it had been possible to eliminate from the class of initially possible rival hypotheses all but one, without it being possible to know beforehand (on purely logical grounds) which degree of complexity must characterize that hypothesis which will finally be the only remaining one.[17]

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Let us illustrate the limitations in the power of pure elimination by an example. This example will to some extent go beyond the basis of the above strictly formal considerations, but might be useful in assessing the value and epistemological significance of the classical ideas about eliminative induction.

As an example of how the Method of Difference works the following is sometimes mentioned:[18]

We observe a yellow band at a characteristic place in the spectrum of a spirit-flame containing sodium. We wish to establish a 'causal relationship', i.e. a relationship of universal implication, between the yellow band in the spectrum and the presence of sodium in the flame. To this end we remove the sodium from the flame *leaving all other circumstances unchanged.* If then, together with this removal of sodium from the flame, the yellow band in the spectrum also vanishes, we feel inclined to assert that the presence of sodium in the flame was the 'cause' of the yellow band in the spectrum. This assertion is based on the following argument:

Since the yellow band did not occur when the flame did not contain sodium, it cannot be universally implied by any circumstance present in this case. But since, on the other hand, this second case differed *only* in the absence of sodium from that case where the yellow band appeared, we conclude that, if there is any cause at all for the occurrence of the yellow band, then this cause must be the presence of sodium in the flame.

Already Mill has observed that an argument of this type is inconclusive in the following respect:[19]

Even if it is assumed that the yellow band has a cause, we cannot from the experiment conclude that this cause will be sodium alone, since the possibility remains that it were sodium *and* some other substance — and neither of them alone — which universally implied the occurrence of the yellow band in the spectrum. In other words, the possibility of a 'complex cause' remains. We can only, strictly speaking, conclude that, if there is any cause at all for the occurrence of the yellow band, then sodium is at least *part* of this cause, that is to say in our terminology, that if the condition is complex then it consists of a conjunction of characteristics, one of which is sodium.

Mill and later authors, however, have not rightly estimated the significance of this inconclusiveness in the argument. Actually this inconclusiveness implies that we cannot draw any *general conclusion* at all from the above described experiment.

It is important to point this truth out, since there appears to be a strong inclination to overlook it for the following reason: We admit, following Mill, that we cannot conclude from the experiment that always when sodium is present in the flame there will appear a yellow band in the spectrum, since the cause may be 'complex'. But since, on the other hand, this possible complexity in the cause means that sodium is *at least part of the cause* of the yellow band's occurrence, it seems that we could conclude conversely that always when there is a yellow band at the characteristic place in the spectrum, then there must be (at least) sodium present in the flame. This would amount to asserting that, although we cannot from the experiment conclude that sodium is a sufficient condition of the yellow band's occurrence, we can nevertheless conclude that it is (at least) a necessary condition of it.

The suggested conclusion, however, it must be observed, is entirely unjustified. The method of ascertaining necessary conditions by elimination is, as was shown above, a method which can be roughly identified with the classical Method of Agreement. The experiment described above has not, however, the slightest relevance to the possible result of applying this method in ascertaining a necessary condition of the yellow band in the spectrum of the spirit-flame. It will also be immediately clear on reflexion that there is nothing in the experiment mentioned to exclude the existence of the characteristic yellow band in the spectrum *in spite of the fact* that there were no sodium in the spirit-flame.

Thus we can, on the basis of the experiment described above, neither conclude universally from the presence or absence of such and such substances in the spirit-flame the presence or absence of such and such spectral phenomena, nor conversely from the presence or absence of anything in the spectrum the presence or absence of any substance in the flame. In other words, there is no universal implication whatever to be concluded from the experiment. This important truth is clearly revealed to us when the theory of the classical inductive methods is treated as a theory of necessary and sufficient conditions.

Although the value of pure elimination as a means of attaining certainty is limited, the description of the logical mechanism of elimination is of importance as being the exact and formalized expression of age-old ideas on the way in which truly 'scientific'[20] induction proceeds, particularly when employed for experimentation. The nature of elimination makes it clear why we sometimes consider the examination of a single instance, when pursued carefully and with a certain methodical aim, to contribute very much more to the weight of a *general* proposition than does the verification, regardless of further circumstances, of even an enormous multitude of instances confirming it.[21] The method of ascertaining necessary conditions, in addition, is the exact expression of the rule, which has always been regarded as one of the leading maxims in the practice of science, viz. that the true test of a scientific law does not lie in the number of confirming instances as such, but in the multitude and variety of *different* conditions under which the testing has taken place.[22]

§5. *Remarks about the comparative value of the methods of Agreement and Difference.*

The result that the Method of Agreement is, for the purpose of elimination, not less 'effective' than the Method of Difference might appear highly surprising when we consider the value attributed to the latter method in the inductive logic of Mill.[1] As is well known Mill, and following him most later authors on the subject,[2] regarded the Method of Difference as being the only method by means of which we can reach absolutely certain conclusions as to possible 'causes',[3] and as being much superior for this and other reasons to the method of Agreement.[4]

In order to make clear to what extent our results as to the comparative value of the two methods conflict with those of Mill we have to make the reader conscious of certain peculiarities in Mill's description of the inductive methods, peculiarities which are partly the offspring of serious mistakes and obscurities.

In the logic of Mill the word 'cause' means *sufficient condition*[5] (in time). For this reason the Method of Agreement, as described by Mill, can be used as *a method of elimination* solely for the purpose of looking for the effect of a given cause and *not* for the cause of a given effect. This, however, was overlooked by Mill.[6] After having given a substantially correct description of how the method works by elimination for the detection of the sole possible effect of a given cause[7] he continues:[8]

'In a similar manner we may inquire into the cause of a given effect. Let *a* be the effect. . . . If we can observe *a* in two different combinations, *a b c* and *a d e*; and if we know, or can discover, that the antecedent circumstances in these cases respectively were *A B C* and *A D E*; we may conclude *by a reasoning similar to that in the preceding example*,[9] that *A* is the antecedent connected with the consequent *a* by a law of causation. *B* and *C*, we may say, cannot be causes of *a*, since on its second occurrence they were not present; nor are *D* and *E*, for they were not present on its first occurrence. *A*, alone of the five circumstances, was found among the antecedents of *a* in both instances.'

Here Mill obviously has failed to see that the fact that *B*, *C*, *D* and *E* belong to the negative analogy between the two cases has no bearing whatever upon the question of finding a sufficient condition, i.e. a 'cause' in Mill's sense, for *a*. Actually none of the characteristics has been *eliminated* as a possible sufficient conditon of *A*; all we have achieved with the two instances is that the hypotheses that *B*, *C*, *D* or *E* respectively is a sufficient condition of *a* have been confirmed *once*, and the hypothesis that *A* is such a condition has been confirmed *twice*. *Thus Mill's Method of Agreement, when applied to the task of finding sufficient conditions is not an eliminative method at all but simply a kind of inductio per enumerationem simplicem.*

This important truth never became clear to Mill. Misled by the fact that the increase in the negative analogy, when the Method of Agreement is employed, actually brings about an elimination (of possible necessary conditions) he believed this method also to be one of *elimination* when used in the search for causes of given effects, i.e. of sufficient conditions. On the other hand the fact — although not clearly grasped by Mill — that the method, when applied to sufficient conditions, was *not* one of elimination but one of enumeration, drove him to the reservation against the Method of Agreement, which he expressed by saying that this method can prove a characteristic to be an *invariable*, but not an *unconditional* antecedent of a given characteristic.[10] By this very confused and gravely misleading formulation Mill simply wanted to express that the Method of Agreement cannot prove the characteristic *A* to be a sufficient condition of *a*, even if *A* and *a* are the only properties common to all the examined instances.[11]

Again, with the aid of the Method of Difference, we might prove that if *a* has any sufficient condition in the cases in question, then this condition cannot be any other than *A* or some condition more complex than *A*, i.e. some condition *part of which is A*.

Thus we can understand how Mill, from the false idea that the Method of Agreement as well as the Method of Difference were an eliminative method when used in the search for sufficient conditions together with the true insight that the Method of Difference, but not that of Agreement, can under certain circumstances prove one characteristic to be in a given case the only possible sufficient condition or part of such sufficient condition of another characteristic, arrived at the further idea that the Method of Difference was 'superior' to the Method of Agreement in the search for 'causes', i.e. sufficient conditions. The absurdity of this comparison becomes apparent so soon as we realize that the Method of Difference alone is an eliminative method when applied to sufficient conditions.

Apart from this fundamental misconception of the eliminative power of the Method of Agreement there is another reason which caused Mill to regard the Method of Difference as 'superior' to that of Agreement. This reason consists in his always assuming, in describing how the Method of Difference works, that the first instance lacking the conditioned property agreed with the instance having the conditioned property in all other characteristics except a single one, or else that only *two* instances were needed in this method, whereas in the method of Agreement we need perhaps an unlimited number of instances.[12] Such an assumption was intelligible from the use we actually make of the Method of Difference as a method of *scientific experimentation.*[13] But 'logically' we are equally justified in making a corresponding assumption in the Method of Agreement, *when used as a method of elimination*, viz. that the second positive instance of the conditioned property differs from the first instance in all its properties except a single one. (This assumption might also, as in the case of the other method, be 'practically' justified when the Method of Agreement is used as an experimental method.[14]) Mill was prevented from grasping this probably for the simple reason that he did not realize that the Method of Agreement is an eliminative method when applied to necessary conditions only.

§6. *The general postulates of induction by elimination.*

In ascertaining the possibilities of eliminative induction to attain certainty we must distinguish from each other the following two questions:

(1) Is it logically possible, by elimination alone, to invalidate all concurrent hypotheses *except one* as to a necessary or sufficient condition?

(2) Is it possible to determine, in a given case, whether all concurrent hypotheses *except one* have been eliminated?

It was stated above that an affirmative answer to the first question was *necessary* if eliminative induction was to attain certainty in any of the three previously defined senses.

It is, however, to be observed that this affirmative answer, although *necessary*, is not *sufficient* for the attainment of certainty even in the first of the three senses mentioned. To this end, evidently, it must be possible to answer also the second of the above questions in the affirmative.

It is immediately clear that if the number of independent possible conditioning properties of a given conditioned property were infinite, then it would never be possible to determine whether, in a given situation, all concurrent hypotheses *except one* have been eliminated. For, under such circumstances, we could never know for certain whether, in the basic sets, the positive and negative analogies of which it is the business of eliminative induction to increase and decrease respectively, all relevant properties have been included or whether some property has not, for one reason or another, escaped notice or been neglected.

It is, however, not possible for reasons of logic alone to exclude the possibility that the number of concurrent hypotheses as to a necessary or sufficient condition are infinite. The exclusion of this possibility can be effected only by the introduction of some general assumption as to the constitution of the universe.

Now it has been suggested that the assumption necessary on this point, in order to secure the possibilities of eliminative induction of attaining certainty, were that the number of logically independent properties of any individual are finite in number.[1] This assumption we shall call the Postulate of Limited Independent Variety. It can be said to be the basic supposition of the inductive logic of Bacon.[2] Recently a developed form of the Baconian postulate has been advocated by Keynes.[3] There appears, furthermore, to be some *prima facie* presumption in favour of the truth of this postulate.[4]

It is, however, extremely important to observe that the assumption of a finite number of properties of any individual is not sufficient for the purpose of knowing when all concurrent hypotheses except one have been eliminated. Suppose that we knew the number of properties to be finite. Then, even if we have taken into account, in pursuing the elimination, any finite number n of concurrent hypotheses as to a necessary or sufficient condition, we could never be sure that there does not exist at least one property which so far has not been reckoned with, and which therefore may represent also a true hypothesis as to the condition in question. For from the mere knowledge that the number of properties of an individual is finite, it does not follow that the number of properties (and consequently also of concurrent hypotheses), which in a given situation we ought to take into account, is not greater than the (always finite) number of properties, or hypotheses, which *actually* has been considered.[5]

Consequently, in order to make possible knowledge as to whether in a given situation all concurrent hypotheses except one have been eliminated, we have to introduce some assumption 'stronger' than that of Limited Variety. This new assumption must, generally speaking, assert that, under certain circumstances, it is possible to know when we possess *complete knowledge* of all properties of the examined instances which are to be taken into account for the purpose of elimination of concurrent hypotheses. This postulate of the logic of induction which is to replace the postulate of Limited Independent Variety we shall call that of Completely Known Instances.[6]

For a more detailed formulation of the postulate of Completely Known Instances two principal ways are left open. One of them might be described as a continuation of the way leading to the introduction of the postulate of Limited Variety. It would consist in our introducing some more precise assumption as to the number of possible properties of an individual than simply that it is finite. A sort of 'minimum assumption' in this direction would be to suppose the number of possible properties never to be greater than a fixed number n. Under this assumption it would be possible at least in those cases where we have in each examined instance discovered exactly n properties, to know for certain when all concurrent hypotheses except one as to a necessary or sufficient condition have been eliminated.

On the other hand it seems arbitrary, if not absurd, to assume *a priori*, of the number of possible properties of an individual, that it cannot supersede a *fixed* number n. This way of giving the postulate of Completely Known Instances a more specific formulation must therefore be regarded as extremely unplausible and unsatisfactory.

The second way of specifying this postulate is the following: We make no assumption as to the *number* of possible hypotheses or possible properties of an individual, not even that it must be finite. We assume instead that certain categories of simple properties can be left out of consideration as being *irrelevant* to the eliminative method of induction, and that in each single case we are able to judge whether the information about the instances, which has been taken into account, represents complete knowledge as to all the remaining relevant properties or not. If it does then we can *enumerate* the relevant properties of each individual and hence also determine whether or not, after the examination of a number of instances, the eliminative process has reached the elimination of all concurrent hypotheses *except one*.

The defects also of this formulation of the postulate will be obvious to anybody. How is it possible, it will be asked, to define what is 'relevant' in this connection in any way other than that the definition involves a reference to the eliminative method itself and hence is circular?[7] But apart from this obvious defect it cannot be denied that such a specification of the postulate has *some* plausibility in itself.

First there is, so to speak, a 'practical plausibility' in favour of it, consisting in that we are in any given case usually 'practically sure' about which hypotheses might conceivably be true for a given phenomenon and which again can at once be dismissed as being irrelevant. The number of the first hypotheses, furthermore, is as a rule not very great.

This 'practical plausibility' of the postulate, incidentally, is a fact of the greatest psychological significance for the possibilities of the human mind to detect law and order in the multitude of phenomena with which it is confronted. If it were not possible for us to confine our attention to a fairly small sector of 'relevant' circumstances in the multitude of given data, we should have seldom succeeded in detecting the uniformities and laws of nature of which we actually possess knowledge.

It must also be noted that there seems to be at least one class of 'properties'[8] which can *generally* be excluded as being 'irrelevant'. This class consists of the characteristics which state the spatio-temporal position of the instances. It is an old idea that the validity of natural laws cannot be restricted by time and space *as such*, but that if a law is not valid under spatio-temporal conditions differing from those under which it has been detected and confirmed, then this is due to some difference in circumstances, *other* than spatio-temporal.[9] This idea seems extremely plausible, and one is tempted to say that its plausibility, which almost equals self-evidence, is founded not upon matters of fact, but upon some *a priori* grounds.[10]

So much for and against the postulate of Completely Known Instances as specified above. Irrespective of whether this postulate can be upheld with some plausibility or not, we have to observe that it is absolutely necessary if the second of the above two questions is to be answered affirmatively, i.e. if it is to be possible to determine, in a given case, whether all concurrent hypotheses as to the necessary or sufficient condition of a given characteristic have been eliminated.

Thus only under the Postulate of Completely Known Instances can induction by elimination reach certainty as to which one of a number of concurrent hypotheses is the *only* generalization fitting all the known data. Actually there are passages in the writings of Bacon,[11] Mill,[12] and other authors,[13] from which one gets the impression that it was just this kind of *certainty* that they deemed the eliminative method capable of achieving. The failure to distinguish different senses of the term 'certainty' from each other has, however, caused those authors to make apparently contradictory statements in other places.[14]

It will be useful here to introduce the following definition:

By *the data of the elimination* in a case when the eliminative method is applied we mean all (singular) propositions stating that such and such individuals, examined for the purpose of the method, possess such and such properties.

It is evident that the above postulate of Completely Known Instances is not sufficient if it is to be possible from *the data of the elimination* to *deduce* general propositions, that is to say, if eliminative induction is to reach certainty also in the *second* of the three senses mentioned. For even if the result of actual elimination together with the above postulate had informed us that the only characteristic which can possibly be connected by general implication with the given characteristic, say *A*, is the characteristic *B*, this information cannot exclude the possibility that the *next instance*, so far unknown to us, which exhibits the characteristic supposed to be the *implicans* lacks the property supposed to be the *implicat*, or in other words, that the general implication between *A* and *B* is, after all, false.

Thus a second postulate is needed if the method of eliminative induction is to result in the deduction of general implications (or equivalences) from the data of the elimination. This postulate we shall call the Deterministic Assumption.[15] It can be given various, weaker or stronger, formulations. One such formulation is that every property has, in every positive instance of its occurrence, at least one sufficient condition. From this formulation follows that every property also has at least one necessary condition, viz. the disjunction of all its sufficient conditions.[16]

Mill restricted the applicability of the Deterministic Assumption to properties of individuals succeeding each other in time, thus getting the more specific form of it which might be called the Universal Law of Causation,[17] and did so on the ground that he regarded the postulate as being unjustifiable for simultaneously existing properties.[18] Causal Laws, according to Mill, but not Uniformities of Co-Existence, get their strength from such a general principle.

Bacon on the other hand seems to have assumed precisely the opposite to Mill, viz. that for each property or 'nature' of an individual there exists another simultaneous property, called its 'form', being a necessary and sufficient condition of it,[19] whilst he does not state any corresponding general principle for temporally related properties.

The two general 'inductive principles' called by us the Deterministic Assumption and the Postulate of Completely Known Instances are *necessary*, but also *sufficient*, if eliminative induction is to attain certainty in the second of the three senses, i.e. if inductive generalizations shall be deducible from the data of the elimination. This deduction, generally speaking, is made so that the Deterministic Assumption is applied to a situation where the Postulate of Completely Known Instances, together with the actual results of the elimination, entitles us to the conclusion that all concurrent hypotheses *except one* have been eliminated.

Any system of conditions which makes it possible to deduce inductive generalizations from singular data we shall call a Complete System of Inductive Logic. In this sense the above two postulates, together with the logical mechanism of elimination, make up such a Complete System.

It is, obviously, logically possible to strengthen the postulates of our Complete System so that the system's powers of deduction are widened. We might, for example, introduce some more definite assumption as regards the *number* of (sufficient or necessary) conditions of a given conditioned property. Or, we may assume the existence of some restriction to the *complexity* of the possible conditioning properties. Such stronger assumptions have actually been suggested.[20] They will, however, not be discussed here.

§7. *The justification of the postulates of eliminative induction.*

If induction by elimination is to reach certainty in the third of the previously mentioned senses, that is to say, certainty as to the *truth* of a generalization, then it is not sufficient to know only which premisses are needed for the purpose of deducing general propositions. We must, in addition, know that those premisses are *true*.

The postulates of eliminative induction mentioned are all *general* propositions, by the aid of which from singular propositions other general propositions are deduced. The truth of the singular propositions, i.e. the so-called data of the elimination stating that such and such properties belong to such and such an individual, is in this connection unproblematic. This cannot be said of the truth of the general propositions.

Let us first assume the principles to be true *a priori*. As was seen above the sole way to guarantee the truth of a general proposition *a priori* is to make it analytical.[1]

Now it can be shown that if the postulates are supposed to be true *a priori*, i.e. analytical, then any general proposition which, with their aid, is deduced from particular data can be proved true only in the sense of being analytical. This amounts to the same as that if with a general analytical proposition and a singular synthetical proposition as premisses, we draw a general conclusion, then the conclusion must itself be analytical. One might be inclined to say that this thesis is 'almost self-evident'. It will, however, be of some interest to see, how it is related to our previous elucidations of the notions of analyticity and logical consequence.[2]

If the proposition b follows from the proposition a, then the implication $a \rightarrow b$ is an analytical proposition and hence[3] necessary. This relation between logical consequence and necessary implication cannot be converted. For, if a is an impossible proposition or b a necessary proposition, then the implication is necessary, irrespective of whether b follows from a or not.[4] We shall assume,[5] however, that if $a \rightarrow b$ is necessary, then b follows from a in every situation when a is *not* impossible or b *not* necessary. The desired proof can now be given as follows:

If the general proposition g follows from the particular proposition p and the analytical proposition a, then the implication $a \& p \rightarrow g$ is a necessary proposition. Now, according to a law of modal logic, if a is necessary and $a \& p \rightarrow g$ is necessary, then $p \rightarrow g$ is necessary too. But, on our assumption above, if $p \rightarrow g$ is necessary, then it is the case either that g follows from p *or* that p is impossible *or* that g is necessary. We already know that g, the general proposition, is not a logical consequence of p, the data of the elimination. Nor is it the case that p is an impossible proposition. Hence the only remaining alternative is that g is necessary (analytical).[6]

In presenting the scheme of induction by elimination we intended primarily to inquire whether it could reach inductive propositions which were synthetical. Now we have seen that if eliminative induction is to reach this then the general postulates necessary for this method must be synthetical. Our next task will be to see what bearing this has upon the question as to whether eliminative induction can justify inductive inference or not.

If the postulates are synthetical principles, their truth cannot be guaranteed *a priori*. But perhaps it could be established *a posteriori*, i.e. generally speaking according to some 'inductive method'.[7]

It is immediately clear that this 'inductive method' cannot be induction by elimination. For this method presupposes, for the attainment of truth, the two postulates the truth of which is now to be proved. Thus to try this way would be circular.

But it is equally clear that the method cannot be enumerative induction. This method is constantly '*periculo ab instantia contradictoria exponitur*', and can hence never reach certainty.[8]

The possibility then remains that there exists some third inductive method, beside the eliminative and the enumerative ones, which could be used for proving the truth of the two postulates. But this possibility also can be ruled out on the following general grounds:

That a general proposition g can be established by an 'inductive method' means[9] that g can be inferred from some experiential 'data' d in conjunction with some 'rules' or 'principles' p. Since g is a general and d a singular proposition, it follows that p must be general.[10] If the inductive method has to establish the truth of g, the truth of d and p must be known. But, how can the truth of p be known? If it is known *a priori*, then p is analytical. And then it follows that g is analytical too.[11] If, on the other hand, p is known *a posteriori*, then its truth must have been established by means of some other inductive method using other rules or principles p', different from p. Then the questions arises, how p' is established and we are driven to consider some further principles p'', different[12] from both p and p'.

Thus we see that we cannot prove the truth, as general synthetical propositions, of the postulates upon which eliminative induction is based, without reference to some new inductive method, and it cannot be proved that this new method establishes the truth of the postulates without reference to a further inductive method, again different from the former ones, and so on *in infinitum*. As the truth of the two postulates as synthetical propositions was necessary in order that induction by elimination should lead to true generalizations, not being analytical, we can conclude that the whole idea of an inductive method reaching general synthetical propositions, known to be true, has failed as leading to an infinite retrogression. For from its leading to an infinite retrogression follows that it is never possible to know when and if the generalization aimed at can be regarded as true.

§8. *The eliminative method and the justification of induction.*

Although Bacon's idea of an inductive logic able to conclude 'ita ut instantia contradictoria inveniri non potest' is proved to be a failure, we must not underrate the relevance which a system of inductive logic on the above principles nevertheless possesses to the question of justifying induction.

If anybody were to assert that the principles of eliminative induction *justify* a certain general proposition he is likely to assert, not that this generalization can be proved to be *true*, so much as that it can be proved to *follow* from premisses of a particular kind. These premisses are partly singular propositions asserting that certain individuals have certain properties and that, consequently, certain generalizations are invalidated; partly general propositions or 'inductive principles' such as the above Deterministic Asumption or Postulate of Completely Known Instances. The truth of the former premisses is, as a rule, unproblematic. In the truth of the latter premisses, again, we are seldom directly interested. Therefore also the task of proving their truth is of minor importance in view of the practical needs of science.

The really important task of a logic of induction is to analyse the logical mechanism of elimination so that it becomes clear what pure elimination alone can achieve, and exactly to formulate the content of the principles needed in order to extract inductive generalizations from the data of the elimination.[1] When this is done we can determine in the case of given generalizations how far they are based on known data and how far they go beyond our direct experience. This knowledge, which gives us elucidation as to the relation between our inductive conclusions and the experiential evidence on which they are based, can sometimes truly be said to constitute a *justification of induction.*

CHAPTER V

INDUCTION AND PROBABILITY

§1. *The hypothetical character of induction.*

IN the three preceding chapters we have examined different attempts to justify induction as a species of reasoning leading to certainty. We have seen that those attempts are successful or not depending upon what we expect the justification of induction to be. They succeed, under appropriate conditions, *inter alia*, if by 'justification' we mean any of the following three things:

(1) A proof that a given general proposition is a 'law' for certain data, i.e. that those particulars can be deduced from the generalization. (Whewell.)

(2) A proof that a general proposition can be deduced from given particular propositions by the aid of certain other general propositions, called 'inductive principles'. (Bacon-Mill.)

(3) A demonstration that a general proposition, obtained by induction, is analytical, i.e. true *per conventionem*. (Conventionalism.)

But we have also seen that all those attempts *fail* to justify induction in one very important sense, viz. that of proving predictions from an inductive generalization to be true. Or to use a different mode of expression: we cannot prove a synthetical general proposition to be true prior to experiential testing.

The truth that inductive propositions, when used for the purpose of predicting future happenings, are and must always remain *hypotheses* which coming experience may either confirm or refute, has been already clearly apprehended by men of science centuries ago. It was expressed, for instance, by Newton in his *Opticks*, when he said that 'the arguing from experiments and observations by induction' is 'no demonstration of general conclusions'.[1] The same clarity as to the impossibility of proving predictions from a law to be true *a priori* also pervades the works of another great man of science of about the same time, Huyghens.[2]

In spite of all the various doctrines of synthetical judgments *a priori* it can hardly be maintained that any philosopher had explicitly asserted the possibility of proving predictions of concrete events in advance of experience. But it is true that philosophers, even those who have devoted much ingenuity and work to the theory of induction, have until recently paid only slight attention to the fact that inductive propositions, the consequences of which are deduced and successively tested, are *hypotheses*.[3] The significance of this truth has usually been entirely minimized by the stress that those philosophers have laid upon the element of certainty which is also inherent in the inductive mode of reasoning.[4]

The first philosopher to have clearly apprehended and separately emphasized the epistemological significance of the hypothetical element in induction, i.e. of the impossibility of proving the truth of predictions, is Jevons.[5] To have done this is the chief merit of his philosophy of induction. Today the opinions of Jevons in these matters may seem almost trivial, but the way in which they were misunderstood and contested in contemporary philosophy[6] is the best proof that they represented a real step forward in the theory of induction.[7]

§2. *Hypothetical induction and probable knowledge.*

One of the chief aims of science is to provide a basis for successful predictions. Does the fact that knowledge used for predictions is hypothetical affect the possibilities of science to achieve this aim? Is the impossibility of guaranteeing the truth of predictions prior to testing a 'catastrophe to science', does it mean that all prediction is simply haphazard guessing, and that we are left in 'complete uncertitude' as to the future course of nature?

It is obvious that if the results of our previous investigations make such questions seem justified, then the results have been misinterpreted. Because those results, as has already been pointed out several times, are 'grammatical' in their nature, i.e. concern the use of certain words, whereas the above questions seem to protest against some absurd consequence of our results for 'matters of fact'. The questions arise out of the same fundamental mistake as that which makes Berkeley's treatment of the existence of things and Hume's theory of causation appear absurd and catastrophic — the confusion between the clarification of thought and the discovery of facts.

The above questions, therefore, could be said to express a vain worry about the implications of our investigations. But if we are asked to show in detail *why* we need not worry about these imagined consequences, we are soon confronted with the most perplexing problems.

The first answer which suggests itself as settling our anxiety as to the 'catastrophe of science' is roughly the following:

From the fact that we cannot prove what is going to happen it does not follow that we could not estimate, prior to testing, the degree of reliability possessed by a prediction. Such estimations as a matter of fact take place, since certain predictions are actually regarded as very reliable ones, and others again as less or in a very small degree reliable. These different degrees of belief in predictions we also express by saying that predictions are more or less *probable*. Inductive knowledge, in so far as it is hypothetical, is *probable knowledge*. This is why the impossibility of guaranteeing the truth of predictions is no 'catastrophe to science', and why it is overhasty to say that we are left in 'complete uncertitude' as to the future.

If this answer were satisfactory it would imply, that the *fact* itself that certain predictions are believed more, and others again less strongly, were all the *justification of inductive predictions* that we need.

To this, however, there appears to be a strong *prima facie* objection. From the fact, it will be said, that one prediction or one generalization is *regarded* as more reliable, i.e. believed more strongly than another, it does not follow that this prediction or generalization really *is* more reliable than the other one. That is to say, we might have been mistaken in our judgments about probabilities. The reference to degrees of belief, therefore, is unsatisfactory as a justification of induction unless we can justify the beliefs themselves, that is to say can guarantee with certainty, or at least with probability, that we are not mistaken in our estimations of the probability of inductive propositions.[1]

Thus we have arrived at the idea that the justification of hypothetical induction does not lie in the fact, as such, that we *estimate* degrees of probability, but is to be found in some 'mechanism of probability' underlying these estimations and guaranteeing their validity. To the examination of this idea, which has played, and still plays, a profound role in discussions and philosophical controversies about induction, we shall devote the following chapters.

§3. *A scheme for the treatment of inductive probability.*

The idea of a 'mechanism of probability' underlying our estimations of reliability in inductive propositions gains support from the following observations:

These estimations, it appears, are not made on 'intuition' only but take place in conformity with certain rules. Such rules are for instance: The probability of a generalization increases with the number of verified instances of it; the verification of an unexpected or surprising instance of a law contributes more to its reliability than the verification of an instance of a type with which we are familiar; the probability of an induction is, somehow, proportionate to the scope of the generalization.

It will be our task in Chapter VI to give a *formal analysis* of the rules of inductive probability. We will inquire into the conditions under which the above-mentioned rules and certain others are *provable*, that is to say, what assumptions need to be made about probabilities in order that those rules shall become logically necessary. That this analysis is purely 'formal' also implies that it is pursued without any presuppositions as to the 'meaning' of probability or as to how probability-values are empirically determined.

The formal treatment of inductive probability will show that the rules mentioned are all deducible from a common set of simple assumptions. This is important as it proves that those rules, frequently mentioned in works on induction but seldom analysed into their formal interconnections, form part of *a coherent system of inductive probability.*

Moreover the analysis is peculiar inasmuch as that it shows the formal structure of the probability-concept of this system of inductive probability to be the same as the formal structure of *that* probability-concept which is treated in the branch of mathematics known to us under the name of the ('classical') calculus of probability. This shows that the idea of two kinds of probability — 'mathematical' probability and 'philosophical' probability, the latter being essentially the probability of inductions — is unnecessary at least in so far as the formal nature of inductive probability is concerned.

The formal analysis of the rules of inductive probability cannot in itself determine the relevance of those rules to the *justification* of induction.

In Chapter VII we return to the problem of justification. We shall try to show, why any justification of induction with probability, intended to refute Hume's scepticism as regards the possibility of guaranteeing anything about the future course of nature, is doomed to failure irrespective of how we interpret the probability-concept. From this will follow that no dichotomy into different kinds of probability — 'mathematical' and 'philosophical' or whatever the terms be — is helpful towards a solution of Hume's Problem.

CHAPTER VI

FORMAL ANALYSIS OF INDUCTIVE PROBABILITY

§1. *The Abstract Calculus of Probability.*

Historically, the mathematical study of probability was developed on the basis of a study of certain mathematical *models* of the concept. The oldest[1] of these models is provided by the well-known definition of probability as a ratio of cases or possibilities, 'favourable' and 'unfavourable' to a certain event or to the outcome of a certain experiment. This model was suggested by considerations pertaining to games of chance. Another model is provided by the definition of probability as the relative frequency of a characteristic or an event within a class ('population'). Probability-mathematics, when developed on the basis of the first model, was primarily a branch of the theory of combinations and permutations. Probability-mathematics developed from the frequency or statistical model, may be termed a class-ratio arithmetic.[2]

In the two models are reflected different opinions as regards the meaning of probability and about the relation of the mathematical theory to empirical reality. 'Philosophically', these opinions are highly divergent. We shall not here be interested in the question, which of them is right or whether some of them can be 'reconciled' by virtue of the fact that they fit *different* concepts of probability.[3] The logico-mathematical nature of the models themselves will be somewhat more closely scrutinized in the next section.

It is a most important fact that the theories of probability, which can be developed on the basis of the two models mentioned, though differing in their conception of the 'meaning' of their fundamental notion, yet agree, by and large, in their *logical structure*. This fact suggests the possibility of creating an Abstract Calculus of Probability, i.e. a deductive theory which is 'neutral' with regard to conflicting opinions about the meaning of probability and studies only the mathematical laws which this notion obeys. Within a theory of this abstract kind 'probability' figures as an undefined term, for which certain 'axioms' or 'postulates' are laid down. The axioms are sometimes said to constitute an *implicit definition* of probability.

Several abstract calculi of probability have been suggested[4] and some have been developed in detail. They fall into two groups. We shall call them the *set-function* calculi and the *logistic* calculi.

An Abstract Calculus of Probability on a set-function basis has been developed by the Russian mathematician A. Kolmogorov in an important publication from the year 1933.[5] In Kolmogorov's calculus probability figures as a *function of sets*. The theory has received much favour among mathematicians[6] and is perhaps the most satisfactory mathematical treatment of probability which has been presented. It incorporates probability mathematics within the general theory of measurable sets of points.

The theory of J. M. Keynes from the year 1921 may be regarded as the first attempt on a large scale at the development of an abstract calculus of the logistic type.[7] Another, more consciously 'formalist', system of the same type is that of Hans Reichenbach. It was first presented in 1932.[8] Axiomatic systems, similar to that of Keynes, are due to S. Mazurkiewicz and H. Jeffreys.[9]

Systems of probability of the type here called 'logistic' form a rather heterogenous group. Common to all members of the group is that they conceive of probability as a 'logical relation' between two entities. The entities may be propositions, as in the systems of Keynes and Jeffreys, or they may be attributes (propositional-functions, properties, classes) as in the theory of Reichenbach.[10] They might with a common name be called *proposition-like entities*. Their 'proposition-likeness' consists, vaguely speaking, in the fact that they can all be manipulated with the aid of the so-called truth-connectives: negation, conjunction, disjunction, etc.[11]

The logistic calculi of probability thus stress the *relative* (or relational) nature of probability. Probability is a quantity depending for its value upon the 'field of measurement' in which it is determined.[12]

For our purposes a logistic calculus of probability is better suited than a set-function one. We shall in this section outline a logistic Abstract Calculus of Probability. In order to simplify the treatment we have made as many omissions of points of a formal nature as has seemed to us possible without seriously damaging the logical rigour of the arguments. The purpose of our inquiry, it will be remembered,[13] is to indicate, how certain popular ideas about the probability of inductions may be assigned a place within the common framework of probability mathematics.

We introduce a symbol '$P(a/h)$' which we call probability-functor. It can be read: the probability of a relative to h. Instead of 'relative to' we may also say 'on data' or 'given' or simply 'in'.

It may be asked, whether '$P(a/h)$' makes sense for *any* two propositions, a and h, i.e., whether it makes sense to say that any given proposition possesses, relative to any other given proposition a (known or unknown) probability. It is *prima facie* plausible to think that a and h would need to be somehow 'materially related' in order to determine a probability-relation. What sense could it make to speak of the probability of a proposition of, say theoretical physics relative to a proposition of history? It is further doubtful, whether a proposition can have a probability relative to a self-contradictory or for other reasons logically impossible proposition.[14]

It is thus reasonable to think that the propositions (proposition-like entities) a and h must satisfy certain conditions in order to determine a probability-relation.

It may further be asked, whether '$P(a/h)$', when significant, necessarily signifies a number, i.e. whether the probability of a proposition relative to another proposition is necessarily a numerical magnitude. This is a serious question, not least from the point of view of a theory of induction. For it is sometimes said that probability as an attribute of inductive conclusions, though a legitimate concept, differs from 'ordinary' probability, among other things, in being *non-numerical.*[15] Of probability which is considered as non-numerical the question may be raised, to what extent it is *comparative*, i.e. obeys laws about 'greater', 'equal', and 'less'.[16]

We shall, however, in this chapter deliberately ignore the problems about *the existence of the probability-relation* and about *the numerical or non-numerical nature* of (inductive) probability. In constructing our Abstract Calculus we simply proceed on the assumption (convention) that with such pairs of proposition-like entities as may enter our arguments there may be co-ordinated a *unique, non-negative real number* subject to the following axioms or postulates:

A1. $P(a/h)+P\ (\sim a/h)=1$. The sum of the probability of a proposition and the probability of its negation, relative to one and the same datum, is 1.

A2. $P(a\&b/h)=P(a/h)\times P(b/h\&a)$. The probability, given h, of a conjunction of two propositions, a and b, is the probability of a on h as datum multiplied by the probability of b on h *and* a as datum. We call this the Multiplication Principle.

A3. If h is self-consistent, then $P(h/h)=1$.

On the basis of these axioms, and a few principles of subordinate nature, the whole fabric of what might be called 'classical' probability mathematics can be erected.

The deduction of theorems from the axioms can be completely 'formalized', i.e. subjected to explicitly stated rules for the manipulations of formulae. We shall not burden the exposition by enumerating the rules of inference. We only mention that the deduction, by and large, proceeds through 'substitutions of identities', and that of such substitutions there are two kinds, viz.

(i) *external* substitutions, of numerically identical expressions, in the equations, and

(ii) *internal* substitutions, of logically identical propositions, in the probability-functors.

(Examples of the two types of substitution of identities are indicated below.)

We mention a number of elementary theorems.

T1. $O\leqslant P(a/h)\leqslant 1$. Probability is a magnitude between O and 1, inclusive the limits.

That probability is not-negative, i.e. equal to or greater than O, was established by convention. (P. 92.) From this convention *and* the postulate (*A1*) stating that the probabilities of a proposition and of its negation, on one and the same datum, add up to 1, it immediately follows that probability must be equal to or smaller than 1.

T2. If h is self-consistent and entails a, then $P(a/h) = 1$.

Proof:

(1) If h entails a, then $a\&h$ is logically identical with h.[17]

(2) If $a\&h$ is logically identical with h, then by internal substitution $P(h/h) = P(a\&h/h)$.

(3) $P(a\&h/h) = P(h/h)\times P(a/h\&h)$. (From *A2*.)

(4) $h\&h$ is logically identical with h.

(5) Thus, by internal substitution, $P(a/h\&h) = P(a/h)$.

(6) Hence, by external substitution in (3), $P(a\&h/h) = P(h/h) \times P(a/h)$.

(7) If h is self-consistent, then $P(h/h) = 1$. (*A3*.)

(8) Hence, by external substitution in (6), and considering the identity established in (2), we get $P(a/h) = 1$. This completes the proof of *T2*.

T3. $P(a/h) = P(a\&b/h) + P(a\&{\sim}b/h)$.

(1) $P(b/h\&a) + P({\sim}b/h\&a) = 1$. (From *A1*.)

(2) Since a probability is, by convention, a unique value, we have $P(a/h) = P(a/h)$.

(3) $P(a/h) = P(a/h) \times [P(b/h\&a) + P({\sim}b/h\&a)]$. (From (1) and (2).)

(4) $P(a/h) = P(a/h) \times P(b/h\&a) + P(a/h) \times P({\sim}b/h\&a)$. (From (3).)

(5) $P(a/h) = P(a\&b/h) + P(a\&{\sim}b/h)$. (From (4) with the aid of *A2*.)

We shall call *T3* the Division Principle.

T4. $P(avb/h) = P(a/h) + P(b/h) - P(a\&b/h)$.

(1) $P(avb/h) = P((avb)\&b/h) + P((avb)\&{\sim}b/h)$. (From *T3*.)

(2) $(avb)\&b$ is logically identical with b alone, and $(avb)\&{\sim}b$ is logically identical with $a\&{\sim}b$. Thus by internal substitution in (1) we obtain

(3) $P(avb/h) = P(b/h) + P(a\&{\sim}b/h)$.

(4) $P(a\&{\sim}b/h) = P(a/h) - P(a\&b/h)$. (From *T3*.)

(5) $P(avb/h) = P(a/h) + P(b/h) - P(a\&b/h)$. (From (3) and (4) by external substitution.)

T4 will be called the Addition Principle.

That two propositions are *mutually exclusive* means that the negation of either proposition logically follows from the other proposition. Thus, if a and b are mutually exclusive, ${\sim}b$ follows from a.

T5. If a and b are mutually exclusive and consistent with h, then $P(avb/h) = P(a/h) + P(b/h)$.

It may easily be proved that, if a and b are mutually exclusive and consistent with h, then $P(a\&b/h) = O$. We leave the proof as an exercise to the reader.

We shall call *T5* the Special Addition Principle.

We say that a is *independent* of b (for probability) in h, if $P(a/h) = P(a/h\&b)$.

T6. If a is independent of b in h or if b is independent of a in h, then $P(a\&b/h) = P(a/h)\times P(b/h)$.

(1) $P(a\&b/h) = P(a/h)\times P(b/h\&a) = P(b/h)\times P(a/h\&b)$. (From *A2*.)

(2) If a is independent or b, then $P(a/h) = P(a/h\&b)$. If b is independent of a, then $P(b/h) = P(b/h\&a)$. In either case we get from (1) by external substitution:

(3) $P(a\&b/h) = P(a/h)\times P(b/h)$.

We call *T6* the Special Multiplication Principle.

For the higher development of the calculus, the notion of independence is of the greatest importance.[18]

If extreme probabilities (the values 0 and 1) are excluded, we can prove the following theorems:

T7. If a is independent of b in h, then b is also independent of a in h.

T8. If a is independent of b in h, then a is also independent of $\sim b$ in h.

T9. If a is independent of b in h, then $\sim a$ is also independent of b and of $\sim b$ in h.

We leave the proof of these elementary theorems to the reader.

*

Consider an event such as, e.g., getting 'head' in tossing a coin. We symbolize the occurrence of the event by 'E' and its non-occurrence by '$\sim E$'.

Consider further a sequence of occasions, on which the event may occur or fail to occur, e.g., a sequence of tosses with a coin. We symbolize the sequence of occasions by 'x_1', 'x_2', . . .

'Ex_n' means that the event has occurred on the nth occasion, and '$\sim Ex_n$' means that the event has failed to occur on the nth occasion.

We shall say that the occurrence (and non-occurrence) of E on the occasions x_1, x_2, . . . are independent for probability in h, if the probability, relative to h, of any proposition Ex_n is independent for probability in h of any proposition h' to the effect that E has occurred or failed to occur on some other occasions than x_n. (The proposition h' is thus itself a conjunction of propositions of the form Ex_i and $\sim Ex_i$.)

A sequence of occasions of independent occurrences (and non-occurrences) of an event are said to constitute an *independence-realm.*[19]

An independence-realm will be called *normal,* if the probabilities for the occurrence of the event on the respective occasions are neither 0 nor 1.

A normal independence-realm will be called *Bernoullian,* if the probabilities for the occurrence of the event on the respective occasions are all *equal.* If the probability of the occurrence of the event is p (*the Bernoullian probability*), then the probability of its non-occurrence is $1-p$.

The notions of the various independence-realms can easily be generalized so as to apply to sequences of occasions for the occurrence and non-occurrence, not of *one* event E only, but of any number n of events $E_1, \ldots, E_n$. For our purpose, however, it will suffice to consider the simplest case, when there is one event E only.

For Bernoullian independence-realms can be proved a very important theorem, usually known as Bernoulli's Theorem. It is convenient to divide its content into two parts or stages.

We ask for the probability that, on n occasions, E will occur m times and fail to occur n-m times. This can happen in ${}^nC_m$ different ways,[20] all of which are mutually exclusive. The probability that E will occur and fail to occur in *a given one* of these ${}^nC_m$ ways is, by repeated use of the Special Multiplication Principle, found to be $p^m \times (1-p)^{n-m}$, where p is the Bernoullian probability. The probability again that E will occur and fail to occur in *any one* of the ${}^nC_m$ ways is, by repeated use of the Special Addition Principle, found to be ${}^nC_m \times p^m \times (1-p)^{n-m}$.

It can be proved [21] that, for given p and n, the calculated probability has its maximum value for that value of m:n which is closest to p. In other words: *the most probable value of the event's relative frequency on n occasions is the value which is closest to the event's Bernoullian probability p.* This, which may also be called the Direct Law of Maximum Probability (for Bernoullian Independence-Realms), constitutes the first stage in the proof of Bernoulli's Theorem.

We next ask for the probability that, on n occasions, E will occur m times and fail to occur n-m times, m now being a *variable* which runs through all values (integers) for which the ratio m:n falls in the interval $p \pm \varepsilon$. (ε is an arbitrary quantity which may be as small as we please.) This probability is calculated with the aid of a further application of the Special Addition Principle and is simply the sum Σ of the values ${}^{n}C_{m} \times p^{m} \times (1-p)^{n-m}$ when m runs through the interval just mentioned. Of this sum Σ it can be proved[22] that, for given values of p and ε, it is greater, the greater n is. As n approaches infinity, Σ approaches 1 as a limit. In other words: *if the Bernoullian probability of the event is p, then the probability that the event's relative frequency on n occasions will deviate from p by less than an amount* ε, *however small, approaches as a limit the maximum value* 1 *as n is indefinitely increased.* This, which may also be called the Direct Law of Great Numbers (for Bernoullian Independence-Realms), constitutes the second and final stage in the proof of Bernoulli's theorem.

Thus, loosely speaking, the first part of Bernoulli's Theorem tells us that the most likely relative frequency of an event is that which is indicated by its probability, and the second part that 'in the long run' it becomes infinitely probable that the event's relative frequency will equal its probability. The danger of using this loose mode of speech is that it leaves without mention the, rather sweeping, assumptions of independence which are essential to a correct proof of the theorem.

§2. *The interpretation of formal probability.*

It is usual to distinguish between two types of interpretation or model of abstract calculi.[1] In the one type of interpretation it is *assumed* that certain empirical objects 'satisfy' the postulates (axioms) which the calculus lays down for the undefined concepts. In the other type of interpretation it is *proved* of some entities that they conform to the postulates.

An example of an interpretation of the first type is when we conceive of Euclidean geometry as a physical theory of space ('light-ray geometry'). As is well known Euclidean geometry, as such a *theory*, is supposed to have been falsified by some experiments which are relevant to the acceptance of Einstein's Theory of Relativity.

An example of interpretations of the second type is Descartes's invention of analytic geometry. In this geometry the system of

Euclid is 'modelled' in the realm of numbers, the axioms and theorems of abstract Euclidean geometry becoming provable propositions within another branch of mathematics.

It is of some importance to note that all known interpretations of abstract probability are interpretations of the second type, i.e. logico-mathematical models. For this reason one ought not, as is sometimes done,[2] to regard the relation between abstract and interpreted probability as presenting a close analogy to the relation between axiomatic geometry and the physical theory of space.

There are many models of abstract probability, but all of them which are known to be important fall within one of two main categories.[3] We shall call these categories frequency-models and range-models. Since the members of the respective catagories are closely similar to each other, we may also speak of the categories themselves as *the* frequency- and *the* range-model of abstract probability. The frequency-model may also be called a *statistical*, and the range-model a *modal* interpretation of probability ('possibility'-interpretation).

The Frequency-Model. Any model of the statistical type presupposes that the terms of the probability-relation are, not propositions, but propositional-functions. It will suffice for our purpose to consider propositional-functions of the simplest kind only, viz. propositional-functions of *one* variable. We shall further make the simplifying assumption that the propositional-functions which enter as terms of one and the same probability-relation are functions of one and the same variable. Under these simplifying assumptions we can speak of the probability-relation as a relation between some attributes or properties or classes.

On the frequency view, the probability of a given h is the *relative frequency* of such values of the variable which statisfy the propositional-function a among all values of the variable which satisfy the propositional-function h. (Instead of 'value of the variable' we may say 'individual'.) Popularly speaking, the probability of a given h is the relative frequency with which an 'event' of the nature a takes place when the 'conditions' h are fulfilled. Or differently again: the probability of a given h is the proportion of h's which are a's.

Since the relative frequency in question thus is a proportion of true propositions within a class of propositions, it may also be called a *truth-frequency*.[4]

The notion of a relative frequency or a proportion is straightforward and presents no difficulties, if the number of values of the variable which satisfy h is restricted to a finite number n. It is easy to show that such a Finite Frequency-Model satisfies our postulates of abstract probability.[5]

The Finite Frequency-Model is, for reasons which we need not consider here, thought very unsatisfactory from the point of view of accounting of the 'meaning' of probability.

If the number of values of the variable which satisfy h is potentially infinite, the proportion of h's which are a's is the *limiting value* of a relative frequency in a sequence.[6] The notion of a limiting-frequency must not be regarded as 'meaningless' or in any other way logically unsatisfactory. But it is important to observe that the notion makes sense only relative to a *way of ordering* the values of the variable which satisfy h.[7] By re-ordering the sequence we may alter or even destroy the limiting value.[8]

The Frequency-Limit-Model can be shown to be also a valid interpretation of the Abstract Calculus of Probability.[9]

Against the Frequency-Limit-Model too, as a suggested analysis of probability, many grave objections can be levelled. One is that empirical propositions about proportions in infinite 'populations' can be neither verified nor falsified by statistical observation. Another is that a probability is (usually) not thought to depend upon a way of ordering the members of a population. And a third is that use of probability calculations for statistical predictions is by no means always tied up with beliefs in limiting-frequencies in nature.

A problem which has been very much discussed in connection with the Frequency-Limit-Model of probability concerns the manner in which those values of the variable which satisfy a are *distributed* among the values which satisfy h. It has been thought that unless this distribution satisfies some conditions as to *irregularity* or *randomness*, the frequency-model cannot give an *adequate* account of what we mean by a probability. The definition of random distribution, however, constitutes a difficulty.[10]

It should be observed that none of the difficulties mentioned impairs the *logico-mathematical correctness* of the Frequency-Model (for finite or infinite populations) of abstract probability. And *this* correctness of the frequency-view is all that concerns us in the present investigation.[11]

The Range-Model.[12] A model of this type can be worked out both on the assumption that the terms of the probability-relation are propositions and on the assumption that they are attributes.[13] We here adopt the former alternative, which appears to be the more usual one.[14]

The range-theory of probability, in its simplest form, can be loosely explained as follows:

We 'analyse' the evidence-proposition h into a number, say n, of alternatives, $h_1, \ldots h_n$, which are mutually exclusive and such that some of them, say m, entail the proposition a and the rest entail the proposition $\sim a$.[15] In conformity with a traditional terminology, we shall call the alternatives, which entail a, 'favourable' alternatives and the alternatives, which entail $\sim a$, 'unfavourable' alternatives. By the probability of a given h we now understand the ratio of 'favourable' alternatives to all alternatives, or the proportion of alternatives 'favourable' to a among all alternatives which fall under h.

We may call this the 'classical' form of the range-definition of probability. Omitting an important qualification to be mentioned presently, it answers approximately to the definition of probability proposed by Laplace[16] and current in books on the subject up to the present day.

The 'classical' definition can be generalized and the description of it made more exact in the following way:

We consider some set σ of propositions, such that a and h are truth-functions[17] of some members of σ.[18] We thereupon consider the disjunctive normal forms of $a \& h$ and of h in terms of all the members of σ. Let us assume that the normal form of $a \& h$ is m-termed and that the normal form of h is n-termed, i.e. let us assume that the normal forms are disjunctions of m and of n conjunctions respectively. By the probability of a given h we now mean the ratio $m:n$.[19]

By the *σ-range* of a proposition we shall mean its disjunctive normal form in terms of the members of σ.[20] (In this definition it is presupposed that the proposition in question *is* a truth-function of the propositions in σ.) The conjunctions in the normal form we shall call *unit-alternatives*.[21]

The numbers m and n above are *measures* of the σ-ranges of the propositions $a\&h$ and h respectively. These measures are obtained simply by counting the number of units in the ranges of the propositions. But we may also adopt some other method of measurement which assigns, not an equal, but an unequal 'weight' to these units. In this way we arrive at a generalized notion of the measure of a range. We introduce the symbol 'mr_σ' for 'measure of σ-range'.

On the basis of this generalized notion of a range-measure, we introduce the definition $P(a/h) = df\ \dfrac{mr_\sigma(h\&a)}{mr_\sigma(h)}$.[22]

It may be shown that for any choice of σ and mr_σ — subject only to a few restrictions of a very general nature[23]—this ratio of measures of ranges satisfies the postulates of abstract probability. The above 'classical' range-definition and its generalized form may be regarded as special types of such ratios.

It is characteristic of this definition that if, given a and h, we ask what is the value of $P(a/h)$ the answer will depend upon the choice of σ and the choice of mr_σ.[24] Since, from the point of view of the *applications* of probability, not all choices are equally good, we are led to consider the problem of *adequacy* in the choice.

The answer given to this problem in the 'classical' theory of probability can, in our terminology, be stated as follows:

The choice of σ ought to be such that each unit in the ranges can be given an *equal weight*. Or, popularly speaking, the data ought to be analysable into a number of *equipossible* alternatives.

When this condition is added to what we called above the 'classical' range-definition we get the following: The probability of a given h is the proportion of alternatives, which are 'favourable' to a among a number of *equally possible* alternatives which fall under h.

But how can we be sure that an analysis of the data leads to equipossible unit-alternatives? To this question the 'classical' theory proposed as an answer the famous Principle of Insufficient Reason, also called the Principle of Equal Distribution of Ignorance or the Principle of Indifference. The applicability and formulation of this principle have been the object of much discussion and controversy.[25] In the original formulation, given to it by James Bernoulli, the principle states that alternatives should be held equally possible, when no reason is known why one of them rather than another should come about.[26]

One other answer to the problem of the adequate choices of σ and mr_σ should be mentioned before we leave this topic:

The choice of mr_σ ought to be such that for a given σ, the ratio $\frac{mr_\sigma(h\&a)}{mr_\sigma(h)}$ equals the value of $P(a/h)$ on the frequency-interpretation.[27]

To accept this answer is to let the frequency-interpretation function as a check on the *adequacy* of any particular range-interpretation of the concept of probability. This may be said to ignore the problem of adequacy for the frequency-interpretation. But it is nevertheless noteworthy that one of the difficulties confronting the range-theory of probability is how it can be used to account adequately for probability-values without taking refuge, so to speak, in the frequency-theory.[28]

In this inquiry we are, however, not interested in the difficulties and problems confronting the range-theory as a proposed analysis of the meaning of probability. For our purposes it will suffice to note that a definition of probability in terms of range-measures — just as a definition of the concept in terms of relative frequencies — is possible and gives a mathematically correct model of our Abstract Calculus.

§3. *The doctrine of Inverse Probability.*

In the Theorem of Bernoulli, one might say, we argue *from* probabilities *to* probable values of relative frequencies or proportions. Can this argument be reversed or inverted? Is there a theorem of the calculus which enables us to conclude *from* knowledge of relative frequencies *to* probable values of probabilities? If this theorem were analogous to the Theorem of Bernoulli, it would, speaking approximately, tell us that the most likely value of an event's probability is that indicated by its actual relative frequency.

and that 'in the long run' it becomes infinitely probable that the relative frequency with which an event occurs gives us the true value of its (hitherto unknown) probability.

It is a noteworthy fact that James Bernoulli, in proving the Direct Law of Great Numbers, evidently believed himself to have proved also the inverse of it, i.e. that knowledge of actual frequencies entitles us to probable conclusions about probabilities.[1] Later authors again sometimes believed the inverted theorem to follow as a matter of course from the direct one.[2] The reasoning underlying such an idea has an apparent plausibility in its favour, but is nevertheless thoroughly fallacious. Since the question is of a certain interest to the problem of induction, we shall examine in some detail the way to a correct proof of the Inverse Laws of Maximum Probability and of Great Numbers.

The inversion essentially relies upon an elementary formula of probability theory which is *not* needed for the proof of the direct principles. We shall call it, following Keynes,[3] the Inverse Principle. Its proof is as follows:

(1) $P(b/h\&a) = \dfrac{P(a\&b/h)}{P(a/h)}$. (From *A2* provided $P(a/h)>0$.)

(2) $P(a\&b/h) = P(b/h) \times P(a/h\&b)$. (From *A2*.)

(3) $P(a/h) = P(a\&b/h) + P(a\&{\sim}b/h)$. (*T3*.)

(4) $P(a/h) = P(b/h) \times P(a/h\&b) + P({\sim}b/h) \times P(a/h\&{\sim}b)$. (From (3) and *A2*.)

$$T10.\ P(b/h\&a) = \frac{P(b/h) \times P(a/h\&b)}{P(b/h) \times P(a/h\&b + P({\sim}b/h) \times P(a/h\&{\sim}b).}$$

(From (1), (2), (4).)

This is the simplest form of the Inverse Principle. We next consider two generalizations of it.

Let $b_1 \ldots b_s$ be s mutually exclusive and jointly exhaustive alternatives. Then we have $P(a/h) = P(a\&(b_1 \mathrm{v} \ldots \mathrm{v} b_s)/h) =$ $P(a\&b_1 \mathrm{v} \ldots \mathrm{v}\, a\&b_s/h) = P(a\&b_1/h) + P(a\&b_2/h) + \ldots + P(a\&b_s/h) =$ $P(b_1/h) \times P(a/h\&b_1) + \ldots + P(b_s/h) \times P(a/h\&b_s) =$

$$\sum_{i=1}^{s} P(b_i/h) \times P(a/h\&b_i).$$

If, in *T10*, we substitute the last expression for the denominator and b_1 for b, we reach the following generalization of the Inverse Principle:

T11. If b_1 is one of s mutually exclusive and jointly exhaustive alternatives $b_1 \ldots b_s$, then $P(b_i/h\&a) = \dfrac{P(b_i/h) \times P(a/h\&b_i)}{\sum_{i=1}^{s} P(b_i/h) \times P(a/h\&b_i)}$.

From *T11* we immediately reach one further generalization of the principle, viz..

T12. If $b_{i_1}, \ldots, b_{i_k}$ are k of some s mutually exclusive and jointly exhaustive alternatives $b_1 \ldots b_s$, then

$$P(b_{i_1} \vee \ldots \vee b_{i_k}/h\&a) = \frac{\sum_{j=1}^{k} P(b_{i_j}/h) \times P(a/h\&b_{i_j})}{\sum_{i=1}^{s} P(b_i/h) \times P(a/h\&b_i)}.$$

In speaking of the Inverse Formula we shall usually mean the formula given in *T11.* In conformity with traditional terminology we shall speak of the probabilities $P(b_i/h)$ as the initial or *a priori* probabilities, of the probabilities $P(a/h\&b_i)$ as the likelihoods, and of the probabilities $P(b_i/h\&a)$ as the *a posteriori* probabilities.

If all *a priori* probabilities are equal, the values $P(b_i/h)$ cancel out in the right-hand member of the Inverse Formula. Then it is readily seen that the *a posteriori* probability $P(b_i/h\&a)$ has its maximum when the corresponding likelihood $P(a/h\&b_i)$ is greatest.

From the Inverse Principle we arrive at the Inverse Laws of Maximum Probability and Great Numbers in the following principal steps:

Let there be s Bernoullian independence-realms for the event E. And let the Bernoullian probabilities be $p_1 \ldots p_s$. We assume, for the sake of simplicity, that these probabilities are all different from each other and that $p_1 < \ldots < p_s$.

The reader may think of these s independence-realms as s different sets of conditions under which E may occur. Each time the conditions are satisfied we have an occasion for E's occurrence. On each occasion E may either occur or fail to occur. That the conditions constitute an independence-realm for E means that the probability of E's occurrence on a given occasion is not affected by E's occurrence and non-occurrence on other occasions. And that the independence-realm is Bernoullian, finally, means that the probability of E's occurrence remains the same on each occasion.

The 'classical' illustrations are from games of chance. An independence-realm could be the potentially infinite sequence of drawings, with replacement, of balls from an urn. If there are s different urns, there are s such independence-realms. The event E could be the drawing of a black ball. Under 'normal' conditions of drawing we regard the results as independent for probability of each other. The *Bernoullian* character of the independence-realms, again, is guaranteed by the stipulation that the ball should be replaced after each drawing.

In each of these independence-realms we can calculate the probability that E will happen exactly m times on n occasions. In the realm with the Bernoullian probability p_i this value is

$$^{n}C_{m} \times p_i^{m} \times (1-p_i)^{n-m}.$$

Let q_1 be the probability that (a random set of) n occasions for E's occurrence belong to the independence-realm with the Bernoullian probability p_1 . . . and q_s the probability that they belong to the realm with the Bernoullian probability p_s.

For example: q_1 is the probability that n drawings, with replacement, are from an urn in which the probability of drawing a black ball is p_1. It is assumed that the n drawings are from one and the same urn.

We can now, given that E occurs exactly m times on n occasions, use the Inverse Formula of *T11* to calculate the probability that those occasions belong to an independence-realm with the Bernoullian probability p_i. ($1 \leqslant i \leqslant s$.) This value is:

$$(A) \qquad \frac{p_i^{m} \times (1-p_i)^{n-m} \times q_i}{\sum_{i=1}^{s} p_i^{m} \times (1-p_i)^{n-m} \times q_i}.$$ [4]

Further, we may use the Inverse Formula of *T12* to calculate the probability that those occasions belong either to an independence-realm with the Bernoullian probability p_{i_1} or . . . or to an independence-realm with the Bernoullian probability p_{i_k}. This value is:

(B) $$\frac{\sum_{j=1}^{k} p_{i_j}{}^m \times (1-p_{i_j})^{n-m} \times q_{i_j}}{\sum_{i=1}^{s} p_i{}^m \times (1-p_i)^{n-m} \times q_i}.$$

For example: Let there be n drawings, with replacement, from one and the same urn. On exactly m of those n occasions a black ball is drawn. (A) now gives us the probability that the drawings were from an urn, in which the probability of drawing a black ball is p_i. (B) gives the probability that they were either from an urn, in which this probability is p_{i_1}, or . . . or from an urn in which it is p_{i_k}.

Let us assume that all q-values are *equal*. Then they cancel out from (A) and (B) above and we get the simplified expressions:

(A') $$\frac{p_i{}^m \times (1-p_i)^{n-m}}{\sum_{i=1}^{s} p_i{}^m \times (1-p_i)^{n-m}}$$

and

(B') $$\frac{\sum_{j=1}^{k} p_{i_j}{}^m \times (1-p_{i_j})^{n-m}}{\sum_{i=1}^{s} p_i{}^m \times (1-p_i)^{n-m}}.$$

For given values n and m, the denominators of the four expressions have a constant value. The maginitude of the four expressions themselves is then directly proportionate to the value of their numerators. This value again varies with the choice of i or of the set $i_1, \ldots, i_k$.

Of $p_i{}^m \times (1-p_i)^{n-m}$ it can be proved[5] that it reaches its maximum for that value of i which is nearest to the ratio $m{:}n$. This means that (A') reaches its maximum for that value of i too. The result can be expressed in words as follows:

If it is initially or a priori *equally probable that a set of n occasions for the occurrence of an event E belongs to any given one of s Bernoullian independence-realms for E's occurrence, then it is* a posteriori, *i.e. given the information that E occurs on m of those n occasions, most probable that the set of occasions belongs to an independence-realm,* *in which the Bernoullian probability of E is closest to its actual relative frequency m:n.*

We shall call this the Inverse Law of Maximum Probability (for Bernoullian independence-realms).

For example: If it is initially equally probable that a set of n drawings are from any given one of s urns, then it is *a posteriori* most probable that the drawings are from an urn, in which the probability of drawing a black ball is closest to the actual relative frequency of black balls drawn.

Consider an interval $\pm \varepsilon$ round the ratio m:n. Let us assume that $p_{i_1} \ldots p_{i_k}$ are all the p-values which happen to fall in this interval. and that not all the corresponding q-values $q_{i_1} \ldots q_{i_k}$ equal O. On these assumptions it can be proved[6] that the value of the numerator in (B) increases with n and approaches the value of the denominator as n is indefinitely increased. Thus the value of (B) increases with n and approaches 1 as a limit. The result can be expressed in words as follows:

If it is not initially or a priori *infinitely improbable, i.e. probable to degree O, that a set of n occasions for the occurrence of an event E belongs to some Bernoullian independence-realm for E's occurrence, in which the Bernoullian probability of E differs by, at most, an arbitrary amount* ε *from the actual relative frequency m:n of E, then the probability that the set of occasions belongs to some such independence-realm increases with the number n of occasions and approaches the maximum value* 1 *as n is indefinitely increased.*

We shall call this the Inverse Law of Great Numbers (for Bernoullian independence-realms).

For example: Unless it is initially infinitely improbable that the drawings are from an urn, in which the probability of drawing a black ball differs by, at most, the value ε from the actual relative frequency of black balls in those drawings, then the probability that the drawings are from some such urn increases with the number of drawings and approaches the maximum value 1 as a limit.

The Inverse Laws of Maximum Probability and Great Numbers can be given a particularly elegant formulation, if we use integration. Then we have to replace the assumption that there is a limited number s of Bernoullian independence-realms for the event by the assumption that there is a not-denumerable infinity of such independence-realms, one for each of the possible values of E's Bernoullian probability. The range of the possible values is the whole range from 0 to 1 inclusive.

The *a priori*-probability or q-value, associated with a given Bernoullian probability or p-value p, we denote by q_p. The formulae (A) and (B) above now become formulae

$$(C)\qquad \frac{p^m \times (1-p)^{n-m} \times q_p}{\int_0^1 p^m \times (1-p)^{n-m} \times q_p \, dp}$$

and

$$(D)\qquad \frac{\int_{(m:n)-\varepsilon}^{(m:n)+\varepsilon} p^m \times (1-p)^{n-m} \times q_p \, dp}{\int_0^1 p^m \times (1-p)^{n-m} \times q_p \, dp}.$$

If all q-values are *equal*, the values q_p cancel out from these expressions and we get simpler expressions corresponding to (A') and (B') above

$$(C')\qquad \frac{p^m \times (1-p)^{n-m}}{\int_0^1 p^m \times (1-p)^{n-m} \, dp}$$

and

$$(D')\qquad \frac{\int_{(m:n)-\varepsilon}^{(m:n)+\varepsilon} p^m \times (1-p)^{n-m} \, dp}{\int_0^1 p^m \times (1-p)^{n-m} \, dp}.$$

(C') has its maximum value for *max.* $p^m \times (1-p)^{n-m}$ which is reached when $p = m{:}n$. (The Inverse Law of Maximum Probability.)

If q_p is not 0 for all values of p in the interval $m{:}n \pm \varepsilon$, then (D) increases with n and approaches 1 as a limit. (The Inverse Law of Great Numbers.)

The inversion of Bernoulli's Theorem is also known under the name of Bayes's Theorem.[7]

The first part of Bayes's Theorem or the Inverse Law of Maximum Probability depends on an assumption of equal *a priori* probabilities.

The second part of Bayes's Theorem or the Inverse Law of Great Numbers requires only the much weaker assumption that it is not *a priori* infinitely improbable that the event's Bernoullian probability deviates by at most ε from its actual relative frequency. One might say that this assumption means that the increase in *a posteriori* probability towards 1 is 'practically independent' of *a priori* probabilities.[8]

Thus, loosely speaking, the first part of Bayes's Theorem tells us that, if all values of an event's probability are *a priori* equally likely then it is *a posteriori* most likely that the event's probability is as indicated by its relative frequency. And the second part tells us that, 'practically independently' of *a priori* probabilities, it becomes 'in the long run' infinitely probable that the event's probability will equal its actual relative frequency. The danger of using this loose mode of speech is, among other things, that it leaves without mention the assumptions of independence needed in order to warrant the Bernoullian character of the event's probability.

From Bayes's Theorem we easily reach another famous principle of Inverse Probability known as Laplace's Law of Succession.[9] We raise the following question:

If an event has occurred on all of n occasions, what is the probability that the event will occur as well on the next occasion?

It is taken for granted that the formula (C) above can be used for calculating the probability that, if the event has occurred on all of n occasions, its Bernoullian probability will be p. The Special Multiplication Principle is then used for calculating the probability that the Bernoullian probability is p *and* the event occurs on the next occasion as well. Finally, use of the Special Addition Principle is made to 'add up', p passing through all values from 0 to 1, all these conjunctive probabilities. Thus we get for the calculated probability the value

$$(E) \qquad \frac{\int_0^1 p^{n+1} \times q_p dp}{\int_0^1 p^n \times q_p dp}.$$

On the assumption that all the initial probabilities q_p are equal, we get the simplified expression

$$(E') \qquad \frac{\int_0^1 p^{n+1}dp}{\int_0^1 p^n dp}.$$

By integration we obtain for (E') the 'famous' value $\frac{n+1}{n+2}$ for the probability that, if the event has occurred n times in succession, it will occur on the next occasion as well.[10]

Mention should be made of a version of the Law of Succession which is independent of the assumption of equality in the initial probabilities and obtainable without the use of integration.

Let there be s Bernoullian independence-realms for a certain event. Let the Bernoullian probabilities be $p_1 \ldots p_s$ and the corresponding initial probabilities $q_1 \ldots q_s$. We assume that $q_1 < \ldots < q_s$.

Now suppose that the event has occurred n times in succession. We choose an arbitrary value ε such that at least one of the Bernoullian probabilities falls in the interval $1-\varepsilon$. Formula (B) may be used for calculating ths probability that the n occurrences belong to some independence-realm in which the Bernoullian probability of the event falls in the interval $1-\varepsilon$. It follows from the Inverse Law of Great Numbers that the calculated probability is the greater, the greater is n (provided that not all the initial probabilities, which correspond to the Bernoullian probabilities in the interval $1-\varepsilon$, are 0). For a sufficiently large n it is more probable that the occurrences belong to an independence-realm, in which the Bernoullian probability of the event falls inside this interval, than that they belong to a realm in which the Bernoullian probability falls outside the interval. And 'in the long run' it becomes infinitely probable that the n occurrences belong to some such independence-realm.

We raise the question: What is the probability that the event, which has occurred n times in succession, will occur on the next occasion as well? This value we may calculate from (A) with the aid of the Special Multiplication and Addition Principles in a manner exactly similar to the derivation of the formula (E) from the formula (C). We get the formula:

$$(F) \qquad \frac{\sum_{i=1}^{s} p_i^{n+1} \times q_i}{\sum_{i=1}^{s} p_i^{n} \times qi}.$$

This formula can be expanded as follows: $p_1 \times \frac{p_1^n \times q_1}{\sum_{i=1}^{s} p_i^n \times q_i} + \ldots + p_s \times \frac{p_s^n \times q_s}{\sum_{i=1}^{s} p_i^n \times q_i}$. We have $p_1 < \ldots < p_s$. Since we can always choose ε in such a way that p_s is the only p-value which falls in the interval 1-ε, it follows from the Inverse Law of Great Numbers that $\frac{p_s^n \times q_s}{\sum_{i=1}^{s} p_i^n \times q_i}$ increases with n (provided q_s is not 0). For a sufficiently large n $\frac{p_s^n \times q_s}{\sum_{i=1}^{s} p_i^n \times q_i}$ is greater than any other term $\frac{p_i^n \times q_i}{\sum_{i=1}^{s} p_i^n \times q_i}$. From arithmetical considerations of an elementary nature now follows that (F) increases with increasing n.[11] In other words, it has been proved that, relative to the assumption mentioned, the probability that an event which has occurred n times in succession will occur on the next occasion as well, increases with n. We may call this the Non-Numerical Law of Succession.[12]

On the assumption that all initial probabilities are equal, (F) reduces to

$$(F') \qquad \frac{\sum_{i=1}^{s} p_i^{n+1}}{\sum_{i=1}^{s} p_i^{n}}.$$

Let us assume that $p_1 = 0$ and $p_s = 1$ and that the difference between any two successive p-values is always the same. The interval from 0 to 1, in other words, is divided by the p-values in $s-1$ subintervals of equal length. On these assumptions it may be shown that, with increasing s, the value of (F') approaches the value $\frac{n+1}{n+2}$ as a limit. Thus, on the assumption of equal probabilities *a priori*, the value given by the Non-Numerical Law of Succession and obtained without the use of integration approaches as a limit the value given by the Numerical Law of Succession, which is obtained by the use of integration.[13]

§4. *Criticism of Inverse Probability.*

The doctrine of Inverse Probability is also known as a doctrine of 'inductive' probability or of the probability of 'hypotheses' or 'causes'. It is not difficult to see the reasons for these names:

Consider first the Inverse Principle or *T10-T12* above. It is sometimes natural to speak of the mutually exclusive and jointly exhaustive alternatives $b_1 \ldots b_s$ as 'causes' of the 'event' a, and of the assumption that an occurrence of a is due to a specific one of these 'causes' as an 'hypothesis'.[1] On each 'hypothesis' the event possesses a certain likelihood, and each 'hypothesis' has itself a certain initial or *a priori* probability of being true. Given these likelihoods and initial probabilities we use the Inverse Principle to calculate the probability that a given occurrence of a is to be explained by the 'hypothesis' that it is due to a specific 'cause' b_i. If any one 'cause' ('hypothesis') has the same initial probability of being operative (true), then the most probable 'cause' ('hypothesis') is the one which gives to the event the greatest probability.[2]

In a similar manner one may speak of 'cause' and 'hypothesis' in connection with Bayes's Theorem.[3] The 'cause' is here the event's membership of a certain Independence-Realm, i.e. the 'cause' consists in the presence of certain conditions under which the event will occur with a certain (Bernoullian) probability. The 'hypothesis' again is *that* those conditions are satisfied, i.e. *that* the occasions for the event's occurrences belong to a certain Independence-Realm.

Very often one has talked of the 'hypotheses' involved in Bayes's Theorem as propositions to the effect that the event possesses a certain (Bernoullian) probability. This loose mode of speech is seriously misleading. (*Vide infra.*)

The Law of Succession was traditionally regarded as a rule for estimating the probability of future events relative to past experience. It has been said that no formula in the alchemy of logic has exercised a more powerful fascination over the human mind.[4] Not only was it uncritically accepted by those, who followed closely in the footsteps of Laplace. Many respectable authors of the nineteenth century on the subject of induction regarded it as an altogether sound formula, whereby to judge with probability of the future course of events.[5] Some put it to the wildest uses, such as calculating that the sun will rise tomorrow or that it will continue to rise regularly for the next 1000 years.[6] The formula still finds favour with authors, who regard the doctrine of inverse probability as being, with due qualifications, tenable.[7]

Criticism of Inverse Probability in general and of the Law of Succession in particular is historically connected with the criticism, mainly by early proponents of the frequency view, of the Laplacean definition of probability and of uncritical use of the Principle of Indifference. Among the early critics Boole, Peirce, and Venn should be mentioned.[8] Keynes took a guarded and in many respects sound view of Inverse Probability, but in his criticism of the Law of Succession he went too far when charging the law with a contradiction.[9]

In recent times Inverse Probability has been severely criticized by one of the champions of modern statistical science, R. A. Fisher. He rejects the entire doctrine as theoretically unsound and as useless in practice.[10] Fisher's criticism of the theoretical foundation, however, is not in every way clear and convincing. The subject is still open to controversy.[11]

Here we shall have to content ourselves with a number of critical observations which no attempted rehabilitation of Inverse Probability will be able to 'get round'.

(1) In the Inverse Principle (*T10-T12*) three factors may be said to be involved. These are the individual propositions a and h and the set of propositions b_i. It is convenient to speak of a as a proposition to the effect that a certain event has occurred, and of each proposition b_i as some condition, under which the event will take place with a certain probability. We may, for present purposes, ignore h.

That the probability of the event relative to conditions b_i is p_i is a probability-proposition. But the so-called *a posteriori*-probability which is calculated by means of the Inverse Formula, cannot properly be described as the probability of a probability-proposition ('second order probability'). The calculated probability is the probability that the conditions, under which an occurrence of the event took place, were just the conditions b_i. The unknown of the problem, therefore, *is not a probability.* It is *the presence of certain conditions*, relative to which the event has a probability. The 'inverse problem' in all its variations can be described as a problem of *re-identification* of the conditions under which an event has occurred, these conditions constituting a 'field of measurement' (data, information) of the event's probability.

When from the Inverse Principle we pass on to Bayes's Theorem we conceive of the event a of the Inverse Principle as a complex event, consisting of m occurrences of an event E on n occasions. The complex event a is, as in the Inverse Principle, supposed to have taken place under some condition(s) b_i. This b_i is, moreover, supposed to be a Bernoullian independence-realm for the occurrence of E. In other words: given that the condition b_i is satisfied, the event E will occur with a certain Bernoullian probability p_i. On this supposition it is then possible to calculate also the probability that, given b_i, the complex event a will occur. The 'inverse problem' now consists in determining the probability that the conditions, under which an occurrence of the complex event a took place, were exactly the conditions b_i. It is thus a problem of 'identifying', with probability, an independence-realm for E.

(2) What has so far been said goes a long way towards explaining certain limitations in the applicability of inverse probability to concrete cases. Let us first consider to what type of situation inverse formulae *may be* applied:

Let there be a number of urns containing black and white balls. With each urn we associate a certain probability of drawing a black ball. (*How* we have come to associate these probabilities with the respective urns is for present purposes quite irrelevant: it might have been on the basis of knowledge of the proportions of black and white balls in the various urns, or it might have been on the basis of the results in long series of drawings from the urns.) Now n drawings, with replacement, are made from one of the urns and we get exactly m balls. We do not know which probability of drawing a black ball has been associated with this particular urn. The inverse problem before us is now to use the information obtained from the drawing to 'identify' the urn as being one associated with a certain Bernoullian probability p_i of drawing a black ball.

Any legitimate application of inverse probability has to be essentially analogous to this case as regards the problem of identifying the independence-realm. And from it follows at once that such uses of inverse probability as those of determining the probability that the sun will rise tomorrow or that the next raven to be observed will be black are illegitimate. The possibility of identifying birds as ravens, independently of observations concerning their colours, makes the suggested application of the Law of Succession lose its point. For, it is a part of the data on which this law would rest, when applied to birds and their colourings, that being a raven is associated with a certain (Bernoullian) probability p of being black. This value p is the probability that any random bird, which is known to be a raven, will be black — and hence also that the next raven to be observed will be black. There is, however, a question to which the Law of Succession could be quite sensibly applied here, although this question would hardly arise in practice. It is the following: What is the probability that the next member to be observed in a set of (one and the same) unknown species of bird will be black, given that all the n members of the set which have been so far observed have been found to be black? In calculating an answer to this question, using the Law of Succession, we would have to rely on probabilities p_1, p_2, etc. that a random individual of the species of bird, b_1, b_2, etc. are black. And among these probabilities would also be the probability that a random individual of the species of raven is black.

(3) We have so far said nothing of the *a priori* probabilities — traditionally considered the crux of the doctrine of inverse probability. We have been concerned to show that, independently of the problems connected with the initial probabilities, the doctrine, if applicable at all, is so only to situations of a very peculiar nature. When initial probabilities are considered too, further severe restrictions to the applicability of the formulae become apparent.

The *a priori* probabilities are the probabilities, relative to some piece of information h, that the respective conditions b_i, under which the event a may or may not occur, are satisfied. If h is of the form $h \vee \sim h$, the initial probabilities may be said to be 'eminently' *a priori*, i.e. subsisting relative to no particular information.

To many of the notorious uses of the Inverse Formula for determining the probability of some 'causes' or 'hypotheses', and to any use of the Inverse Law of Maximum Probability and the Numerical Law of Succession, it is essential that the *a priori* probabilities involved in the problem under consideration should be *equal*.

Traditionally, the needed equality of the *a priori* probabilities was regarded as a consequence of a Principle of Indifference. In short: ignorance of the initial probabilities was considered a sufficient condition of their equality.[12] We shall not criticize this deduction of knowledge from ignorance here. It seems to us that a Principle of Indifference may be legitimately invoked as a ground for framing a *hypothesis* about the equality of certain initial probabilities, but that use of this principle can never amount to a *proof* of the equality in question.[13]

It is difficult to see, how any assumption about the *a priori* probabilities — be it about their equality or inequality or exact numerical values — could be anything but a hypothesis for the correction of which future experience about the case under consideration may constitute a reason. This hypothetical nature of the initial probabilities already destroys the faith which the 'classical' doctrine put in the power of inverse probability to justify induction.

(4) In order to reach that level of the doctrine of Inverse Probability, where Bayes's Theorem and the Law of Succession belong, it is necessary to assume that the conditions under which the event in question may occur or fail to occur constitute Bernoullian independence-realms for the event. These assumptions of independence are of a sweeping nature. Their problematic character was practically never noticed in the traditional doctrine. It is difficult to see, how the truth of these assumptions could be established on *a priori* grounds. The assumptions of independence may, with the assumptions about initial probabilities, be said to form part of the *hypothetical* framework of any significant use of Bayes's Theorem and the Law of Succession.

(5) In the case of those formulae of inverse probability which employ integration, an additional difficulty enters. They presuppose

that the conditions under which the event may occur or not, constitute not only an infinite, but a not-denumerable manifold. This presupposition can never be empirically satisfied. It is an 'idealization', the legitimacy of which — at least in the opinion of the present writer — remains problematic even in those cases where all the other conditions for a legitimate use of the inverse principles are fulfilled.

We do not think that the question of the legitimacy of this 'idealization' can be settled simply by reference to an analogy with other applications of mathematical formulae involving integration to cases in nature. For it is not clear to what extent use of integration in the doctrine of inverse probability is, from the point of view of application, *peculiar* and to what extent it is *analogous* to such other cases.

It is not a correct presentation of the nature of the case to say that the 'idealizing' assumption is the assumption that the possible values of a certain event's (Bernoullian) probability continuously cover the range from 0 to 1 inclusive. What we have to say is that the alternative conditions under which an event may occur — e.g. the alternative urns from which the drawing of balls takes place — form a manifold within which all possible values in the interval from 0 to 1 of a certain (Bernoullian) probability are represented. It is not *prima facie* obvious that this assumption — even as an 'idealization' — makes any sense at all.

§5. *Confirmation and probability.*

Let a be some proposition and h some logical consequence of it. Deductive logic studies the relation of *entailment* between a and h. Inductive logic, one might say, studies the degree of *confirmation* or *support* which h gives to a. (We do not mean to say that this is the only task of inductive logic.)

By the hypothetical method or use of hypothesis[1] is often meant the deduction and subsequent verification of consequences of an assumed (usually general) proposition. It is a major task of inductive logic to study the way in which verified consequences 'inductively' affect the hypothesis from which they deductively follow.

By Confirmation-Theory we shall here understand the theory of how the probability of a given propositon is affected by evidence in the form of propositions which are logical consequences of it. A case of particular importance to this theory is when the given proposition is a generalization and the evidence for it is some of its instances. Verified instances *confirm* the generalization. It is a primary task of Confirmation-Theory to evaluate, in terms of probability, the confirming effect of the instances on the generalization. The notion of an 'instance' of a generalization will have to be discussed somewhat more in detail later. For present purposes it suffices to lay down merely that an 'instance' of a generalization is a logical consequence of it.

The doctrine of Inverse Probability, which we have examined in the two preceding sections, is not a Confirmation-Theory in the sense here understood. The same is true, moreover, of some recent investigations which call themselves theories of confirmation.[2]

The creation of a Confirmation-Theory for inductive generalizations is of comparatively recent date. The theory was founded by C. D. Broad (1918) and J. M. Keynes (1921). After Keynes there has been very little further development of the theory, but a certain amount of dicussion of the significance of his achievement.

We prove the following elementary Lemma which is of basic importance both to Confirmation-Theory and to the Theory of Scope to be discussed later:

Lemma. The probability of a proposition, on given data, is smaller than or at most equal to the probability, on those same data, of any of its logical consequences.

Proof: Let h be the datum, and let a entail b. In virtue of the General Multiplication Principle (*A2*) we have $P(a\&b/h) = P(b/h) \times P(a/h\&b)$. Since a entails b, $a\&b$ is logically identical with a. Thus we have $P(a/h) = P(b/h) \times P(a/h\&b)$. Since probabilities are in the interval from 0 to 1 inclusive, it follows that $P(a/h) \leqslant P(b/h)$.

Let g be a generalization. Let $i_1 \ldots i_n \ldots$ be confirming instances of it. Let I_n be the conjunction $i_1 \& \ldots \& i_n$ of the first n instances of g. Let e, finally, be some piece of evidence or information, relative to which we may estimate the probability of g and of I_n.

Let p be the probability of g given e. Thus we have $P(g/e) = p$. We shall call p the initial or *a priori* probability of the generalization.

Let p_1 be the probability of i_1 given e ; p_2 the probability of i_2

given e *and* i_1 ; etc. Thus we have $P(i_n/e\&I_{n-1}) = p_n$. We shall call the values p_n the eductive probabilities of the instances of the generalization.

Given the eductive probabilities we can use the General Multiplication Principle to calculate the probability of I_n given e, i.e. the probability of n successive confirmations of the generalization. Let Π_n be the product $p_1 \times \ldots \times p_n$. Then we have $P(I_n/e) = \Pi_n$.

Let q_n be the probability of g given e *and* I_n. Thus we have $P(g/e\&I_n) = q_n$. We shall call the values q_n the probabilities *a posteriori* of the generalization.

From the General Multiplication Principle follows $P(g\&I_n/e) = P(I_n/e) \times P(g/e\&I_n)$.

Since the generalization entails (the conjunction of) its confirming instances, it follows from our Lemma above that $p \leqslant \Pi_n$.

From this immediately follows that, if $p > 0$, then $\Pi_n > 0$. And if $\Pi_n > 0$, we can transform the last identity above to $P(g/e\&I_n) = \dfrac{P(g/e)}{P(I_n/e)}$, i.e. to $q_n = \dfrac{p}{\Pi_n}$. ($g\&I_n$ is logically identical with g alone).

Now compare q_n and q_{n+1}, i.e. compare $\dfrac{p}{\Pi_n}$ and $\dfrac{p}{\Pi_{n+1}}$. It is evident that, if $p > 0$ and $\Pi_n > \Pi_{n+1}$, then $q_n < q_{n+1}$. But, since probabilities are in the interval from 0 to 1 inclusive, it follows that $\Pi_n > \Pi_{n+1}$, if and only if $p_{n+1} < 1$.

Herewith has been proved that, if $P(g/e) > 0$ and $P(i_{n+1}/e\&I_n) < 1$, then $P(g/e\&I_{n+1}) > P(g/e\&I_n)$. In words:

If the initial probability of a generalization is not minimal, its a posteriori *probability increases with each new confirmation which is not maximally probable relative to the previous confirmations.*

(It should be observed that in this formulation of the theorem in words no mention is made of e. The choice of e, as we shall see presently, is crucial to the 'meaning' of the theorem.)

We shall call this the Principal Theorem of Confirmation.

Consider the difference $\dfrac{p}{\Pi_{n+1}} - \dfrac{p}{\Pi_n}$. It can also be expressed in the form $\dfrac{p}{\Pi_n}\left(\dfrac{1}{p_{n+1}} - 1\right)$. It is seen that it is inversely proportionate to the value of p_{n+1}. Thus we have the theorem:

The smaller the eductive probability of the confirming instance, the greater is its contribution to the increase in the a posteriori *probability of the generalization.*

The ideas contained in these two theorems, viz. the ideas that the probability of a generalization increases with the number of confirming instances and that it increases the more, the more improbable, i.e. surprising and unexpected, are the confirmations, can truly be regarded as belonging to the 'classical' ideas of the theory of induction and probability.[3]

The next question to be raised is, whether the increasing probability of a generalization tends towards a limit and, particularly, whether this limit is the maximum value 1.

On this question divergent opinions have been expressed. Keynes wanted to show that the increasing probability approaches 1. His proof makes use of extra-logical assumptions about the constitution of the universe.[4] Nicod tried to show that Keynes's use of his assumptions was based on an error, and that the increasing probability *cannot* be proved to approach 1.[5]

The condition of approach to maximum probability is easily stated. From the formula $q_n = \frac{p}{II_n}$ it follows that q_n approaches 1, if II_n approaches p. In words:

The probability *a posteriori* of a generalization approaches 1, if the probability of n successive confirmations of the generalization approaches its probability *a priori.*

By virtue of *A2* we have $P(\sim g \& I_n/e) = P(\sim g/e) \times P(I_n/e \& \sim g) = P(I_n/e) \times P(\sim g/e \& I_n)$. By virtue of *A1* we have $P(\sim g/e) = 1-p$ and $P(\sim g/e \& I_n) = 1 - \frac{p}{II_n}$. We also have $P(I_n/e) = II_n$. Substituting these values in the equation $P(\sim g/e) \times P(I_n/e \& \sim g) = P(I_n/e) \times P(\sim g/e \& I_n)$, we get $P(I_n/e \& \sim g) = \frac{II_n - p}{1-p}$, on condition that $1-p>0$.

(This last condition is trivial. For, if $1-p=0$, then $p=1$, which means that the generalization is already *a priori* maximally probable. And in this case its probability can no longer be increased by confirmation.)

If now II_n approaches p, then $P(I_n/e \& \sim g)$ approaches 0, and conversely. Thus we get a new condition of convergence, which can be formulated in words as follows:

The probability *a posteriori* of a generalization approaches 1, if the probability of n successive confirmations, on the assumption that the generalization is false, approaches 0.[6]

The thought that the probability of n successive confirmations approaches as a limit the probability *a priori* of the generalization is intuitively plausible, if we think of the generalization as a *conjunction* of its confirming instances. If the generalization is 'genuine', i.e. numerically unrestricted, this conjunction is (at least 'potentially') infinite. In symbols:

$$(1) \qquad i_1 \& \ldots \& i^n \& \ldots \textit{ad inf.} =_{Df} g.$$

A shorter way of expressing the same thought symbolically is

$$(2) \qquad \lim_{n \to \infty} I_n =_{Df} g.$$

Here we have a case of a sequence of propositions approaching another proposition as a limit.[7] (The sequence is the sequence of *conjunctions* I_n.) It is 'natural' to think that, under such circumstances, the probabilities associated with the members of the sequence of propositions will converge towards the probability associated with the limiting-proposition. The probabilities associated with the members of the sequence are the values II_n. The probability of the limiting-proposition is p. In view of (2) it is therefore 'natural' to think that we also have:

$$(3) \qquad \lim_{n \to \infty} II_n = p.$$

The 'naturalness' of this thought, moreover, has nothing to do with assumptions about the structure of the universe. It is of a logico-mathematical character and connected with the conception of the generalization as an infinite conjunction. But we cannot *prove* this idea from the axiom system of probability, presented in the first section of the present chapter. In order to prove it we have to add to the system a new axiom to the effect that, if a sequence of propositions approaches a given proposition as a limit, then the probabilities associated with the members of the sequence approach as a limit the probability associated with the given proposition. (It is assumed that the probabilities are throughout taken relative to the same data.) In symbols:

A4. If $\lim_{n \to \infty} a_n = a$, then $\lim_{n \to \infty} P(a_n/h) = P(a/h)$.[8]

From this axiom and the identity (2) above we can prove that the probability of n successive confirmations of a generalization approaches as a limit the probability *a priori* of the generalization and that hence *the probability a posteriori of the generalization approaches as a limit the maximum value* 1.

The above argument, we believe, is the right and conclusive answer to the Keynes-Nicod dispute over the question, whether or not the increasing probability of a confirmed generalization approaches maximum probability as a limit.

It is important to stress that the convergence towards maximum probability does not follow from the fact alone that the generalization is confirmed in an indefinite number of instances ('indefinitely confirmed'). It must also be the case that the confirmations cover the *whole* range of instances of the generalization, and not only some infinite sub-class of this range. From the sequence $i_1, \ldots, i_n, \ldots$, assumed to consist of *all* instances of g, we may select every second member i_1, i_3, i_5, etc. and thus obtain another infinite sequence of confirming instances of g. But there can be no assurance (of a logico-mathematical nature) that indefinite confirmation of the generalization through *these* instances will make the probability of the generalization approach 1 as a limit.[9]

Lastly, it should be noted that the proof of our Principal Theorem does not require the use of principles of Inverse Probability.

§6. *The Paradoxes of Confirmation.*

It is a major task of Confirmation-Theory to make the notion of a (confirming) instance of a generalization precise. Of this notion we have so far only said that the confirming instance must be a logical consequence of the generalization. The further clarification of the concept turns out to involve difficulties.[1]

We shall here briefly consider some of these difficulties. Attention will be confined to the simplest case only, viz. to Universal Generalizations of the form $(x)\ (Ax \rightarrow Bx)$.

First a certain ambiguity in the term 'instance' should be noted. The term can be used to refer only to instances which confirm the generalization, or it can be used to refer to instances which either confirm or disconfirm it. Only if 'instance' means 'confirming instance' is it true that the instance is entailed by the generalization.

In speaking about instances we have so far always meant confirming instances. For present purposes, however, it is convenient to take 'instance' in the broader sense and distinguish between instances which are confirming and instances which are disconfirming of the generalization.

Anything which is A but is not B disconfirms (falsifies) the general proposition (x) $(Ax \rightarrow Bx)$. By a disconfirming instance of (x) $(Ax \rightarrow Bx)$ we shall therefore understand any *proposition* of the form $Ax \& \sim Bx$. And we shall say of the *thing* x that it 'affords' or 'constitutes' the disconfirming instance (disconfirmation).

The notion of a disconfirming instance raises no problem. Not so the notion of a confirming instance. As regards its definition several possibilities are open.

One possibility is to define a confirming instance as that which is *not* a disconfirming instance. Or, more precisely, as the contradictory of a disconfirming instance. In the case of (x) $(Ax \rightarrow Bx)$, the general form of the confirming instance would thus be $\sim(Ax \& \sim Bx)$. But $\sim(Ax \& \sim Bx)$ is the same proposition as $Ax \rightarrow Bx$ and also the same as $Ax \& Bx \vee \sim Ax \& Bx \vee \sim Ax \& \sim Bx$. Thus, if the notions of a disconfirming and a confirming instance are contradictories, then we shall have to say that the generalization that all A are B is confirmed by (*i*) anything which is both A and B, (*ii*) anything which is not A but is B, and (*iii*) anything which is neither A nor B.

This way to define a confirming instance, however, leads to two 'paradoxes'. These are variants of the well-known Paradoxes of Implication. The first 'paradox' is that anything which is *not* A will constitute a confirming instance of (x) $(Ax \rightarrow Bx)$, irrespective of whether it is B or not. And the second 'paradox' is that anything which *is* B will likewise constitute a confirming instance, irrespective of whether it is A or not. Thus for example any swan, whether white or not, would serve to confirm the proposition that all ravens are black. And the same holds good for any black object, whether a raven or not. Such consequences as these plainly conflict with our intuitive notions of what it is for a state of affairs to 'confirm' a generalization.

Another possibility is to look for a narrower definition of a confirming instance. To this end one might suggest that only things which are both A and B are really confirmative of the generalization that all A are B. The general form of the confirming instance of $(x)\ (Ax \rightarrow Bx)$ would thus on this proposal be $Ax\&Bx$. This idea is sometimes called the Nicod Criterion.[2]

But this definition, too, leads to a 'paradox'.

If the general form of a confirming instance of $(x)\ (Ax \rightarrow Bx)$ is $Ax\&Bx$, then, by substituting $\sim B$ for A and $\sim A$ for B, it follows that the general form of a confirming instance of $(x)\ (\sim Bx \rightarrow \sim Ax)$ must be $\sim Bx\&\sim Ax$ which is the same as $\sim Ax\&\sim Bx$. This piece of reasoning seems quite unobjectionable.

Now $(x)\ (Ax \rightarrow Bx)$ and $(x)\ (\sim Bx \rightarrow \sim Ax)$ are logically equivalent. It is highly plausible to think that anything which counts as a confirming instance of a certain proposition should also count as a confirming instance of any propositon which is logically equivalent with the first. This idea may be called the Equivalence-Criterion (-Condition).[3]

If we accept the Equivalence-Criterion, we should thus have to reject the proposal that $Ax\&Bx$ is the sole general form of a confirming instance of $(x)\ (Ax \rightarrow Bx)$. We should have to recognize that $\sim Ax\&\sim Bx$ is another form of confirming instance.

On the other hand, to regard a proposition of the form $\sim Ax\&\sim Bx$ as a confirmation of the generalization that all A are B seems not to accord very well with our 'intuitions' in the matter. We would not normally regard the fact that this particular object, say a swan, is neither a raven nor black as a confirming instance of the proposition that all ravens are black. And we *would* admit that the fact in question confirms the generalization that all things which are not black are not ravens.

This conflict between two 'intuitions', the obvious plausibility of the Equivalence-Criterion on the one hand and the reluctance to accept $\sim Ax\&\sim Bx$ as a confirming instance of $(x)\ (Ax \rightarrow Bx)$ on the other hand, has been called a Paradox of Confirmation.[4]

Of those 'paradoxes' which are special cases of the Paradoxes of Implication it is easy to show that they are 'harmless' in the sense that paradoxical confirmations of a generalization cannot influence (increase) the generalization's probability by virtue of the Principal Theorem of Confirmation. The fact that this particular animal is a swan *cannot* effect the probability that all ravens are black. This is seen from the following considerations:

The proposition $Ax_n \rightarrow Bx_n$ logically follows from the proposition $\sim Ax_n$ and also from the proposition Bx_n. If therefore, in estimating the probability of $(x)\ (Ax \rightarrow Bx)$ relative to verified propositions of the form $Ax_n \rightarrow Bx_n$, it is part of our data that an object x_n does not possess the property A or that it possesses the property B, then it is certain and hence also maximally probable that this object will verify the proposition $Ax_n \rightarrow Bx_n$. Expressed in our symbolism above: If e contains either $\sim Ax_n$ or Bx_n, i.e. if e is identical with $e' \& \sim Ax_n$ or $e' \& Bx_n$, then, writing i_n for $Ax_n \rightarrow Bx_n$, and I_{n-1} for $(Ax_1 \rightarrow Bx_1)$ $\& \ldots \& (Ax_{n-1} \rightarrow Bx_{n-1})$, we have by virtue of *T2* (p. 93) $P(i_n/e \& I_{n-1}) = 1$. And this means that i_n, i.e., $Ax_n \rightarrow Bx_n$, does not satisfy the condition which is necessary if its verification is to contribute to increase in the generalization's probability.

'Paradoxical' confirmations of a universal implication, afforded by things which are either known not to satisfy the antecedent or known to satisfy the consequent, are thus 'harmless', i.e. they do not affect the probability of the general proposition. Instead of saying that they are 'harmless' one may also say that they are 'valueless' for confirmation, have no 'confirmatory value'.

It seems to us that the solution to the 'paradox' which results from the conflict between the Nicod- and Equivalence-Criteria has to be sought along the following lines:

We should start by questioning the validity of the Nicod-Criterion. Is it necessarily the case that anything which is both A and B genuinely affords a confirmation of the law that all A are B? It seems to us that the answer is quite certainly negative.

Whether the fact that a thing which is both A and B is a genuine or a paradoxical confirmation of the law that all A are B depends upon *the way in which this fact becomes known to us*. If we know that the thing is A but do not know whether it is or is not B, then it will be of interest, from the point of view of testing the law in question, to find out whether it is B or not. If the thing is found not to be B, the law is falsified, and *therefore*, if the thing is found to be B, the law is confirmed. 'Genuinely confirmed', one might say, means the same as 'saved from falsification after having stood the risk'. But if we know that the thing is *B* and do not know whether it is or is not *A*, then it will be of no interest, from the point of view of confirmation, to find out whether it is *A* or not. For in neither case would the law be falsified and *therefore* not (genuinely) confirmed either. 'Paradoxically confirmed', one could say, means 'confirmed under circumstances which involve no risk of falsification'.

Thus the fact that a thing is both *A* and *B*, genuinely confirms the law that all *A* are *B* only if, on first knowing that the thing is *A*, we subsequently verify that it is *B*.

Next we raise the following question: Is it necessarily the case that nothing which is neither *A* nor *B* genuinely affords a confirmation of the law that all *A* are *B*? It seems to us obvious that the answer to this question, too, is negative. If we know that a thing is not *B* but do not know whether it is or is not *A*, then showing that it is not *A* saves the law that all not-*B* are not-*A* and thus also the law that all *A* are *B* from falsification, and can *therefore* be truly regarded as confirmatory of either law. But if we know that a thing is not *A* but do not know whether it is or is not *B*, then showing that it is not *B* saves neither the law that all not-*B* are not-*A*, nor the law that all *A* are *B* from falsification and *therefore* does not genuinely confirm either of them.

Thus not *any* confirmation of the law that all ravens are black through an object which is neither black nor raven can be rightly called 'paradoxical'. It is paradoxical only if, prior to knowing anything about the object's colour, we know that it is not a raven. Then the object is valueless for the purposes of confirming that all ravens are black and also for confirming that all not-black things are not-ravens. But if, prior to knowing anything about the object's membership of a certain species of bird, we know that it is not black, then there is no 'paradox'. The object is then of value as affording a potential confirmation both of the law that all ravens are black and of the law that all not-black things are not-ravens.

The conclusion is that the Equivalence-Criterion is sound and that the 'paradox' arises from the Nicod-Criterion alone. It is a mistake to believe that everything which is both *A* and *B* constitutes a genuine confirmation of the law that all *A* are *B*. And it is equally a mistake to believe that nothing which is neither A nor B could constitute a genuine confirmation of the law that all A are B. Whether something which is both A and B genuinely confirms the law or not, depends upon whether the fact that this thing is B, is or is not known prior to knowledge of the fact that it is both A and B. Similarly, whether something which is neither A nor B paradoxically confirms the law or not, depends upon whether the fact that this thing is not A, is or is not known prior to knowledge of the fact that it is neither A nor B. But anything which genuinely confirms the law that all A are B also genuinely confirms the law that all not-B are not-A. And anything which paradoxically confirms the first law also paradoxically confirms the second. For they are one and the same law.

§7. *Confirmation and elimination.*

Does the Principal Theorem of Confirmation mean that induction by the multiplication of instances, sometimes also called Pure Induction, possesses a value independently of induction by elimination?

Keynes tried to show that the condition $P(i_{n+1}/e\&I_n)<1$, mentioned in the Theorem, is satisfied only if the thing affording the $n+1th$ confirming instance differs in at least one property from all the previous things affording confirmations of the generalization. In his argument he makes a rather dubious use of the principle known as the Identity of Indiscernibles. He interpreted his result as meaning that the contribution of confirming instances to the probability of laws really is rooted in their contribution to the elimination of concurrent possibilities.[1]

In opposition to the view of Keynes, Nicod made an attempt to defend the value of Pure Induction independently of elimination.[2] He pointed out some errors and insufficiencies in Keynes's arguments, but he cannot be said to have been successful in vindicating his own position.[3]

It is hardly possible to settle the Keynes-Nicod controversy over the value of Pure Induction without resort to a *model* of the abstract notion of probability as it occurs in the Principal Theorem. An adequate model is provided in the following way:[4]

Let g be the Universal Generalization $(x)\,(Ax \rightarrow Bx)$. We replace B by a variable X. Thus we get a propositional function $(x)\,(Ax \rightarrow Xx)$.

It is satisfied by any property which is a necessary condition of A. By Φ we shall understand the class of all necessary conditions of A. The generalization g states that B is such a condition. Thus g can also be expressed symbolically by $\Phi(B)$.

Let e be of the form $\Psi(B)$, i.e. let e be a proposition to the effect that the property B is one of a certain class of properties Ψ.

The confirming instances i_1, i_2, etc. of g are the propositions $Ax_1 \rightarrow Bx_1$, $Ax_2 \rightarrow Bx_2$, etc.

That a property is *co-present* with the property A in a thing is to mean that it is not the case that A is present but the property in question absent in this thing.[5]

Consider the propositional function $Ax_1 \rightarrow Xx_1$, which is obtained from i_1 by replacing B by the variable X. By φ_1 we shall understand the class of all properties which satisfy this propositional function, i.e. the class of all properties which are co-present with A in the thing x_1. Similarly, we define φ_2, φ_3, etc. By Φ_n we understand the conjunction (logical product) of classes $\varphi_1 \& \ldots \& \varphi_n$. Thus Φ_n is the class of properties which are co-present with A in *all* (every one of) the things x_1 and . . . and x_n.

$P(g/e)$ or $P(\Phi(B)/\psi(B))$ is the probability that B will be a necessary condition of A, *given* that B is a member of the class of properties ψ. That this probability has the value p means in the Frequency-Model: the proportion of necessary conditions of A among all members of ψ is p.

$P(i_n/e\&I_{n-1})$ or $P(\varphi_n(B)/\psi(B)\&\Phi_{n-1}(B))$ is the probability that B will be co-present with A in x_n, *given* that B is a member of ψ *and* B has been co-present with A in every one of the things $x_1 \ldots x_{n-1}$ That this probability has the value p_n means in the Frequency-Model: p_n is the proportion, among all member of ψ which are co-present with A in x_1 and . . . and x_{n-1}, of properties co-present with A in x_n.

$P(I_n/e)$ or $P(\Phi_n(B)/\psi(B))$ is the probability that B will be co-present with A in the first n things, affording confirming instances of g, *given* that B is a member of ψ. That this probability has the value $p_1 \times \ldots \times p_n$ means in the Frequency-Model: this probability is the proportion, among all members of ψ, of properties co-present with A in x_1 *multiplied by* the proportion, among members of ψ co-present with A in x_1, of properties co-present with A in x_2 *multiplied* *by . . . multiplied by* the proportion, among members of ψ co-present with A in x_1 and . . . and x_{n-1}, of properties co-present with A in x_n.

$P(g/e\&I_n)$ or $P(\Phi(B)/\psi(B)\&\Phi_n(B))$ is the probability that B will be a necessary condition of A, *given* that B belongs to a class of properties ψ *and* that B has been co-present with A in every one of the first n things which have afforded confirmations of the law that B is such a condition. That this probability is q_n means in the Frequency-Model that the proportion of necessary conditions of A among members of ψ which are co-present with A in x_1 and . . . and x_n is q_n. That q_n equals the ratio $\frac{p}{\Pi p_n}$ means that the proportion just mentioned equals the proportion of necessary conditions of A among all members of ψ divided by the proportion, among members of ψ, of properties which are co-present with A in x_1 and . . . and x_n.

That q_{n+1} is greater than q_n means in the Frequency-Model that the proportion of necessary conditions of A among members of ψ which are co-present with A in x_1 and . . . and x_{n+1} is greater than the proportion of necessary conditions of A among members of ψ which are co-present with A in x_1 and . . . and x_n. Considering the meaning, in the Frequency-Model, of the ratios $\frac{p}{\Pi p_{n+1}}$ and $\frac{p}{\Pi p_n}$, it follows that two conditions must be fulfilled if q_{n-1} is to be greater than q_n, viz:

(i) p, or the proportion of necessary conditions of A among all the members of ψ, must not be minimal, i.e. must be greater than 0, and

(ii) p_{n+1}, or the proportion, among all members of ψ which are co-present with A in x_1 and . . . and x_n, of properties co-present with A in x_{n+1}, must not be maximal, i.e. must be smaller than 1.

If the class of properties ψ is finite, then (*i*) simply means that there must exist at least one necessary condition of A in ψ, and (*ii*) simply means that, if x_{n+1} is an instance of A, then x_{n+1} must lack at least one property which is co-present with A in x_1 and . . . and x_n. And the latter again means that the $n+1$:th instance must exclude at least one member of ψ, which has not already been excluded, from the possibility of being a necessary condition of A.

If the class ψ is infinite, then (*i*) means that a 'perceptible', i.e. not-minimal, proportion of members of the class must be necessary conditions of A, and (*ii*) means that the $n+1$:th instance must exclude a 'perceptible' proportion of members of the class, which are co-present with A in the n first things affording confirming instances, from the possibility of being necessary conditions of A.

The condition that p or the *a priori* probability of the generalization must be greater than 0 is thus, in the Frequency-Model, tantamount to a Condition of Determinism. And the condition that p_{n+1} or the eductive probability of the confirming instance must be smaller than 1 is tantamount to a Condition of Elimination.

The above considerations started from replacing B in $(x)\ (Ax \rightarrow Bx)$ by a variable X. We might instead have replaced A by a variable. Then we should have got the propositional function $(x)\ (Xx \rightarrow Bx)$. It is satisfied by any property which is a sufficient condition of B. The Universal Generalization states that A is such a condition. The generalization can also be expressed symbolically by $\Phi(A)$.

Let e be of the form $\psi(A)$.

Consider the propositional function $Xx_1 \rightarrow Bx_1$. By φ_1 we now mean the class of all properties which satisfy this propositional function. A member φ_1 is thus a property, of which it is *not* true that it is present in x_1, if B is absent in x_1. Of such a property we shall say that it is *co-absent* with B in (from) the thing in question.[6]

Similarly, we define φ_2, φ_3, etc. By Φ_n we understand the product $\varphi_1 \& \ldots \& \varphi_n$. ($\Phi_1$ thus is the same as φ_1.)

If, in our previous interpretations in frequency terms of the *a priori* probability, of the eductive probabilities, and of the *a posteriori* probability, we substitute the phrase 'sufficient condition(s) of B' for the phrase 'necessary condition(s) of A', and the phrase 'co-absent with B' for the phrase 'co-present with A', then we get another interpretation in terms of frequency of the magnitudes in question. The two conditions of increase in probability now assume the following shape:

(i') p, or the proportion of sufficient conditions of B among all the members of ψ, must not be minimal, and

(ii') p_{n+1}, of the proportion, among all members of ψ which are co-absent with B in x_1 and . . . and x_n, of properties co-absent with B in x_{n+1}, must not be maximal.

As before, the condition $p>0$ is tantamount to a Condition of Determinism, and the condition $p_{n+1}<1$ is tantamount to a Condition of Elimination.

It is clear that, on the interpretation $\Phi(B)$ of g, elimination can take place only if x_{n+1} *has* the property A. For it is only when A is *present* that any other property could be denied to be co-present with it. And similarly it is clear that, on the interpretation $\Phi(A)$ of g, elimination can take place only if x_{n+1} *lacks* the property B. For it is only where B is *absent* that any other property could be denied to be co-absent with it.

This is in harmony with the two basic facts of Elimination-Theory, viz. that when we are in search of a necessary condition of a given property we ought to compare with each other positive instances of that property, whereas when we are in search of a sufficient condition of a given property the elimination is promoted by negative instances of that property. (Cf. above Ch. IV, §4.)

We now also see how the fact that the probabilifying effect of a confirming instance is inversely proportional to the eductive probability of the instance, is reflected in the Frequency-Model. It simply means that the probabilifying effect is inversely proportional to the eliminative effect. The eliminative effect, in its turn, is the greater the more unlike the new confirming instance is as compared with the previous confirming instances. Thus, we increase the probability of a generalization by confirmation, the more effectively, the more we succeed in varying the circumstances under which the generalization is put to successful test.

If the eductive probability of an instance is 1, its eliminative effect is nil. This sheds light upon the Paradoxes of Confirmation. A thing which has the property B cannot eliminate anything from the possibility of being a sufficient condition of B. And a thing which lacks the property A cannot eliminate anything from the possibility of being a necessary condition of A. Such things are therefore necessarily ineffective from the point of view of elimination. This is the counterpart, in the Frequency-Model, to the fact that such things are ineffective from the point of view of producing an increase in probability. But by being necessarily ineffective, the 'paradoxical' confirmations are also 'harmless', they do not 'genuinely' or 'really' confirm the generalization in question at all.

The abstract notion of probability, which figures in the Principal Theorem of Confirmation, can thus be given a model which makes the logical mechanism of the theorem reflect the working of the logical mechanism of induction by elimination.

The possibility of this model may be said to support the view, which Keynes put forward but supported with dubious or false arguments, that the multiplication of instances in induction has a probabilifying effect on the conclusion only in so far as it has an eliminative effect within a field of concurrent possibilities.

Can the model be said definitely to settle the dispute over the value of Pure Induction? The answer to this question depends upon whether some other model of the Principal Theorem can be worked out which would establish the independence (in that model) of the probabilifying effect of the instances from their eliminative effect. No such model is known. And it seems to us doubtful, whether such a model could be given without the introduction of highly arbitrary assumptions as regards the way in which probabilities are measured.[7]

It would involve no contradiction, if there existed a model which would establish the independence of the probabilifying effect from the eliminative effect. We should then only have to say that, abstract probability being interpreted in *one* way, the Principal Theorem makes the probability of inductions dependent of elimination, and abstract probability being interpreted in *another* way, the theorem makes confirmation independent of elimination.

In any case the possibility of a model, which makes the probabilifying effect of confirmation mirror a process of elimination, is of great interest. It establishes a not-trivial logical connection between the two main branches of the formal study of induction, viz. Elimination-Theory and Confirmation-Theory. And in doing this it lends support to an epistemological attitude, shared by some of the ablest thinkers in the field of induction (Bacon, Mill, Keynes), as regards the logical nature of inductive reasoning.

§8. *Probability, scope and simplicity. Reasoning from analogy. Mathematical and philosophical probability.*

Besides the point of view of confirmation, there are at least two other points of view from which the probability of inductive generalizations may be studied. One is the point of view of *scope* of generalizations. The other is the point of view of *simplicity* of generalizations.

J. M. Keynes, with C. D. Broad the founder of Confirmation-

Theory, also made a first attempt to study the probability of generalizations from the point of view of their scope.[1] The theory of scope turns out to bear relevantly on one 'classical' type of argument intimately connected with induction, viz. reasoning from analogy.[2]

We need not here give a general definition of the notion of the scope of a generalization. The idea is connected with some difficulties, which are related to the difficulties which arise in connection with the notion of a confirming instance. (See this chapter §6.)

We shall content ourselves with a notion of *relative magnitudes of scope* in propositions (not necessarily generalizations). We shall say that, *if one proposition entails another, then the scope of the first proposition is smaller than, or at most equal to, the scope of the second.*

Together with our previous Lemma (p. 118), according to which the probability of a proposition, on given data, is smaller than, or at most equal to, the probability of its logical consequences, we immediately obtain the following result:

The probability of a proposition, on given data, is directly proportionate to its scope.

It must be understood that this formulation is a shorthand for saying that, relative to the same data, a propostion of given scope cannot have a smaller probability than a proposition of smaller scope.

Consider a Universal Generalization of the simple form $(x)\ (Ax \rightarrow Bx)$.

The scope of this generalization is *increased*, if (*i*) to the antecedent is conjoined a new term, or (*ii*) to the consequent is alternated a new term.

For example: $(x)(Ax \rightarrow Bx)$ entails $(x)(Ax \& Cx \rightarrow Bx)$ and also $(x)(Ax \rightarrow Bx \vee Cx)$.

Similarly, the scope of a generalization is *decreased*, if (*i*) to the antecedent is alternated a new term, or (*ii*) to the consequent is conjoined a new term.

For example: $(x)(Ax \vee Cx \rightarrow Bx)$ and also $(x)(Ax \rightarrow Bx \& Cx)$ entail $(x)(Ax \rightarrow Bx)$.

From the definition of relative magnitudes of scope of propositions it follows that, in general, a conjunction has a smaller scope and a disjunction a greater scope than its single members. This result is now easily generalized to the following:

The scope of a universal implication is directly proportionate to the scope of its consequent and inversely proportionate to the scope of its antecedent.

Or, considering the proportionality between scope and probability: *The probability of a universal implication is in direct proportion to the scope of its consequent and inverse proportion to the scope of its antecedent.*

We may call this the Principal Theorem on the Scope of Generalizations.

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Reasoning from analogy is, roughly speaking, an argument of the following character:

From the fact that two things, x and y, resemble each other in a number of features, $A_1 \ldots A_n$, we conclude that a further feature B which is characteristic of x will also be characteristic of y.

This sort of argument is, in general, considered to be stronger (its conclusion more probable) the greater the number n is of common characteristics of the two things. (This is not to say that the strength of the argument depended *only* on the number of common characteristics.)

What is the logical foundation of this belief? *Why* do we think that the degree of likeness between two things is relevant to the question whether a certain property, known to belong to one of the things, will also belong to the other?

It is difficult to see, how the argument from analogy could appear convincing at all, if it were not for the fact that we suspect a *connection* between the presence of the properties $A_1 \ldots A_n$ and the property B in a thing. The presence of the former properties is thought of as somehow 'responsible' for the presence of the latter property.

A connection, which satisfies this requirement of causal 'responsibility', is the connection of sufficient (or necessary-and-sufficient) conditionship. If the set of properties $A_1 \ldots A_n$ contains among themselves a sufficient condition of B, i.e. if one or other of the properties individually or some of them taken in conjunction are sufficient to produce B, then the presence of *all* of them in a thing will be accompanied by the presence of B. Now the meaning of reasoning from analogy becomes this: The probability that n properties, $A_1 \ldots A_n$, contain among themselves a sufficient condition of B is greater, the greater n is.

The validity of this argument may now be examined within a theory of the scope of generalizations. For the argument amounts to saying that, in general, the probability of $(x)\,(A_1x\& \ldots \&A_{n+1}x \rightarrow Bx)$ will be greater than the probability of $(x)\,(A_1x\& \ldots .\&A_nx \rightarrow Bx)$. (It is understood that both probabilities are taken relative to the same data.)

From our Principal Theorem in fact follows that
$P(\,(x)(A_1x\& \ldots \&A_nx \rightarrow Bx)/h) \leqslant P(\,(x)(A_1x\& \ldots \&A_{n+1}x \rightarrow Bx)/h)$.
It is of some interest to examine when $<$ holds and when $=$ holds between the two probabilities.

$(x)\,(A_1x\& \ldots \&A_nx \rightarrow Bx)$ is the same proposition as the conjunction $(x)\,(A_1x\& \ldots \& A_nx\&{\sim}A_{n+1}x \rightarrow Bx)\;\&\;(x)\,(A_1x\& \ldots \& A_{n+1}x \rightarrow Bx)$. By virtue of the General Multiplication Principle it is easily shown that the two probabilities under consideration are equal if, and only if, $P(\;(x)\;(A_1x\& \ldots \& A_nx\;\&{\sim}A_{n+1}x \rightarrow Bx)/h\&(x)\;(A_1x\& \ldots \&A_{n+1}x \rightarrow Bx)\,) = 1$. Conversely, the first probability is smaller than the second, if this last (third) probability is smaller than 1.

This means: If the addition of a new common property A_{n+1} to the previous common properties $A_1 \ldots A_n$ is to strengthen the argument by analogy under consideration, then it must not be maximally probable that the set $A_1 \ldots A_n, {\sim}A_{n+1}$ contains a sufficient condition of B, *given* that the set $A_1 \ldots A_{n+1}$ contains a sufficient condition of B.

Considering reasoning from analogy, these conditions of equality and inequality are intuitively most plausible. For, assume that both sets actually contained a sufficient condition of B. Then it would follow that $A_1\& \ldots \&A_n\&{\sim}A_{n+1}$ and $A_1\& \ldots \&A_{n+1}$ are both sufficient conditions of B. And from this again would follow that $A_1\& \ldots \&A_n$ is a sufficient condition of B. In other words: if both sets contained a sufficient condition of B, then it would be *certain* that already the smaller set $A_1\& \ldots \&A_n$ contained a sufficient condition of B. And if this were the case, then the discovery of any *further* resemblance between things, which *already* agree in all the properties $A_1 \ldots A_n$, would be worthless as a contribution to the logical force of reasoning from analogy. Conversely we may say that, *if* the discovery of a further resemblance A_{n+1} is to be of value to the argument, then there must be some chance that a sufficient condition of B is to be found in the set $A_1 \ldots A_{n+1}$ *rather than* in the set $A_1 \ldots A_n$. And this is precisely what we should say there is *not*, if the assumption that there were a sufficient condition of B in the set $A_1 \ldots A_{n+1}$ would make it maximally probable that there is one in the set $A_1 \ldots A_n, \sim A_{n+1}$ as well.

Thus reasoning from analogy may be shown to depend, for its logical force, on simple ideas concerning the proportionality of scope and probability in generalizations.[3]

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The idea of relating the probability of generalizations to their 'simplicity' can truly be said to be among the classical ideas of scientific methodology.[4] *Simplex sigillum veri.* The notion of simplicity in generalizations has often been compared to the notion of simplicity in curves (and their algebraic expressions); and the problem of generalizing from particular data has been compared to the problem of tracing the simplest curve through a number of points in a diagram.[5] Sometimes ideas on simplicity have been related to ideas on the scope of generalizations.[6]

In spite of many efforts, no satisfactory theory of the relation of simplicity to the probability of inductions has as yet been developed. The subject largely remains a virgin field of inductive logic.[7] In this work we shall not make an attempt to penetrate into it.[8]

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It is sometimes alleged that probability, when contemplated in relation to simplicity of curves and of laws, is of a different nature from the probability-concept which is 'implicitly defined' in a set of postulates such as ours for abstract probability and 'explicitly defined' either in a Frequency- or a Range-Model of the abstract calculus. This probability of a different nature was traditionally called *philosophical probability* and contrasted with the notion of the calculus which was called *mathematical probability*.[9]

It appears, however, that the suggested dichotomy is at least not very helpful from the point of view of a *formal* examination, such as the one undertaken in this chapter, of ideas concerning the probability of inductive conclusions. It is noteworthy that all achievements so far in the formal clarification of these ideas have been reached either with the aid of the abstract calculus or some of its models or with the aid of some *weaker* calculus such as the various systems of so-called comparative probability. We must not, of course, exclude on *a priori* grounds the possibility of a formalism of probability which would be significantly different from the 'classical' ones and which might be successfully used for analysing some such ideas as those relating to simplicity of inductions.[10] But it is initially difficult to see how such a formalism, if invented for the *ad hoc* purpose of dealing with an obscure corner of inductive theory, could be of much interest either to the logician or to the philosopher. For any contribution, it would seem, becomes *significant* only if it succeeds in assigning to some of our natural, though notoriously obscure and vague, ideas of inductive probability a place within the common framework of all other significant uses of probability.

But irrespective of the question of plausibility and intrinsic interest of other formalisms of probability, *any* formalism of whatever structure would, so far as concerns its power of justifying induction, be subject to the same general conditions as our abstract calculus and its various models. Which these conditions are will be discussed in the next chapter.

CHAPTER VII

PROBABILITY AND THE JUSTIFICATION OF INDUCTION

§1. *Probability and degrees of belief.*

It is our intention in this chapter to answer the following question: In what sense and under what circumstances can the assertion either that a proposition is probable to degree *p*, or that one proposition is more probable than another proposition, be said to *justify induction*?

By its very use in ordinary language the word 'probability' is closely related to ideas such as those of 'possibility', 'degree of certainty', or 'degree of rational belief'. This connection naturally causes us to think of probability as justifying induction in the sense that it were 'better' or 'safer', in making predictions — and, in 'practical life', taking precautions — to *prefer* the more probable propositions to the less probable ones. In other words, this connection suggests the idea of probability as being 'the guide of life',[1] i.e. the safest finger-post to follow in the search for truth.

In order to assess the significance of this idea, it will first be necessary to discuss the relation between abstract probability and the notion of a partial belief.

By an *actual degree of belief* we mean a state of mind, a psychological fact expressing our attitude towards something (usually outside the sphere of our direct knowledge). In order to get a clearer idea of this psychological fact we must consider how degrees of belief might be measured, i.e. determined empirically.

We can, in the first place, regard the psychological facts called degrees of belief as *feelings of different intensity*. It is, theoretically, conceivable that a psychometrical method could be invented for comparing intensities in feelings and for assigning to those intensities numerical magnitudes. Nevertheless, it is for several reasons obvious that this way of evaluating degrees of belief is wholly inappropriate if we wish to relate degrees of belief to degrees of probability. We need only consider the fact that our belief in things which we habitually take for granted is often accompanied by practically no feeling at all.[2]

In the second place we might measure degrees of belief as follows:

We say that our degree of belief in a proposition is p, when we believe that the event which the proposition asserts will occur 'in average' (or 'in the long run') in a proportion p of all occasions for its occurrence.

This definition is peculiar in that it defines *degree of belief* in one proposition by reference to *belief*, as such, in another proposition. 'Belief' in connection with this latter proposition, again means the psychological fact which is expressed in saying that we 'believe' a certain proposition to be true (as opposed to certain other propositions which are 'believed' to be false).

We can, of course, go further and ask what *degree of belief* we have in the latter proposition believed to be true. This degree is then defined by reference to a further proposition believed to be true.

It seems to us that this second way of measuring degrees of belief is a true analysis of the old-established idea of measuring a man's belief by proposing a bet, and also of the philosophical doctrine that degrees of belief were causal properties of our beliefs, that is to say of our preparedness to *act* on our beliefs.[3]

Supposing a method of measuring actual degrees of belief to be given, we turn our attention to the interpretation-problem of formal probability. It is now to be observed that, whatever this method of measurement be, *the definition of probability as actual degrees of belief, would make the axioms and the theorems of the probability-calculus general synthetical propositions*, i.e. a kind of psychological laws for our distribution of beliefs. The interpretation would thus – unlike the frequency- and range-interpretations – be of the same type as the interpretation of Euclidean geometry as a theory of light-rays.

Consider for example the Special Multiplication Principle. If probability means degree of belief, then this principle says that my degree of belief in the proposition a & b (relative to some datum h) is the product of my degree of belief in a and my degree of belief in b, assuming that knowledge of one of these propositions does not influence my belief in the other proposition. This assertion, obviously is *synthetical* in the sense that from my believing the proposition a to degree p and the proposition b to degree p' *it does not follow* that I shall believe a & b to degree $p \times p'$. There is nothing in the nature of things to exclude that my belief in a & b, on the above premisses, is *not* $p \times p'$.[4]

The assumption that an axiom or theorem of the probability-calculus were false under this interpretation is, furthermore, not only possible but also likely to be true.[5] Thus the calculus of probability as a theory for the distribution of actual beliefs would presumably soon break down as being a false theory.

It is, however, evident that the adherents of the 'psychological' theory of probability have not intended the propositions of the probability-calculus to be synthetical propositions concerning the distribution of actual beliefs. They appear, on the contrary, to have assumed that the axioms of probability were a kind of *standard of correctness* in actual beliefs. The calculus of probability does not tell us how we believe *as a matter of fact*, but how we *ought* to believe.[6]

If we accept this regulative function of formal probability, it is necessary to abandon the idea of probability being *defined* or *interpreted* in terms of beliefs as psychological states of mind.[7] The exact nature, however, of this regulative function of the formalism, as also the true connection between probability and belief, still remains to be determined.

We will characterize the regulative function of the probability-calculus by saying that it provides a *standard of rationality* in degrees of belief. What does this characterization convey?

It is first to be observed that the standard of rationality cannot be defined as a *standard of consistency*. This is important. Not only does the calculus of probability not tell us how we actually believe but it does not even tell us that, *if* our actual beliefs in certain simple cases are distributed in such and such a way, *then in order to be consistent* we ought to distribute our beliefs in certain other cases — compounded of those simple ones — in a determinate way.[8] For actual degrees of belief are psychological facts and can, as actual facts, never *contradict* one another. This has been already illustrated in the example above of the Special Multiplication Principle.

It thus appears that 'rationality' in degrees of belief is not a *formal* property of the way in which actual beliefs are related, but that 'rationality' is what may be termed a '*material*' characteristic discriminating between certain actual beliefs and others.

This material characteristic determining certain degrees of belief as *rational* is, evidently, the same as that referred to when we say that it is 'rational' to prefer the more probable to the less probable, implying with this — as was already observed above — that probability is 'the guide of life' or the best finger-post to follow in the search for truth. A rational degree of belief, in other words, is a degree of probability justifying induction. It remains to be analysed what this 'rationality' in beliefs really means.

§2. *Rationality of beliefs and success in predictions.*

Let us, throughout this paragraph, assume that the proposition 'it is probable to degree p' implies the proposition 'belief of degree p is rational', and *conversely*. We ask the following question:

How is it to be determined whether or not it is rational to entertain belief of degree p in a given proposition?

As a first answer to the above question we suggest the following: Whether a certain degree of belief in a proposition is rational or not, is determined by the knowledge relative to which we consider the truth or falsehood of this proposition. By knowledge we mean here any analytical proposition and any synthetical proposition *known to be true*. (That is to say, no synthetical proposition which is *inductive* is included under the term 'knowledge'.[1])

For example: I may call it rational to believe to degree $\frac{1}{2}$ that this coin will come down 'heads' in the next toss-up on the knowledge that the coin is symmetrical and homogeneous and that the toss is made under certain determinate conditions.[2]

Suppose that — on this way of determining rationality in beliefs — it were rational under the circumstances C to believe to degree p a certain proposition asserting the event E. It follows from the initial assumption of this paragraph that it is rational under the same circumstances to believe the negation of this proposition to degree $1-p$.[3] We suppose further that $p>1-p$.

Under these suppositions we can conclude that, if rationality in beliefs is to be of relevance to the justification of induction, then it must be *rational*, in considering whether under the circumstances C

the event E will occur or not, to prefer the prediction of E to the prediction of not-E as a 'guide of conduct'.

Let us assume that, in predicting the event E n times under the conditions C, the prediction turned out to be true m times and false m' times, and that $m/n<m'/n$. If the process of predicting is continued so that n becomes very large, and if the proportion of true and false predictions shows a marked tendency to cluster round the *same* ratios m/n and m'/n respectively, this may make plausible the further assumption *either* that, *in the long run*, the events E and not-E occur under the circumstances C in the proportions m/n and m'/n respectively, *or at least* that not-E occurs more frequently than E.

It is to be observed that there is nothing in the previous suppositions as to the rational degree of belief in E which could preclude any of the above assumptions as to the relative frequency of E from being true. This implies that the suggested way of determining rationality in beliefs may lead to a situation of the following somewhat paradoxical character:

We call it, for determinate reasons, *rational* to prefer the prediction of the event E under the circumstances C to the prediction of not-E. But we assume, nevertheless, that we shall be *less successful* (i.e. that we shall arrive at the truth in a smaller number of cases) in predicting E under these circumstances than in predicting not-E. We know, furthermore, that this assumption may well be true.

From the possibility of such a situation it follows that the suggested way of determining rationality in beliefs cannot provide a satisfactory justification of induction. By this we do not wish to maintain that it were not possible to define rationality in beliefs without regard to success in predictions, and not even that one might not *call* this kind of rationality a 'justification' of the predictions which we actually make. One must simply be clear that this 'justification' is not of the slightest relevance to the 'sceptical' results of Hume as to the impossibility of foretelling the future.[4]

Consequently, if degrees of probability *qua* degrees of rational beliefs are to 'relieve' us from Humean scepticism, then the answer to the question as to whether or not it is rational to entertain, in a given proposition, a certain degree of belief must involve some reference to success in using the proposition concerned for predictions.[5] We must, in other words, be able to give some kind of guarantee that, in preferring the more probable to the less probable, we shall be more successful than in making the opposite preference.

In considering what the 'guarantee' mentioned could possibly be, the following will be instantly clear to us:

If, in the example given above, it is rational under the circumstances *C* to prefer the prediction of the event *E* to the prediction of not-*E*, we cannot with this assertion of 'rationality' wish to exclude the possibility that, those circumstances being realized, the more probable prediction will after all turn out to be false, the less probable again to be true.[6] Nor, of the assertion of 'rationality', do we demand that, of all predictions of *E* on the conditions *C* which will actually ever be made, a majority is going to be true and a minority false. We only demand that, if it is rational to prefer the prediction of *E* to the prediction of not-*E*, and it nevertheless happens that only a minority of actual predictions of *E* are true, then this distribution of truth and falsehood on the predictions must be regarded as representing a 'chance-event' which cannot be excluded, but which we think, *in the long run*, will give place to another distribution where the true predictions are in majority.[7] The statement that 'chance-events', such as the above 'abnormal' distribution of true and false predictions of the event *E*, will be *cancelled out* or *eliminated* in the long run so that ultimately the frequencies tend to be proportionate to the probabilities, we shall call the statement on the *Cancelling-out of Chance* ('Ausgleich des Zufalls').[8]

It can thus be stated that the guarantee of success needed in predictions, if induction is to be justified with reference to rationality in beliefs, concerns the statement that the Cancelling-out of Chance will take place for the degrees of probability corresponding to the degrees of rational belief.

It will immediately occur to us that there is one way of securing off-hand the Cancelling-out of Chance for every probability. This consists in *interpreting* formal probability in terms of limiting-frequencies. Under this interpretation the statement that the Cancelling-out of Chance will take place would become *analytical.*

If this method be resorted to, the assertion that it is *rational* to entertain belief of degree *p* in an event *E* would *mean* that this event occurs in a proportion *p* of all occasions. The statement, on the other hand, that the relative frequency of an event on all occasions of its occurrence is p, is a general *synthetical* proposition of the type called Statistical Induction. It is plain that if the truth of such Statistical Inductions be made the criterion for the truth of statements concerning rationality in beliefs, then any argument which tried to justify induction by reference to degrees of probability as representing rational degrees of belief would be circular.

The state of our problem is now the following:

We have shown that if rationality in beliefs is to justify induction, then it must be possible to give some kind of 'guarantee' that the Cancelling-out of Chance will take place for degrees of probability representing rational degrees of belief. We have further seen that this guarantee can be given if we resort to the frequency-interpretation of formal probability, but that this way of securing the Cancelling-out of Chance at the same time vitiates any argument from rationality in beliefs to the justification of induction. Consequently, if we are able to show convincingly that the *only* possible way of guaranteeing the Cancelling-out of Chance is to interpret formal probability in terms of limiting-frequencies, then it will have been demonstrated that the idea of compensating the 'sceptical' arguments of Hume with a theory of inductive probability is vain.

§3. *The Cancelling-out of Chance and the theorem of Bernoulli.*

The theorem of Bernoulli, speaking approximately, tells us that if we are concerned with propositions the probabilities of which all have the value p, then it is infinitely probable that 'in the long run', exactly the proportion p of those propositions are found to be true. Thus, if we have two propositions with the respective probabilities p' and p'', and p' is greater than p'', then we know that it is infinitely probable that the former proposition will be true on a greater number of occasions than the latter. In spite of this it may of course happen that, of the propositions which we actually have tested, those with the probability p'' have more frequently been true than those with the probability p'. This, however, has been due to 'chance'. In the long run we know, according to the theorem, that it is infinitely probable that such chance-events will be eliminated – 'cancelled out' – so that finally the proportions of true propositions in the respective classes become as indicated by the probabilities.

When stated in this way it looks as though the theorem of Bernoulli were closely related to that which we have called the Cancelling-out of Chance. Actually it is an old idea that this theorem amounted to a proof that — although irregularities and chance-events may upset our calculations when applied only to a narrow sector of the world — in the course of nature as a whole regularity, law and order prevail.[1] It is therefore intelligible that the proof of the theorem was regarded as an intellectual achievement of the greatest philosophical significance.[2] In the nineteenth century mathematicians and philosophers still spoke with the deepest awe about the wonderful philosophical implications of this theorem.

A deeper insight into the logical nature of probability, however, has led in our days to a common abandonment of the philosophical aspirations originally connected with the theorem of Bernoulli and the Laws of Great Numbers in general. Nevertheless, the idea of the relatedness between the formal theorem of the probability-calculus and the statement on the course of nature which we have termed the Cancelling-out of Chance, possesses high philosophical value as an illustration of a fallacy of thought, which not only underlies the main 'classical' misuses of the principles of probability for philosophical considerations concerning induction, but also is the source of various erroneous ideas about inductive probability which still play a prominent role in philosophical discussion. We shall therefore examine in some detail the fallacy made in relating the theorem of Bernoulli to the Cancelling-out of Chance.[3]

The apparent relatedness between the theorem of Bernoulli and the Cancelling-out of Chance has its root in the fact that in the theorem mentioned *two* probabilities are involved. Of these probabilities one remains constant throughout the course of considerations, whereas the other increases and approaches the maximum value 1. It is this probability *of the second order* that acquires the appearance of providing a bridge between probabilities *of the first order* and corresponding frequencies This occurs through an unconscious interpretation of the empirical implications of an increasing probability.

It has been already observed (p. 138) that the concept of probability is, by its very *use* in ordinary language, related to the concepts of certainty and of possibility. It seems plausible to say that probability measures *degrees of certainty or of possibility.*[4] The more probable a proposition, the smaller the possibility that this proposition will turn out to be false. If the probability of a proposition is infinitely close to the maximum value 1, it means that its possibility of being false is infinitely small, or in other words that the proposition itself is 'almost certain'.

The interpretation of probability as a magnitude of possibility is natural, particularly when we have to do with a *variable* probability-value. For whereas the grounds for interpreting a *fixed* probability as a degree of possibility must be in some way or other 'objective', (e.g. in the sense in which in games of chance certain properties of symmetry, being physical properties, provide us with a number of equally possible alternatives), the statement, on the other hand, that a probability *varies* seems by its very nature to *mean* a statement about the altered possibilities of a certain thing being true. For this reason it happens that, in the theorem of Bernoulli, we might speak about the constant probability of the first order without attaching to it any special 'interpretation', but nevertheless take as a matter of course the variable probability of the second order to 'mean' an increasing degree of possibility or certainty.

Thus the first step towards the use of the Bernoullian theorem as a bridge from the realm of probability to the realm of empirical frequencies consists in giving the theorem the following content: If the probability in certain propositions is p, then it becomes in the long run *almost certain* that among those propositions a proportion p are true.

At this point the following remark will take the reasoning further. Let us suppose that we have demonstrated, with the aid, for example, of the theorem of Bernoulli, that one possibility is greater than another, or that one is very great, another again very small. Is there anything in this which will preclude, *even in the long run*, the small possibility from being realized very frequently, the great possibility again very seldom or perhaps even not at all?

It is obvious that if there were nothing to preclude this, then a degree of possibility, even at its maximum, could not serve as a bridge to frequencies, since then the statement that a certain frequency is very, or even 'infinitely', possible would not tell us anything about the way in which that frequency will be realized. Therefore, the second step in using the theorem of Bernoulli as a bridge from probabilities to frequencies consists in our tacitly assuming the increasing degrees of possibility, involved in the theorem, to have the following implication: If the probability in certain propositions is p, then it will in the long run *almost always* happen that among those propositions a proportion p are true.[5]

On the other hand there is nothing in the *proof* of the theorem of Bernoulli which would exclude the highly possible from happening very rarely.[6] If we want to effect the necessary exclusion we must therefore turn our attention to the way in which degrees of possibility are measured empirically.

A well-known type of empirical determination of degrees of possibility is represented by the cases where certain physical attributes, known as properties of symmetry, afford a basis for calling alternatives equally possible. Consider, for example, the case of a homogeneous coin. As circumstances stand we are inclined to call the occurrence of 'heads' an event equally as possible as the occurrence of 'tails'. Suppose, however, that in actual trials, 'heads' occurred more frequently than 'tails'. Would this under any circumstances affect the judgement, passed on the basis of the properties of symmetry, that the alternatives are equally possible?

To this question two answers can be given. First, we may say that the judgment mentioned is not under any circumstances affected by what is true of the proportion of 'heads' and 'tails'. Secondly, we might say that *if* it were true that the proportions of 'heads' and 'tails' were unequal, even in the long run, then we were mistaken in calling the alternatives *equally* possible. This does not imply that statistical frequencies must be used as the measure of equal and unequal possibilities, as the observations of frequencies *may* represent chance-events, but it implies that any comparison between degrees of possibility is 'checked up' by a comparison between proportions, so that we cannot imagine the relative frequency of the event E' to be greater in the long run, than that of E'' without also assuming that E' is more possible than E'', and conversely.

It is clear that if the first answer be accepted then again we could not exclude that which was necessary if the theorem of Bernoulli was to serve as a bridge to frequencies, viz. that the small possibility will, even in the long run, occur frequently, and the great possibility seldom or never. Therefore we must resort to the second answer, which moreover seems to accord much better than the first answer with the way in which in practice we would judge the situation, and which actually has been suggested by several supporters of the theory that probability is to be defined in terms of quantified possibilities or 'Spielräume'.[7] But the acceptance of this answer has a remarkable consequence which the supporters of the theory mentioned have as a rule not observed.[8]

If a statement that two possibilities are equal is checked up by a statement about proportions, then the 'grounds' on which the former statement was made — properties of symmetry or whatever these grounds may be — cannot be defining *criteria* of equal possibilities. Rather than *criteria* they are only *symptoms* of this equality and inequality respectively. For a proposition about proportions, which is a general synthetical statement, plainly cannot be a logical consequence from those (singular) propositions which lay down the observable content of the circumstances under which an event takes place.[9] Since, on the other hand, a statement about proportions is 'checking up' the assertion about possibilities based on the latter propositions, it follows that the truth of these latter propositions can never imply the truth of the assertion about possibilities. It might always be the case that the assertion is false although the propositions in question are true.

We have now arrived at the following general conclusion: In whatever way degrees of possibility may be measured[10] it is not possible with this measurement to exclude that a great possibility will, even in the long run, be realized extremely seldom and a small possibility again very often, unless the grounds for measuring possibilities involve the assumption that a proposition will, on repetition, be true in the long run in a proportion of cases proportional to its degree of possibility.

With this we have shown that the theorem of Bernoulli provides a bridge from the realm of probabilities into the realm of empirical frequencies solely under the condition that the probability of the second order, involved in this theorem, is given an interpretation which implies that a proposition will, on repetition, be true in the long run, in a multitude of cases proportional to its probability. But this implication is nothing but that the Cancelling-out of Chance will take place. Consequently, that which makes it appear as though the theorem of Bernoulli were of relevance for the Cancelling-out of Chance in the case of the probabilities of the first order involved in it, is nothing but the tacit or unconscious assumption that the Cancelling-out of Chance is already established as regards the probabilities of the second order.[11]

The idea of the theorem of Bernoulli being a proof of the Cancelling-out of Chance is thus circular. Bernoulli's theorem can be used for proving inductive predictions concerning future frequencies only on assumptions which are themselves inductive. This important truth, incidentally, was pointed out already by Leibniz in his polemics against the uncritical use of the theorem which James Bernoulli himself suggested.[12]

§4. *The idea of 'probable success'.*

The above examination of Bernoulli's theorem was pursued mainly in order to point out an interesting fallacy of thought which will be relevant also to the following discussion concerning probability and the justification of induction. It is, however, clear for general epistemological reasons — and thus independently of the analysis in the preceding section — that neither Bernoulli's theorem, nor any other deductive chain of thought, could ever provide a proof of the Cancelling-out of Chance relevant to the question of justifying induction. This is seen from the following considerations:

The statement that the Cancelling-out of Chance will take place for a given probability is a *general* proposition. It is such because it concerns truth-frequencies[1] 'in the long run', i.e. in an *infinity* of cases. The only way of guaranteeing the truth of a general proposition is to make it *analytical*.[2]

That the statement on the Cancelling-out of Chance for a given probability, say p, is analytical means the following: If it is true that a proposition is probable to degree p, then it is logically necessary that this proposition will be true in a proportion p of all occasions. Or in other words: that the Cancelling-out of Chance is analytical *means* that the frequency-interpretation is accepted as a model of formal probability.

On the other hand it was seen above[3] that if the frequency-interpretation be accepted as a model of formal probability, then any statement that a proposition is probable to such and such a degree becomes a general *synthetical* assertion — i.e. an inductive proposition of the form called Statistical Generalizations — for which reason it would then be circular to justify induction by reference to probability.

We can therefore conclude that the guarantee of the statement on the Cancelling-out of Chance, necessary for the justification of induction with probability, applies to this statement as a general *synthetical* proposition concerning truth-frequencies.[4] Since it is impossible to guarantee *a priori* the truth of a general synthetical proposition, it follows further that the 'guarantee' in question *cannot* be a proof that the statement on the Cancelling-out of Chance will be *certainly* true.

On this point the following idea suggests itself: Perhaps the Cancelling-out of Chance as a synthetical statement could be secured or guaranteed, if not with *certainty*, at least with some degree of *probability* in its favour.

This idea, according to which the justification of induction with probability consists in a proof of 'probable success' in predictions, does not seem unplausible at first sight.[5] If we are asked whether the statement that the proposition *a* is more probable than the proposition *b* can justify induction, we should be likely to give roughly the following answer: Of course we cannot be certain that in predicting the proposition *a* we shall be 'on the whole' or 'in the long run' more successful than in predicting the proposition *b*, but it is *very likely*, if the proposition *a* is asserted to be more probable than the proposition *b*, that the former proposition will be true in a greater proportion of occasions than the latter.

This answer is very suggestive. It gains its suggestiveness partly from the undetermined way in which the word 'likely' or 'probable' is used in speaking *about* probabilities. This makes it appear as though we had to do with two kinds of probability, the one, used in saying that *a* is more probable than *b*, being 'mathematical' probability, i.e. a quantity treated in formal calculations, the other, used in judging the relevance of those calculations with the quantitative concept to matters of fact, being 'philosophical' or 'inductive' probability.[6]

This distinction between two kinds of probability seems the more plausible, as it would make it possible to interpret 'mathematical' probability in terms of limiting-frequencies, i.e. to make statements such as '*a* is more probable than *b*' general synthetical propositions concerning proportions, without nevertheless depriving, with this interpretation, propositions on probabilities of their power of justifying induction.[7] The justification in question would be a judgment in terms of 'philosophical' probability *about* the inductive propositions of 'mathematical' probability.

It is, however, to be observed that, irrespective of whether we want to distinguish between different kinds of probability or to keep to the same interpretation of the concept throughout, the idea that success in predictions could be guaranteed with probability is of no value for the justification of induction unless we can compare the *magnitudes* of the probabilities which are to guarantee this success, (at least) in the following way:

If, in preferring on a given occasion the prediction of the more probable proposition *a* to the less probable proposition *b*, we wish to justify this preference by stating that it will probably lead to success, then this statement must imply that it is *more probable* that we shall arrive at the truth in preferring *a* to *b* than in making the opposite preference. For if the statement on 'probable success' did *not* imply this, then any assertion according to which it were 'probable' that a certain proposition will be true on a greater number of occasions than another proposition would simply mean that 'we do not know, but perhaps' the truth-frequency of the former proposition will be greater than the truth-frequency of the latter, and this is exactly what we can say *in any case* of a general synthetical proposition concerning proportions. It would, under such circumstances, be possible to assert equally as well that I shall probably succeed in preferring the more probable to the less probable as that I shall probably succeed in preferring the less probable to the more probable, without saying anything as to which of these two alternatives is *preferable*. But in this case I have not *justified* the choice which I actually make between them.

If, therefore, philosphical probability is to justify induction, that is to say if it is to be a guide as to which opinion we ought to follow in the search for truth, then this probability must be capable of quantitative evaluation in the sense determined above.[8] This truth, which is obvious even upon the slightest reflexion and is wholly independent of how we wish to *interpret* probability, will soon be seen to possess the most remarkable consequences.

It has been shown above that if induction is to be justified with probability, i.e. if it is to be 'rational' or 'better' or 'safer' to prefer the more probable to the less probable, then it must be possible to guarantee that the more probable 'in the long run' is realized on a greater number of occasions than the less probable, or — more exactly — that the Cancelling-out of Chance will take place for the probabilities under consideration.[9] On the other hand we have seen that if the Cancelling-out of Chance is to be guaranteed with probability, this guarantee must imply that one of two ways of possible success is more probable than another. From this it follows that we must be able to guarantee the Cancelling-out of Chance *also* for the 'philosophical' probability guaranteeing the Cancelling-out of Chance for any 'mathematical' probabilities. From this important conclusion the reasoning easily proceeds as follows:

The Cancelling-out of Chance cannot be guaranteed with certainty if we wish to justify induction with probability. The remaining alternative is that it could be guaranteed with probability. Thus, we might introduce the idea of 'philosophical' probability for a second time. If this new probability is to express anything more than uncertainty in general, it must be capable of 'quantification' in the sense described above which tells us that the one of two alternatives is more probable than the other. This again leads to a justification of induction only if we can guarantee the Cancelling-out of Chance for these last two probabilities. In this way we are involved in an infinite retrogression.[10]

The crucial point of the whole discussion concerning probability and the justification of induction is to see that any statement, according to which something is 'probable', is relevant to what is going to happen, only if it implies that this 'something' is going to happen in a proportion of cases proportionate to its probability.[11] For this reason the idea that success in predictions could be guaranteed with 'probability' is dependent, in its power of justifying induction, on the possibility of guaranteeing that of those predictions a determinate proportion are true (the Cancelling-out of Chance) and consequently any attempt to guarantee the latter with reference to the former is circular.

It is thus impossible to substitute, for the demand of a guarantee with 'certainty' that the Cancelling-out of Chance will take place, the 'weaker' demand that this is to be guaranteed with 'probability'.[12] The apparent possibility of making this substitute arises from an unconscious use of the word 'probability' so as to imply truth-frequencies, that is to say from the same fallacy of thought which was the origin of the classical idea that Bernoulli's theorem amounted to a proof for uniformity and order in the course of nature.[13] The only way to justify this use of probability, however, is to *interpret* the concept in terms of truth-frequencies, and as soon as this is done it is easily seen that any argument which invokes probability for guaranteeing success in predictions has become circular.

It deserves mention that David Hume who was the first to see that general synthetical propositions cannot be proved true *a priori*, also clearly apprehended that this result of the impossibility of foretelling the future cannot be 'evaded' or 'minimized' by reference to probability.[14] He was aware of the infinite retrogression[15] to which the introduction of probabilities in this connection leads and also of the necessity of interpreting probability as a statistical concept if it is to be of relevance to statements on future events.[16] This clarity, in our opinion, gives the highest possible credit to the philosophical genius of Hume and strikingly contrasts him with those numberless critics of his ideas who have in the realm of probabilities found an escape from the 'scepticism' which he taught.

§5. *Logical and psychological, absolute and relative justification of induction with probability.*

The results of the analysis in the preceding sections can be said to have taken us back to the same point from which — in Chapter V — we started our investigations concerning inductive probability. No 'mechanism of probability', whatever be its formal structure and whatever its interpretation, is in itself a better guide to the truth than the mere fact, as such, that we regard certain propositions as more reliable, more probable, than other propositions.

We previously refused to accept this fact alone, as a *justification* of induction. It is now appropriate to consider this point afresh. The following will then occur to us:

In trying to justify inductions as general synthetical propositions we need a guarantee that certain things are going to happen in the future. Such a guarantee can only be given if it is relative to some other assumptions as to the future,[1] and this leads to an infinite retrogression. If, in spite of this, we break the chain of superimposed assumptions as to the future and declare ourselves content with the 'guarantee' given in the last of them, the sole *justification* of this behaviour, to which we can refer, is the fact that we *regarded* this last assumption as being so highly 'probable' as not to need a 'guarantee' itself.

Thus it is, after all, not inappropriate to call the mere fact that certain propositions are deemed more, others again less 'probable', i.e. reliable, a justification of induction. It is not a justification in the sense of its being a *proof* or *guarantee* about what is going to happen in the future, it is simply an expression for the attitude which we take to conjectures on future events. We use certain inductions for making predictions and taking precautions, and discard others for the same purpose. Our use of inductive arguments is, in other words, guided by estimations of reliability which as a matter of fact we perform.

The different weights attached to inductive conclusions might be characterized as different degrees of actual belief. It recommends itself to say that degrees of actual belief provide a *psychological justification of induction with probability.*

The essence of a *logical* justification of induction with probability consists in a proof or guarantee that certain inductions are better guides to the truth than others. If such a proof is to be given without interpreting probability statistically, i.e. without the introduction of any *assumptions* concerning the truth-frequencies in classes of propositions, then we might call the justification *absolute.*

Such a justification, we have seen, is not forthcoming. This, however, does not imply that our formal analysis and proofs of inductive probability, have been altogether useless. We might also speak of a *relative* justification of induction with probability, meaning proofs of the calculus to the effect that a proposition is a guide to the truth, reliable in proportion to its probability, *provided* that certain other propositions are also such guides to the truth. Such a 'relative justification' is obtained, if the probability of propositions is *interpreted* in terms of frequencies.

It remains to be considered to what extent it is possible to apply the frequency-interpretation to inductive propositions, especially to generalizations.

In the Frequency-Model, the probability of a given proposition means a truth-frequency within a class of propositions. This class is determined by those values of a variable which satisfy a certain propositional-function. The truth-frequency is the proportion of those values of the variable which also satisfy a certain other propositional-function. And the proposition, in the probability of which we are interested, is one of the propositions which this second propositional-function yields, when for the variable is substituted a constant.

The problem of how to define the two propositional-functions needed for measuring the truth-frequency, presents no particular difficulties when we are dealing with *singular* propositions. The reason for this is that a singular proposition contains no existential or universal operator. We need only substitute variables for some constant parts of the proposition and we get a propositional function. Any other propositional function with at least the same number of variables (of appropriate logical type) then constitutes a *possible* 'collectivity' for measuring the probability of the singular proposition in question.

When we preceed to *general* propositions, i.e. to propositions containing existential or universal operators, the situation is different. It does not seem natural to speak of an 'occasion' on which a general proposition might 'occur', or of a general proposition being true on certain 'occasions' and false on others. But, this being the case, is it then at all possible to speak of the probability of a general proposition, if probability has to be interpreted statistically?

This question has already been answered — in the affirmative. It was shown in Chapter VI, section 7 that the Principal Theorem of Confirmation can be given a frequency-model, in which the logical mechanism of the theorem 'mirrors' the logical mechanism of induction by elimination. The desired interpretation in frequency terms was obtained by regarding Universal Generalizations as instances of propositional functions of the type 'X is a sufficient condition of A' or 'X is a necessary condition of A', where A is a constant and X a variable property. The probability of the Universal Generalization becomes, on this interpretation, a proportion of actual sufficient (or necessary) conditions of a given characteristic within a class of possible sufficient (or necessary) conditions of it.

If the class in question is infinite, the proportion in question will have to be a limiting-frequency. The notion of a limiting-frequency again presupposes an ordering of the members of the class. The necessity of this presupposition challenges the adequacy of the frequency-interpretation as an analysis of the 'meaning' of probability. But it does not impair the logico-mathematical correctness of the model.[2]

It is an old idea that the 'probability' of a generalization depends on whether it belongs to a *class* of good or less good inductions.[3] We shall show that this idea can be worked out to a formally correct definition in frequency-terms of the probability of natural laws.[4]

Consider the two propositions 'all ravens are black' and 'phosphorus melts at 44° C'. They are both general implications of the form

$$(1) \qquad (x)\,(Ax \rightarrow Bx).$$

If we made a statement according to which one of these generalizations were more probable than the other, it is likely that we would be expressing something such as the following: The generalizations represent two classes (kinds, types) of natural laws, the proportion of true generalizations among all generalizations of the one class being greater than the corresponding proportion in the other class. It would presumably not be immediately clear how those classes of generalizations to which we refer are to be characterized, but it seems plausible to suggest, for example, that they are the class of hypotheses attributing respectively to a species of bird a constant combination of colour, and to a chemical substance one and only one melting-point. 'All ravens are black' is a less probable generalization than 'phosphorus melts at 44° C' if there are more species of birds, the individuals of which vary in colour, than there are chemical substances with more than one melting-point.

Let S_1 denote the class of all species of birds, and S_2 the class of all species of combinations of colours in a bird. The statement that all individuals of the species of birds X have the same colouring Y is then a proposition of the form

(2) $$S_1(X) \& S_2(Y) \& (Ex)(Xx \& Yx) \rightarrow (x)(Xx \rightarrow Yx).$$

If we take X and Y to be real variables (2) becomes a propositional function. Any pair of constant values X_1 and Y_1 which when substituted for the real variables in the propositional function $S_1(X)$ & $S_2(Y)$ & $(Ex)(Xx \& Yx)$, make this a *true* proposition can be said to constitute an 'occasion' for any hypothesis of the form $(x)(Xx \rightarrow Yx)$. The proportion of times on which the propositional function $(x)(Xx \rightarrow Yx)$ turns into a true proposition on all such 'occasions' constitutes the truth-frequency in the class of hypotheses as to constancy of colour in species of birds.

In an analogous way we may, in the form of a propositional function, define a class of 'occasions' for a statistical interpretation of the probability of the hypothesis that phosphorus melts at 44° C.

Against the idea of defining a 'collectivity' for the purpose of measuring the probability of a general proposition the following objection has been made:

A proposition of the form $(x)(Xx \rightarrow Yx)$ is *unverifiable*. Consequently it is not possible to determine whether such a proposition is true on a given 'occasion' of it, nor to count the truth-frequencies even among a finite number of hypotheses. Or as Popper expresses it 'Dieser Versuch scheitert . . . daran, dass wir von einer Wahrheitshäufigkeit innerhalb einer Hypothesenfolge schon deshalb nicht sprechen können, weil wir ja Hypothesen zugestandenermassen nicht als "wahr" kennzeichnen können. Denn könnten wir das — wozu brauchen wir dann noch den Begriff der Hypothesenwahrscheinlichkeit?'[5]

This remark, however, is beside the point. It is a fact that Universal Generalizations cannot be verified, but there is no logical objection to the *assumption* that, in a class of such generalizations, a certain proportion is true. Leaving for a moment all considerations as to 'probability', it is surely in no way absurd to assume a certain thing to be true of, say, the proportion p of all species of birds, although such an assumption, strictly speaking, cannot be 'verified' even for a single species. It is a reason for regarding this assumption as plausible if roughly the proportion $1-p$ of all species are known to lack the property in question – which knowledge can be obtained since Universal Generalizations can be *falsified* – and if continued observation and long experience had *confirmed* the hypothesis as to the presence of this property in individuals of the remaining species.

Propositions concerning the probability of Universal Generalizations, meaning truth-frequencies in classes of hypotheses, therefore cannot be verified, but nevertheless can be, so to speak 'recommended' or 'discountenanced' by the records of experience. The logical difference, consequently, between singular propositions and Universal Generalizations, consisting in the fact that the former may be verifiable and the latter not, is irrelevant to the question of whether or not it be possible to interpret the probability of general propositions in terms of frequencies.

There remains the further problem whether it be possible to determine the propositional functions ('collectivities') for measuring the probability of generalizations in such a way that the loose estimations of reliability, with which we usually content ourselves, can be made into exact numerical evaluations. We shall not embark upon a discussion of this problem here. For several reasons we are inclined to take a sceptical attitude to the possibility in question.[6]

CHAPTER VIII

INDUCTION AS A SELF-CORRECTING OPERATION

§1. *Induction the best mode of reasoning about the unknown. The ideas of Peirce.*

WE say that we employ the inductive mode of reasoning or an 'inductive policy' when we make inferences to the unknown on the principle that future experience will be in conformity with the past. For instance: We conclude from the fact that all observed A's have been B that the unknown A's will also be B, or we infer from the fact that a proportion p of the observed A's have been B that the *same* proportion of all A's are B. We have seen that we cannot guarantee, prior to testing, the truth of any such inference as a synthetical proposition, nor can we show one generalization from a set of data to be more probable than another in the sense that it were a better guide to the truth. We might, in other words, have made the inferences to the unknown in any other than the 'inductive' way and nevertheless been successful. But in spite of these 'negative' results it seems possible to assert that *the inductive method as such is, in a certain sense, the best way of making conjectures about the unknown.* For this method seems to have a certain advantage possessed by no other method of conjecture.[1]

The methods of reasoning about the unknown could be compared to the methods by means of which we find our way out from a complicated labyrinth. This last can be accomplished in many different ways. We might just run ahead and guess the right course at each turn. Or we might determine the course to be chosen according to a fixed rule. One of such rules holds a peculiar position. It is the determination *consistently to keep to the same hand*, either to the right or to the left. Of this rule it can be proved that it must finally lead us out of the maze. It is very likely that the employment of some other rule, or even mere guessing, will lead us more quickly out of the labyrinth, but the employment of any such method *may* also never attain this end. The determination to keep to the same hand is the only method of which we can be sure that, if persisted in consistently, it will lead to the goal.

This power of the method mentioned of finding one's way through a maze is due to its being, by its very definition, a *self-correcting* method. That is to say, it follows from the way in which the method is defined that, even if it momentarily takes us away from the exit of the labyrinth, this deviation from the goal will be automatically corrected until the exit is reached and we leave the maze.

Something corresponding to this seems to be true also of induction. Two classes A and B intersect and we want to generalize as to the proportion of the one class which falls within the other. This can be done either by guessing or 'methodically', i.e. according to some rule. One such rule is provided by the inductive method. It is not impossible that we might arrive at the true generalization more quickly by employing some method other than the last-mentioned one, or even by guessing. But, on the other hand, none of these methods may lead to the truth. The inductive method on the other hand may momentarily give results deviating very much from the true value of the proportion, but it follows from the way in which this method is used that any such deviation is in time *corrected* by experience's own indication. This process of correction continues until the true proportion is reached. Thus induction, like the method mentioned of finding one's way out from the labyrinth, is by its very nature a self-correcting operation, and as such is the only method of making inference about the unknown of which it can be proved that it must, when consistently employed, finally lead to a *true* generalization. This property of the inductive method might plausibly be regarded as a *justification* of our use of induction.

This argument about induction as 'the best mode of reasoning about the unknown', which is related to the idea that our experiences are 'fair samples' from a larger totality, was expressed for the first time by Peirce.[2] Peirce speaks of 'the constant tendency of the inductive process to correct itself' as the 'essence' and the 'marvel' of induction.[3] He also says that 'the validity of an inductive argument consists, then, in the fact that it pursues a method which, if duly persisted in, must, in the very nature of things, lead to a result indefinitely approximating to the truth in the long run'.[4]

The validity of the argument, naturally, depends upon the assumption that in the respective cases the proportion as to which we generalize *exists*. For if, of the two classes A and B, no definite proportion of the first is included in the second, then obviously any method of generalizing as to the proportion would be equally vain.

This condition, however, was not stated by Peirce. It is likely that he assumed it to be tautologous, and hence unnecessary to formulate explicitly. But in fact the condition is not tautologous. We shall dwell on this point for a moment, as there is psychologically a most interesting origin for the supposition that always when two classes intersect it is a tautology that a definite proportion of the first is included in the second.

We denote each member of the class A that is also a member of the class B by 1, and each member of A that is not a member of B by 0. The *observed* A's can always be pictured in a series of a definite number of alternating 1's and 0's, such as this

(1) $$1\,1\,0\,1\,0\,0\,0\,1\,1\,1\,1\,0\,1\,1\,.$$

In any such series there is a definite proportion of A's which are B. This follows *per definitionem* from the way in which such a series is constructed. And now we fancy the picture of *all* A's to be something like this

(2) $$1\,1\,0\,1\,0\,0\,0\,1\,1\,1\,1\,0\,1\,1\,\ldots,$$

where the dots indicate that the series of 1's and 0's perhaps goes on indefinitely.

This picture, however, is most fallacious. It causes us to think of the class A as in some way 'resembling' finite collections of the class, picturable in rows such as (1), chiefly on account of its being a '*very long*' row of this kind. (It is the same type of fallacy which occurs over and over again in the philosophy of mathematical conceptions. We know it, for example, from the controversies about the infinitesimals or the transfinite, controversies of which the deepest root lies in the inclination to think of infinitesimal quantities as something 'very small' and of transfinite magnitudes as something 'very great'.)

Actually the only way to state the fact that a definite proportion of the class A is included in the class B is to assert the proposition

$$(3)\quad (E!p)\,(\varepsilon)\,(x_m)\,(Ex_n)\left[n>m\ \&\ \frac{\overset{i=n}{\underset{i=1}{N}}\ A\,x_i\ \&\ B\,x_i}{\overset{i=n}{\underset{i=1}{N}}\ A\,x_i} = p\pm\varepsilon \right]$$

or some proposition equivalent to it. But the statement (3) is no tautology, for, as was seen above[5] it is conceivable that it were false. And hence the assumption that when two classes intersect there must be a definite proportion of the one included in the other is itself an inductive assumption.[6]

This fact can hardly be said to intrude upon the *validity* of the argument about induction being the best mode of reasoning about the unknown, as this argument *makes sense* only under the assumption that the proportion as to which we generalize exists. It imposes, however, a serious restriction upon the *applicability* of the argument to single cases. For as the statement that a definite proportion of one class is included in another is itself inductive, it follows that we can never guarantee that in *this* case such a proportion exists and hence that induction here will be the best mode of reasoning.

On the other hand knowledge that the existence of a proportion is a non-tautologous fact, expressed in a statement of the form (3), gives us an unexpected possibility to strengthen Peirce's argument. This has been pointed out by Reichenbach, whose system of an 'inductive logic' is very closely related to the idea about induction as a self-correcting operation.[7]

It can be proved that if the statement (3) is true, i.e. if a definite proportion p of the A's are B, then also the statement

$$(4)\qquad (\varepsilon)\,(Ex_m)\,(x_n)\left[n>m\rightarrow \frac{\overset{i=n}{\underset{i=1}{N}}\ Ax_i\ \&\ B\,x_i}{\overset{i=n}{\underset{i=1}{N}}\ A\,x_i} = p\pm\varepsilon \right]$$

is true for this p, i.e. then there exists a finite number m such that from the m'th A onwards the proportion of A's which are B *remains* in the interval $p \pm \varepsilon$.[8] (The series of relative frequencies corresponding to the observations is, in other words, convergent.) But this means that if two classes intersect so that a definite proportion of the first is included in the second and we want to generalize as to this proportion, then the inductive method can be proved *to attain this goal in a finite number of steps*, i.e. after a finite number of corrections.[9]

It is clear that for 'empirical' classes it is not possible to calculate the exact value of the ordinal m from which point onwards (for a given ε) the observed proportions will remain within the fixed interval. Thus it is never possible to tell, in a given situation where we are using induction, whether the corrections can still be expected to continue, or whether the true generalization has already been reached. But perhaps in spite of this it would be possible to devise some 'technique' by means of which the approach to this important m could be accelerated. Such a 'technique' has actually been suggested by Reichenbach. We shall therefore examine his ideas as regards this topic somewhat more closely.

§2. *Reichenbach's Method of Correction.*

In this section we shall attempt to give a simplified description of Reichenbach's idea which should make it easier to assess its epistemological significance.[1]

Let the problem be to find 'by induction' what proportion of members of a certain, potentially infinite, sequence S possess a certain characteristic A.

We have observed an initial segment of n members of S. The recorded proportion of A among them is p. We generalize, following the inductive principle that 'the future will resemble the past',[2] that p is the limiting frequency of A in S. This generalization we call a *posit* ('*Setzung*') *of the first order*.[3]

For this posit we can find an *appraisal* ('*Beurteilung*') which may result in a *correction* of the posit. The procedure is as follows:

We consider s different, potentially infinite, sequences $S_1 \ldots S_s$. (We shall assume that our sequence S above is one of them.) We observe initial segments of n members of each sequence and record the proportion of A in these segments. The recorded proportions are $p_1 \ldots p_s$.

Following the inductive principles we assume that $p_1 \ldots p_s$ are the limiting-frequencies of A in the s sequences. Thus, we get in all s posits of the first order.

Some of the s p-values may be identical (or 'nearly identical').[4] We assume that there are r different p-values. ($r \leqslant s$.) We call them $q_1 \ldots q_r$.

We record the relative frequency of p-values which are q_1. (If there are m such p-values, the recorded relative-frequency is $m{:}s$.) Similarly, we record the relative frequency of p-values which are q_2, etc. In this way we get in all r relative-frequencies $f(q_1) \ldots f(q_r)$. Some, or even all, of them may be identical. Their sum, of course, is 1.

Following the inductive principle we assume that $f(q_1) \ldots f(q_r)$ are the limiting-values, which those r recorded frequencies will approach, when the segments of n members of the s sequences are indefinitely increased. Thus we get r inductive generalizations. We call them *posits of the second order.*

We raise the following arithmetical problem:

If the limiting-frequency of A in a sequence is q_i, what is the limiting-frequency, among all sets of n members of this sequence, of such sets in which the proportion of A is q_k?

In order to be able to answer this question, certain assumptions about the 'inner structure' of the sequences in question will have to be made.[5] For purposes of our simplified description, however, we ignore these assumptions. We imagine the question to have been answered for all possible pairs q_i and q_k. (Since there are r q-values, there are r^2 such pairs.) The calculated limiting-frequency, for given q_i and q_k, we symbolize by $f(q_i, q_k)$.

We can now use the values $f(q_i)$ and $f(q_i, q_k)$ for calculating an answer to the following question, which may be termed the 'inverse' of the question just answered:

What is the limiting-frequency, among all sets of n members from any of the s sequences, of such sets as satisfy the following two conditions:

(*i*) the proportion of A in the set is q_k, and

(*ii*) the limiting-frequency of A in the sequence, from which the set is a selection, is q_i?[6]

The calculated limiting-frequency we symbolize by $F\ (f(q_i);\ f(q_i,q_k))$ or shorter $F_{i,k}$.

Now, for a fixed i, the value of $F_{i,k}$ will, in general, be different for different values of k. Its maximum value we call $F_i max$.

Since i can take r different values in all, we have altogether r such maximal values $F_1 max \ldots F_r max$.

The value of $F_{i,i}$ we call the *appraisal* of the first order posit (inductive generalization) that q_i is the limiting-frequency of A in a sequence, from which an initial segment of n members has been found to contain A in the proportion q_i.

The appraisals are used for the purpose of *correcting* the posits according to the following rule:

If $F_{i,i}$ equals $F_i max$, no correction is needed. If $F_{i,i}$ is less than the maximum, then we correct the posit by assuming that the limiting-frequency of A in the sequence is, not q_i, but a value $q_{i'}$ such that $F_{i,i'}$ equals $F_i max$.[7]

This correcting procedure, which even under the above simplified description of its mathematical mechanism may appear involved and cumbrous, has in fact a very simple and clear-cut meaning. It amounts to this:

We should posit *that* value as the limiting-frequency of A in the sequence S, which is *most frequently* the limiting-frequency of A in a sequence, of which a set with the recorded relative frequency of A is a selection.[8]

It must now be observed that the posits of the second order upon which the correction essentially depends, are themselves generalizations about proportions, and may as such be in need of correction. To this end we may derive an appraisal of the second order for each posit of the second order and then use the appraisal for possible corrections, all in a way analogous to the one described above for the first order posits. The corrected values of the posits of the second order could then be used for correcting our previous calculations of appraisals for the posits of the first order, and may thus ultimately lead to further corrections in the first order posits.

The correction of the posits of the second order would in their turn depend on certain posits of the third order, which again are capable of appraisal and correction. Thus we get an infinite hierarchy of superimposed posits and appraisals.

We are now in a position to tell in what sense the Method of Correction means an accelerated approach to the points of convergence in sequences of relative frequencies, and to assess the significance of this to the problem of induction.

On the assumption that the recorded relative frequencies possess limiting values in the sequences under consideration, we can from what was said above (in section 1 of the present chapter) on the nature of Statistical Generalizations, draw the following conclusion:

For any given value of ε, however small, there exists some value of n, call it n_1, from which onwards *all* the posits of the first order are true within the limits $\pm\varepsilon$. And similarly, there exists some value of n, call it n_2, from which onwards *all* the posits of the second order are true within the limits $\pm\varepsilon$.

That a posit of the first order is true means that the limiting-frequency of the characteristics in the sequence under consideration is as indicated by the relative frequency of this characteristic in the observed initial segment of the sequence. That a posit of the second order is true means that the proportion, among all sequences under consideration, of sequences with a certain limiting-frequency of the characteristic, is as indicated by the observed proportion of initial segments of sequences showing this relative frequency of the characteristic.

From considering these meanings it is clear that if *all* the posits of the first order are true, then *all* the posits of the second order are true also. But it is equally clear that this proposition cannot be converted. For *all* the posits of the second order may be true, and yet *some* of the posits of the first order false. It follows from this that $n_2 \leqslant n_1$. And this means that all posits of the second order will become true not later than, i.e. either sooner than or simultaneously with, all posits of the first order.

Generalizing, we can prove that all posits of a higher order will necessarily become true not later than all posits of inferior orders. *This* is the sense in which the building up of the hierarchy of posits and appraisals and its use for corrections may be said to 'accelerate' or 'speed up' the inductive approach to truth.

It is important to observe that the 'acceleration' in question does not amount to a proof that we shall, with our posits of the first order, sooner reach the point of convergence if we resort to the Method of Correction, *than* if we generalize on the basis of the inductive principle alone.[9] And it is also evident from the way in which the hierarchy of posits and appraisals is constructed that we can never determine, after any number of corrections, whether the true value of the proportion as to which we are generalizing has been reached or not, nor can we ever know how many corrections, if any, still remain to be made. And we cannot even exclude the possibility that the corrections will, for any length of time, take us further away from the true value of the proportion instead of letting us approach it.[10]

For these reasons it seems to us that Reichenbach's Method of Correction cannot be said to add anything epistemologically significant to the idea that induction is an indefinite and self-correcting approximation to the truth.

§3. *The goodness of inductive policies reconsidered.*

We return to the problem, whether induction can be justified as being, in some sense, the *best* policy for making conjectures about the unknown. The superiority of induction, we have seen, was thought to be in its self-correcting nature and in its alleged indefinite approximation to the truth. In view of what has been said above of these attributes of induction, it is at least doubtful whether they can really be said to constitute a 'superiority' of inductive over alternative policies.[1] We shall now consider a somewhat different way, related we believe to the Peircean approach, of establishing the superiority of induction.

It is useful here to make a rough distinction between *prediction* and *generalization.* A prediction, we shall say, is about a single case ('event') or about a finite number of cases. A prediction should be, in principle, verifiable and falsifiable. A generalization is about an unlimited number of cases.[2]

Accordingly, we shall say that a method or policy for reasoning about the unknown can be either a *prediction policy* or a *generalization policy.*

A prediction policy will be called *inductive,* if it proceeds according to a maxim of one of the following schematic types:

(*i*) If all observed A are B, then predict that the n next A are B. ($n \geqslant 1$.)

(*ii*) If a proportion p of all observed A are B, then predict that a proportion (as near) p (as possible) of the n next A are B. ($n \geqslant 1$.)

Similarly, a generalization policy will be called *inductive*, if it proceeds according to a rule of one of these types:

(*i*) If all observed A are B, then generalize that all A are B.

(*ii*) If a proportion p of all members of a sequence S are B, then generalize that the limiting-frequency of B in S is p.

This characterization of inductive policies is only a rough first approximation. It would probably be sensible to count as 'inductive' also policies which employ rules somewhat 'laxer' than those above, but resembling them in essential features. We need not discuss this question here.

(In view of the fact, among others, that generalization policies of type (*ii*) presuppose the ordering of the members of a class into a sequence, it seems to us doubtful whether they play any great role in science.[3] It might be suggested that there is a more important (and embracing) type of policy, viz.)

(*ii*)′ If a proportion p of all observed A are B, then generalize that the *probability* that any given A is B is p.

We shall not, however, here discuss inductive policies of the probability-type.)

We shall say that a policy is *truth-producing* (possesses a 'truth-producing virtue') if its predictions or generalizations always, or at least in a great majority of cases, are true. We shall also say that verified predictions *confirm* a policy.

We shall now briefly consider policies which are, or may be claimed to be, *not-inductive*. Not-inductive policies are probably worth a closer scrutiny than is given to them here and elsewhere in the literature on induction.

Let n be an integer and p a value in the interval between 0 and 1 inclusive. $f(p,n)$ is to be a function of p and n which satisfies the following three requirements: (*a*) for any given p and n we can calculate a unique value of $f(p,n)$; (*b*) the value of $f(p,n)$ is in the interval between 0 and 1 inclusive; (*c*) the value of $f(p,n)$ is different from p.

Consider a prediction policy of the following type:

If a proportion p of the n last A which have been observed are B, then predict that a proportion (as near) $f(p,n)$ (as possible) of the n next A are B.

A policy of this type resembles induction in that it is *guided by experience*. What we anticipate according to this policy is rigorously determined by what we have recorded. The policy is thus *self-correcting*. (Being, in this sense, self-correcting, is therefore no privilege of the inductive method.)[4]

The policy differs from induction in that it proceeds, *not* on the principle that 'the future will resemble the past', but on the principle that the future will, in a characteristic way, be different from the past. We shall call a policy which proceeds on this principle, *counter-inductive*.[5] A more rigorous definition of such policies will not be attempted here. The counter-inductive policies are a sub-class of not-inductive policies.

As a prediction policy the above not-inductive method may be superior to an inductive prediction policy in a very palpable sense. Consider the following situation:

The property B becomes *rarer and rarer* among instances of A. If we predict its frequency in a set of n new instances of A following an inductive policy we shall, on the whole, predict a too high frequency. But if $f(p,n)$ is a function of p and n which corresponds to the 'rate of diminution' in the frequency, then we shall with the aid of the counter-inductive policy, on the whole, predict the right frequencies.

Consider next a generalization policy according to which the limiting-frequency of a characteristic in a sequence is consistently assumed to differ in a determinate way from the recorded relative frequency. Use of such a policy would, in the following sense, always be futile:

Either the relative frequency of B in S has a limiting-value, or it has not. In the first case we shall, following the inductive policy, in a finite number of steps reach the point of convergence which answers to an arbitrary value of ε. This entails that by following the counter-inductive policy we shall in a finite number of steps reach a point from which onwards we make only false generalizations. In the second case again, *no* generalization policy about limiting-frequencies will approximate to the truth.

As we have said repeatedly before,[6] it is not necessary that the relative frequency of a characteristic in a sequence should have any limiting value at all. Beside approximating to a limit there is also the behaviour of relative frequencies which is best termed an *oscillation* between two extremes.[7] Special policies may be devised for predicting and for generalizing about such oscillating behaviour of frequencies. Such policies may be either inductive or counter-inductive or not-inductive. We shall not stop to examine them here. It is a lacuna in the literature on induction that, as far as we know, oscillating relative frequencies have never received systematic attention.[8]

Of inductive and counter-inductive policies we may, comparing them with each other, say that they use the *same* premisses but draw *different* conclusions from them. We now turn our attention to not-inductive policies which do *not* use the same premisses as inductive arguments (but may reach the same conclusions).

It is noteworthy that it is difficult to give any uniform characterization of such not-inductive policies or to illustrate them by an example which seems worthy of being seriously considered as a *policy* or *method* at all. (This, by the way, throws light on the proposition that induction is the best *method* for making conjectures.)

As a crude example of such a not-inductive policy we may take the consulting of an 'oracle' for purposes of prediction and generalization. We shall not attempt to explain what should be the other characteristics of an 'oracle' besides the negative one that we must not be able to calculate the oracle's answer from experiential premisses according to some known rule. If the 'oracle' did not possess this characteristic, consulting it would be equivalent to adopting a counter-inductive policy. Since this negative feature must be common to all not-inductive policies, which are not counter-inductive, we shall call such policies *oracle-policies*.

It is clear that we cannot exclude the *possibility* that an oracle-policy would be superior to induction in the sense that its predictions and generalizations were more often true than those made in accordance with inductive (or counter-inductive) policies.[9] And it is noteworthy that there is no limitation to an oracle-policy's powers of competing successfully with an inductive policy, when we are generalizing about proportions, which would correspond to the limitation in a counter-inductive policy's capacity.

Having thus made clear in what respect not-inductive policies may and may not be superior to inductive policies in the search for truth, we raise the question: Could there ever be a *ground* or *reason* for adopting a not-inductive policy, and what would such a reason look like?

If asked why we adopt a certain policy for predicting or generalizing, the 'reason' given is often some fact about our *beliefs* and (other) *attitudes* in the matter. Why, for example, did we predict *B* with a lesser frequency among the last 100 *A* than the recorded frequency among previous *A*? Because we believe that the occurrence of *B* will become rarer. Why did we consult an 'oracle' about tomorrow's weather and act according to its prediction? The answer could be that we regard the oracle as the representative of a God, whose powers of foretelling the future we trust, or whose wrath for not having taken his advice we fear.

If belief is called a 'reason' for adopting a policy, it should be borne in mind that a 'reason' of this sort is quite without relevance to the question of *justifying* the choice of policy, i.e. to the question of objectively appraising the policy's truth-producing capacity. The same is true of any 'reason' which consists in our attitude to our source of information about the future – such as an attitude of love or trust or fear of a divine power. We shall therefore, for the sake of clarity, distinguish between *reason* and *motive* and say that a belief or other attitude may be a motive for adopting a certain policy, but not a reason for doing so.[10] By a reason for adopting a policy we shall understand a *reason for belief* in the truth-producing virtue of the policy. A reason in this sense, moreover, should be some *known fact* about the 'world', i.e. about something which exists independently of the predicting or generalizing subjects. (This last excludes beliefs and attitudes from being reasons for beliefs.)

The above is a rough characterization only, but it will have to suffice for present purposes. Be it remarked, however, that the logic of the case is further complicated by the fact that an attitude (other than belief) to a source of information is sometimes a motive both for adopting a certain policy and for believing it, and sometimes a motive only for adopting but not for believing a policy. Thus, for example, fear of punishment for not having taken the oracle's advice could be a motive for adopting a policy in which we do not believe. In such a case it is difficult to see how anything which counts as a reason for our attitude to the source of information could ever be a reason for belief in the policy. If, however, the attitude to the source of information is also a motive for belief, then a reason for the attitude *may* but *need not* be at the same time a reason for the belief.

In addition the following point should be noted about our conception of 'reason'. Since the fact which constitutes the reason should be *known*, we cannot — on our terminology — count as reasons unknown facts which, *if known, would have been reasons* for adopting a certain policy. Thus, for example, we must not say of the fact, that B actually became rarer and rarer among A's from a certain moment on, that this *was* already before it became known a reason for adopting a counter-inductive policy for predicting B.

After these preliminaries we raise the question: *Of what kind* would the facts about the world have to be in order to qualify, if known, as reasons for adopting a certain *not*-inductive prediction or generalization policy?

We first consider counter-inductive policies.

What should we, for example, consider a reason for predicting that the relative frequency of B among the next 100 A will be 45 per cent, although the relative frequency of B among, say, the first 500 A which we have observed was 48 per cent? As a reason we might count the observation, i.e. known fact, that among the first 100 A the frequency of B was 50 per cent, among the next 100 it was 49 per cent, among the next 100 again 1 per cent less, and so on. Or the reason might be some other, similar observation about the fluctuation of the frequency of B among the A. Or it could be the observation, say, that the frequency of B among certain *other* properties $A_1 \ldots A_n$ has sunk from 50 per cent among the first 100 instances to 45 per cent among the sixth 100 instances.

This is only a very rough indication of possible examples. And in this place it is important to warn against a misunderstanding. The above illustration of reasons must not be taken to mean that any observation of the kind indicated would *ipso facto* constitute a reason for the prediction policy under discussion. Whether the observation will be a reason or not for adopting the policy depends upon its relation to all the other facts which are known about the case. Thus the 'weight' of some of the observations mentioned in favour of the counter-inductive policy may be, so to speak, 'counteracted' by some other observation speaking in favour of a different, e.g. of an inductive prediction policy. What the examples were intended to show was only what kind of facts *might* count as reasons for adopting a counter-inductive policy.

The facts which we indicated agree in an important feature, which the reader may already have discerned. The reasons for adopting a counter-inductive policy were thought to be confirmative of some *inductive* prediction policy which produces the same predictions as the counter-inductive policy. The observed facts that among the first 100 *A* the proportion of *B* was 50 per cent, among the next 100 49 per cent, etc. confirm the inductive policy, always to predict *B* among the 100 next *A* with 1 per cent less frequency than among the 100 last *A*. This policy is inductive in as much as that it may be said to proceed on the principle that the future will resemble the past with regard to the 'rate of diminution' for the relative frequency of *B* among *A*. The policy, moreover, conforms to the prediction-scheme (*i*) on p. 167 above, if for '*A*' we substitute 'sets of 100 *A*' and for '*B*' we substitute 'showing 1 per cent less *B* than the immediately preceding set of 100 *A*'.

Thus from the view which we took of the possible reasons it follows that a *reasoned* counter-inductive policy is equivalent to some inductive prediction policy. A similar argument may be conducted for counter-inductive generalization policies.

It may be asked: How can we be sure that *any* known facts which constitute reasons for a counter-inductive policy must be confirmative of some inductive policy? The answer is that, in a sense, we cannot be sure of this at all. The only indubitable certainty about it which we could reach would arise from a decision not to *call* any other facts 'reasons' for a counter-inductive policy. But no such decision would strengthen the case which we are here pleading. If the argument which we have presented carries any weight, it must be because it is difficult to see what *other* known facts (excepting beliefs and other 'subjective' states of affairs which are admittedly incapable of justifying a choice of policy) could conceivably be counted as reasons.

We next turn our attention to oracle-policies.

What would constitute a *reason* for believing an oracle? As already observed, the reason cannot consist in the *authority* which the oracle enjoys among those who consult it, or, which means the same: it cannot be the *attitude* – of love or trust or fear, as the case may be – which the oracle's consultants take to it. For facts concerning attitude or authority are not 'objective' in the sense in which we required that facts providing reasons, as opposed to motives, should be objective. But, with this distinction between reasons and motives in mind, is not the sole reason which we can imagine for believing an oracle, knowledge of the fact that it had proved to be a 'good guesser' in the past or, more precisely, proved to be a better 'guide to the truth' than alternative policies?

Granting an affirmative answer to the last question, a reason for adopting an oracle-policy is thus something which is confirmative of a certain inductive policy. *This inductive policy proceeds on the principle that the future will resemble the past with regard to the oracle's truth-producing powers.*

A certain ambiguity in the use of the term 'induction' or 'inductive policy' should be noted here. Let us assume that all *A* so far observed have been *B*, but that an oracle tells us that the next *A* will not be *B*. If, in this situation, we actually believe that the next *A* will not be *B*, it might be natural to say that we here believe the oracle *rather than induction*. Now, what would be a *reason* for believing the oracle rather than induction? If we accept the account of the concept of a 'reason' which we have given, it could only be our past experience that inductive inferences to the next case from facts that all *X*'s so far observed have been *Y*, have more often broken down than the oracle's predictions in similar situations. But this, of course, is equivalent to saying that a reason for believing the oracle rather than induction is tantamount to a reason for believing the induction from past experience of the oracle's predicting-powers rather than the induction from past experience of certain regularities in nature. In a sense, therefore, it is misleading to say that we have a reason for believing an oracle *rather than induction*. What we have a reason for doing is to trust one inductive policy rather than another.

The conclusion which emerges from the above considerations is that a *reasoned* policy for purposes of prediction and generalization is necessarily equivalent to an *inductive* policy. If we wish to call reasoned policies *better* than not-reasoned ones, it follows further that induction is of necessity the *best* way of foretelling the future.

The suggested use would certainly be *a* sensible use of 'better'.

But it is not the only sensible use of the attribute in connection with prediction and generalization. A policy might also be called good in proportion to its *successfulness*. And under *this* use of 'good' it may well be the case that a reasoned policy turns out to be inferior to a not-reasoned one. Neither with certainty nor even with 'probability' can this possibility be excluded.

With our term 'reasoned' may be compared the words 'reasonable' and 'rational'. It is noteworthy that the two latter have, so to say, a double face. One face looks to the past, another to the future. 'Reasonable' and 'rational' as attributes of a policy for prediction and generalization *may* mean 'reasoned' in our sense, i.e. grounded on past experience. But they *may* also mean the policy which will hold good in future.

Thus, depending upon which use of 'reasonable' and 'rational' we contemplate, we are entitled or not to say that induction is *ipso facto* rational. But there is no way of securing the rationality of induction under *every* sensible use of 'rational'.

Our examination of policies for predicting and generalizing has thus led us to the conclusion that the truth contained in the idea that induction is the best mode of reasoning about the unknown is — a disguised tautology. It is not that the inductive method possesses some features, *besides being inductive*, which give it a superiority over other policies. Its superiority is rooted in the fact that the inductive character of a policy is the very criterion by means of which we judge its goodness. The superiority of induction, in other words, is concealed in the *meaning* of a policy's goodness.

Our argument, it will be remembered, hinges on the assumption that the only things which count as reasons for a belief about the future are known facts which are confirmatory of some inductive policy. We shall not dispute that this assumption may be successfully challenged, although *we* see no way of doing it. But it is our thesis that with a changed conception of a 'reason' we should have to give up or modify the idea that the justification of induction consists in the superiority of inductive over rival policies.

CHAPTER IX

SUMMARY AND CONCLUSIONS

§1. *The thesis of the 'impossibility' of justifying induction.*

THE subject-matter of this treatise has been an investigation into the logical nature of the relation in which so-called inductive inferences or inductive conclusions stand to the data or grounds on which they are established. This investigation has been pursued with the chief purpose of answering the question as to whether or not it be possible in the logic of the relation mentioned to find a justification for induction as an operation on which reasoning, both in science and in every-day life, has to rely.

We have seen that the demand for a justification of induction covers — in ordinary language as well as in the records of the history of philosophy — not one but several distinct ideas, and that consequently the question mentioned can be answered in the negative or in the affirmative depending upon what we expect a justification of induction to be. We have, moreover, seen that there is one sense in which any attempt at such a justification necessarily fails. This failure consists, roughly speaking, in the impossibility of guaranteeing (with certainty or even with probability) the truth of any synthetical assertion concerning things outside the domain of our actual or recorded experience. This impossibility was first pointed out by Hume, and from the anxiety over its alleged philosophical implications has originated the 'problem of Hume' or the 'problem of induction' *par préférence*.

Before leaving our topic we must scrutinize the logical nature of the statement concerning the sense in which any attempt to justify induction fails. This statement will in what follows be referred to as the thesis on the impossibility of justifying induction. With regard to this thesis the problem of induction has been called 'the despair of philosophy'[1] and the failure to justify inductive inference has been deemed a scandal to philosophical thinking.[2] From such statements it might appear that philosophy, in attacking the inductive problem, has undertaken a task too mighty for its faculties and that the thesis under consideration was the acknowledgement of ultimate defeat in this task. Actually, however, these statements are the offspring of certain typical misinterpretations of Hume's results and of the failure clearly to apprehend the logical character of the question presented to us by the demand for a 'justification' of induction. When this is understood it is seen why the thesis on the impossibility of justifying induction is, not a scandal to philosophy, but a philosophical achievement of great importance.

The typical way of misinterpreting the thesis in question is the following: Our thesis states that in a certain and furthermore, as it seems, very important sense our efforts to justify induction have been all in vain, since such a justification is not forthcoming. Induction, in other words, is in a certain sense an unjustifiable operation. We cannot 'prove', with certainty or even probability, that the sun will rise tomorrow. It is easy to take this as implying that it were, after all, worthy of a rational man to take a sceptical attitude towards this conjecture.

On the other hand such 'sceptical' consequences of a theory of induction are repugnant to sound judgment. We revolt against ideas such as that it is not 'probable' that the sun will rise tomorrow, and as this idea was the logical outcome of a certain philosophy of induction we revolt against this philosophy as well. And we maintain with a deep moral awe that induction *must* be justifiable also in the sense with which we are here concerned, even if such a justification has not hitherto been found.

Of such an attitude towards induction there are good representatives also in recent philosophy. The following passages may be quoted from a distinguished contemporary philosopher as illustrating it. He says:

'The most important postulate of science is induction. This may be formulated in various ways, but, however formulated, it must yield the result that a correlation which has been found true in a number of cases, and has never been found false, has at least a certain assignable degree of probability of being always true.'[3] 'I am convinced that induction must have validity of some kind in some degree, but the problem of showing how and why it is valid remains unsolved . . . Until it is solved, the rational man will doubt whether his food will nourish him, and whether the sun will rise tomorrow.'[4]

This typical misinterpretation of the thesis on the impossibility of justifying induction originates from a confusion with which we are already familiar from earlier portions of the present treatise. This confusion, which is deeply rooted in the philosophical inclinations of man and which is one of the fundamental sources of philosophy as such, consists in the failure to separate from one another questions of language and questions of fact. In almost any situation where there is alleged to be a conflict between 'philosophy' and 'common sense' the conflict can be shown to be the offspring of this confusion.

It will be our final task, therefore, to show why and in what sense the above thesis on the impossibility of justifying induction is — when rightly understood—grammatical in its nature, and, as such, free from all 'sceptical' implications. We shall do this by applying a characteristic 'technique of thought' to the thesis mentioned, the essential of this 'technique' being to demonstrate that the truth in the thesis is a disguised tautology.

§2. *The logical nature of Hume's 'scepticism'*.

Let us give to the 'thesis' which we are here examining the following more specific formulation:

It is impossible to guarantee, with certainty or with probability, that an unknown instance of the property A will also exhibit the property B, if A and B are different properties.

(This formulation must not be taken to represent, *in itself*, an important result of philosophical thinking. The philosophical achievement which it has been the chief purpose of this treatise to expound does not consist in the above thesis itself but in a certain *interpretation* of it.)

The thesis states that a certain thing is '*impossible*'. It is important to observe that the phrase 'it is impossible' here means the same as 'it is contradictory'. There is also another interpretation of the phrase which suggests itself, viz. 'there does not exist'. This last interpretation need not be false if used in the appropriate way, but it should, however, be avoided for the reason that it is misleading. For, if we take 'it is impossible to justify induction' as meaning 'there does not exist a justification of induction', then this suggests that we *know* what the desired justification ought to be, although upon investigation we have not been able to *find* it. But this gives an entirely wrong picture of the logical situation. Actually the failure of the attempts to justify induction was caused by the fact that we had no clear idea as to what exactly we were seeking. Now we maintain that in clarifying the meaning of 'justification' we find that the reason why it was impossible to justify induction *in a certain sense* is that this 'sense' was a hidden contradiction.

The next task, therefore, is to show that that which the thesis asserts to be impossible is a contradiction. This is done by summarizing the chief results of the foregoing chapters into an analysis of the constituents of the thesis.

It was shown in Chapter VII that a guarantee for something with 'probability' is relevant to that which is going to happen only if it means that this 'something' is going to be true in a certain proportion of cases. The difference, therefore, between a guarantee with 'certainty' and one with 'probability' is that the former concerns the truth of a single statement, the latter the truth of a statement on the truth-frequency in a class of statements. The problem of 'guaranteeing' something as to the future is thus fundamentally the same in both cases, viz. that of assuring that a proposition, about the truth of which we are uncertain, will be true. We can, consequently, without altering the content of the thesis mentioned on the impossibility of justifying induction, omit from its formulation above the qualification 'with certainty or with probability' added to the word 'guarantee'.

(This, it must be noted, does not mean that it is *the same thing* to guarantee that *A* will be *B* with certainty and to guarantee it with probability. We merely maintain that to guarantee that *A* will probably be *B* means to guarantee the truth of another proposition itself of the same 'inductive' kind as this one. The guarantee of the truth of the second proposition can again be demanded either with certainty or with probability.)

We next turn our attention to the condition that the properties *A* and *B* ought to be 'different'. The meaning of this was analysed in Chapter II, section 2, where we distinguished between 'psychological' and 'logical' difference, the latter alone being of relevance to the problem of Hume. That two properties are logically different meant that the presence (or absence) of one of the properties does not logically follow from the presence (or absence) of the other property.

If one property entails another, then its presence, in a given situation, is a *standard* or *criterion* for the presence of this other property. (The absence of the second property again is a standard for the absence of the first.) That two properties are (logically) different therefore means that neither of them is a standard for the presence or absence of the other.

The phrase that the instance of the property *A* ought to be 'unknown' then remains to be interpreted. The first interpretation to suggest itself is the following:

An instance of *A* is 'unknown' so long as we do not know of any property which it is going to possess except *A*, which it has *per definitionem*. That the instance of *A* is 'unknown' thus implies that we do not know whether it will have the property *B* or not.

To '*know*' that an instance of the property *A* will have the property *B* can mean different things. But, as was shown in Chapter II, section 5, unless it means that the presence of *A* is taken as a *standard* for the presence of *B* (*or* the absence of *B* for the absence of *A*), then to 'know' that *A* will be *B* is not relevant to the question whether *A* 'really' is going to be *B* or not.

It follows that *not* to know whether or not *A* will be *B* must imply that the presence of *A* is *not* taken as a standard for the presence of *B*. Otherwise the interpretation of the phrase that the instance of *A* must be 'unknown' would contradict the condition that *A* and *B* are logically different.

It is, however, easy to see that even if the phrase is thus interpreted we get a contradiction. The thesis on the impossibility of justifying induction would then imply that it is impossible to guarantee that *A* will be *B* if the presence of *A* is not a standard for the presence of *B*. According to the analysis in Chapter II a 'guarantee' that *A* will be *B* can mean several things, but unless it means that we make the assertion '*A* will be *B*' analytical, the 'guarantee' is not relevant to the question whether or not the property *B* really will be present in A. On the other hand, if it is analytical that *A* is *B* then the presence of *A* is a *standard* for the presence of *B*, and the demand for a 'guarantee' becomes contradictory.

On this point the following 'objection' is likely to be suggested:

The above interpretation of the qualification that the instance of *A* ought to be 'unknown' is evidently beside the point, since on reflection it is clear that under it the demand for a justification of induction becomes a demand for knowledge about something of which, according to our own premisses, we can know nothing. But surely, in demanding a guarantee that certain things are going to be such and such in the future, we do not demand this. Therefore, an interpretation of our demand as such a self-contradictory wish does not do justice to that for which we are really asking. The true interpretation seems to be something of this kind:

In speaking of an instance of *A* as being 'unknown' we mean that certain reasons for judging the presence of *B* (and other properties) in the instance are as yet not available. These reasons, sense-perception or whatever they may be, we shall call the *experiential grounds* on which a proposition concerning the presence or absence of properties is 'verified' or 'tested'. Induction, speaking generally, is the anticipation of the results to which experiential tests will subsequently lead. To justify induction is to provide some other grounds or reasons – let us call them *inductive grounds* – which are somehow to 'rationalize' this process of anticipation. Since these 'inductive' grounds are to be different from the above 'experiential' ones, the demand for a justification of induction may perhaps be impossible to satisfy, but is surely not self-contradictory.

To this objection it is easy to reply:

What would be the logical relation, we ask, between the 'inductive' grounds and the 'experiential' ones for judging the truth of propositions on future events? Let us suppose that the former were *criteria* of the results to which an anticipation of the testimony of the latter will lead. This would mean that if I had 'inductive' reasons for anticipating that *A* will be *B* then I am bound to interpret *any* subsequent experiential information so as to accord with the anticipation. But then the proposition '*A* will be *B*' is analytical, and the presence of *A* is a standard for the presence of *B*, and the absence of *B* a standard for the absence of *A*. This again contradicts the condition that *A* and B ought to be different.

Consequently in demanding a justification of induction we cannot demand *criteria* of the truth of inductions. The remaining possibility is that we demand something which can be conveniently called *symptoms* of the experience anticipated in inductive inferences. Such 'symptoms' for the anticipation of truth may be obtained in various ways, but irrespective of how they are obtained we must know something of their *reliability* if they are to be relevant to the *truth* of inductions. A statement concerning their reliability, again, is either an *assumption* that those 'symptoms' will lead to true inductions, and as such is itself in need of justification, or it is a *guarantee* that the indications of the inductive grounds will accord with the testimonies of the experiential grounds. On the other hand we know that the sole way of guaranteeing that those indications and testimonies are going to give concordant results is to make the statement on their concordance analytical. This means that the indications of the inductive grounds are standards or criteria for the testimonies of experience, and this again contradicts the assumption that the former are only 'symptoms' of the latter.

We have thus seen that the same contradiction, which, according to the above 'objection' it was unfair to attribute to the demand for a justification of induction, is innate also in the 'objection' itself. It is the logical peculiarity of this demand that, although it *need* not be interpreted as self-contradictory, any interpretation which evades the contradiction is not relevant to the demand *in that sense of it*, in which it is to be satisfied by a solution of 'Hume's problem'.[1]

But if the demand for a justification of induction is self-contradictory, when taken in that particular sense, then the above thesis on the 'impossibility' of justifying induction is a tautology.

The view that Hume's 'sceptical' result as to the justification of induction is a consequence, not of the constitution of the world but of the use of language, can truly be said to constitute the 'solution' of the problem put to philosophy by Hume. To Hume the failure to justify induction seemed the discovery of a serious limitation in man's intellectual faculties.[2] We, in realizing what this 'failure' *means*, also understand that from the very meaning of words it follows that we can never imagine these faculties, in the aspect of them under consideration, to be greater than they are. When this is clearly apprehended, the demand for a justification of induction in the Humean sense is 'satisfied', that is to say it *vanishes* from itself as being devoid of object.

§3. *The critical and the constructive task of inductive philosophy.*

The clarification of language leading to a solution of Hume's problem can be said to constitute the *critical* task of inductive philosophy. With this can be contrasted what we shall call the *constructive* task of a theory of induction. The latter, unlike the former, is not concerned with the meaning and use of words, but with the formal peculiarities of given conceptual structures. It consists in the application of formal logic and mathematics to the analysis of inductive propositions.

As regards this constructive task the treatment in the present work has been far from exhaustive. Our contributions have to some extent been in the nature of first sketches which it will be the task of others to work out in fuller detail. We finally mention some points on which a continuation of the task undertaken here would seem to be worth while.

In Chapter IV we showed that the idea of a 'logic of induction', in that form of it which was invented by Bacon and later developed by Mill, can be profitably treated as a formal theory of necessary and sufficient conditions. This treatment reveals unexpected asymmetries and other logical peculiarities in the fundamental types of method of scientific inquiry. The examination, as pursued by us, applied specifically only to inductive propositions of certain very simple structures. It would be of interest to investigate, *inter alia*, whether the theory of necessary and sufficient conditions can be extended also to relational and quantitative laws of nature, and whether there is some analogy to such a theory among Statistical Inductions.

In Chapter VI we analysed the formal nature and interrelatedness of certain ideas concerning the probability of inductions. We endeavoured to show that those ideas can be formalized and made exact within the 'ordinary' probability-calculus. This system remains to be embellished, especially we think by a fuller analysis of the ideas of simplicity and scope in relation to the probability of inductions.

Philosophy of induction has, at least since the days of Hume, been seriously hampered in its progress by an unwholesome confusion of the two tasks of inductive theory, called by us the critical and the constructive tasks respectively. The constructive value of most efforts to develop a system of inductive logic or inductive probability has been minimized by the fact that those constructions have been undertaken for the vain purpose of solving Hume's problem. On the other hand most critical treatments of inductive philosophy have contented themselves with the easy task of showing that those constructive efforts have failed in that they did not lead to a solution of the problem mentioned, in which case one has not been able to estimate the value of the constructions in spite of their 'failure'.

It appears to us that we have now arrived at a point where the clarification of philosophical ideas has led to a completion of the critical task of inductive theory, and from which the constructive task can be pursued with a clear purpose unhampered by false philosophical pretensions and disentangled from all misleading expectations.

NOTES

NOTE — In the notes any work is usually referred to under the author's name, or if there are several works quoted by the same author, under the author's name followed by a number in brackets. The names of the works themselves are found in the Bibliography. The references and quotations apply, of course, always to the edition mentioned in the Bibliography, but the Author has in certain cases tried, by giving the number of chapters and sections instead of those of pages, to make them apply to other editions of the work also. If no special edition of any work is mentioned, the references should apply to all editions of it.

CHAPTER I. INTRODUCTORY REMARKS ON INDUCTION

§1. *Inductive inference and the problem of induction.*

[1] See, e. g. Jevons [1], p. 211 and 239.
[2] See Mill, bk. II, chap. IV, §2 and §3.
[3] Aristotle [1], 156^a5; Mill, bk. III, chap. II, §1.
[4] Mill, bk. III, chap. III, §1.
[5] To this rule there are indeed important exceptions. The way of applying the principles of probability to induction, which is characteristic of Laplace and his followers, marks an important epoch in the development of the *logical* problem of induction. (See below chaps. VI and VII.) Among works, typical of the psychologizing tendency of French writers on induction, may be mentioned: The methodological works of the great physiologist Claude Bernard, *Introduction à la médecine expérimentale* and *La science expérimentale*, Lalande's *Les théories de l'induction et de l'expérimentation*, Naville's *La logique de l'hypothèse*, and Picard's *Essai sur la logique de l'invention dans les sciences* and *Essai sur les conditions positives de l'invention dans les sciences*.
[6] Mill, bk. III, chap. I, §2: 'An analysis of the process by which general truths are arrived at, is virtually an analysis of all induction whatever' and 'we shall fall into no error, then, if in treating of induction, we limit our attention to the establishment of general propositions'. — Reichenbach [6], p. 265f.: 'Das erkenntnistheoretische Problem . . . liegt gar nicht in der Unendlichkeit der Folgen, sondern darin, dass die Folgen sich stets, über vergangene Ereignisse hinaus, auf zukünftige Ereignisse erstrecken . . . Eben darin liegt das eigentümliche Problem der Induktion; und dieses Problem wird nicht im geringsten dadurch erleichtert, dass man die Zahl der zukünftigen Ereignisse finitisiert.'

§2. *Different forms of inductive generalizations.*

[1] The use of logical symbols and formulas in this book assumes that the reader is to some extent familiar with the symbolic language of modern logic. For any reader, not acquainted with logistics, the following elementary elucidations will be added:

Let a and b be propositions. The conjunctive proposition $a\&b$ ('a and b') is true only if both a and b are true. The disjunctive proposition $a\vee b$ ('a or b') is false only if a and b are both false. The implicative proposition $a\rightarrow b$ is false only if a is true and b false. The equivalence-proposition $a\longleftrightarrow b$ is true when a and b have both the same truth-value ('true' or 'false'). The negative proposition $\sim a$ ('not-a') is true when a is false, and false when a is true.

If the truth-value of a proposition depends (uniquely) on the truth-value of certain other propositions, the former is said to be a *truth-function* of the latter. $a\&b$, $a\vee b$, $a\rightarrow b$, and $a\longleftrightarrow b$ are, according to this definition, truth-functions of the propositions a and b. The proposition $\sim a$, again, is a truth-function of a.

The proposition $a\&b$, $a\vee b$, $a\rightarrow b$, and $a\longleftrightarrow b$ are called *molecular* relative to their constituent propositions, a and b, which again are called *atomic* relative to the above compound propositions.

Of two propositions, a and b, and their negations we can form four conjunctions: $a\&b$, $a\&\sim b$, $\sim a\&b$, and $\sim a\&\sim b$. In a similar manner we can of n propositions and their negations form 2^n conjunctions. Any proposition, which is a truth-function of the n propositions, is equivalent to a disjunction of some of these 2^n conjunctions. This disjunction is called *the disjunctive normal form* of the proposition.

Ax means: the individual *x* has the property *A*. *R*(*x*,*y*) means: the pair of individuals, *x* and *y*, stand to one another in the relation *R*.

x in *Ax* may be regarded as a *constant* or as a *variable*. If *x* is a constant, *Ax* is a proposition. If *x* is a variable, *Ax* is called a *propositional-function*. The propositional-function 'becomes' a proposition when for the variable is *substituted* a constant. The constants which may be substituted for a variable are called the *values* of the variable. A value is said to *satisfy* the propositional-function when the resultant proposition is true.

(*Ex*)*Ax* means: there exists an *x* which is *A* or, in other words, at least one *x* has the property *A*. (*Ex*) is called an *existential operator*. (*x*)*Ax* means: all *x*'s are *A*. (*x*) is a *universal operator*.

The symbols used by us differ from those of *Principia Mathematica* inasmuch as that we, following Hilbert, use '&' instead of '.' as a sign for conjunction, the arrow instead of the horseshoe as a sign for implication, the double-arrow instead of '$\equiv$' as a sign for equivalence, and brackets instead of dots for combining and separating the respective parts of a symbolic sentence.

It is of some importance to observe that phrases such as 'the proposition *a*', 'the property *A*', 'the individual *x*', when they occur in this book, are not about the *symbols* '*a*', '*A*', and '*x*' but about the symbolized entities. Similarly, '*a* is false', 'every *A* is *B*', '*x* is *A*' are about entities and not about symbols.

On the other hand, a phrase such as '*Ax* means' or '(*Ex*) is called' is about *symbols* and not about things symbolized.

Usually, it is clear from the context, whether we are talking about the symbols or about their meanings. Sometimes, however, when we are talking *about* the symbols, we enclose the symbols within single quotation-marks ' ' (as, for example, on line 13 on this same page).

A good modern introduction to logic is Cooley, *A Primer of Formal Logic* (New York, 1946). An excellent text-book of a more advanced character is Hilbert-Ackermann, *Grundzüge der theoretischen Logik* (3rd edn., Berlin, 1949).

[2] Cf. Keynes, p. 220.

[3] Keynes (p. 220) calls them Inductive Correlations. Mill's Approximate Generalizations correspond roughly to our Statistical Inductions. Mill pays very slight attention to this type of inductive inference. See bk. III, chap. xx.

[4] The contrast in question is perhaps more appropriately described as one between Causal Laws and Probability-Laws, than between Universal and Statistical Generalizations. It might be maintained that induction from statistical data usually aims at the establishment of Probability-Laws, and that Statistical Generalizations accordingly are relatively unimportant in science. We shall not discuss the question here. Some writers on induction (Broad [7] and Kneale [1], §48), have tried to argue that Probability-Laws presuppose the existence of causal (nomic) connections in nature. Their arguments do not seem to us convincing.

[5] Characteristic of the elements of a series are said to be *intensionally* given when they may be calculated from a rule. Characteristics which cannot be calculated from a rule but have to be determined by empirical observation from case to case, are called *extensionally* given.

[6] For this notation cf. Reichenbach [4], p. 81 and 347.

[7] Cf. Popper [2], p. 128ff.

[8] Observe for example the series 010100110000111100000000111111111 . . . where the proportion of 1's is 'oscillating' between the limits 1/2 and 1/3.

[9] From the above expression (4) follows a proposition asserting the existence, for any

ε, of a number m such that from x_m on the frequency of A's which are B remains within the interval $p \pm \varepsilon$. See below chap. VIII, §1.

[10] The existence of empirical propositions which involve both universal and existential operators and hence are neither verifiable nor falsifiable originally presented a difficult puzzle to adherents of the so-called verificationist theory of meaning. Later, however, the criterion of meaningfulness, advocated by the logical positivists, has been gradually widened, so as finally to include also propositions with any number of both universal and existential operators in any combination. See Carnap [1].

[11] Consider, e.g. the series 100100010000100000 . . . which contains an infinite number of both 1's and 0's, but where nevertheless the limiting ratio of the number of times when 1 occurs to the total number of members in the series is zero. Sequences of this structure *may* occur in nature (the order of 1's and 0's reflecting, say, the temporal succession of results in repeated experiments or observations). It seems to us, therefore, that R. B. Braithwaite is mistaken, when he writes ([6], p. 152): 'all general statements are in fact probability statements, since to say that all A's are B's is the same thing as to say that 100 per cent of A's are B's, which is (on my thesis) the same thing as to say that the probability of an A being a B is 1. Similarly, to say that no A's are B's is to say that 0 per cent of A's are B's and to say that the probability of an A being a B is 0. Universal generalizations, whether affirmative or negative, are special cases of probability statements.' He argues (loc. cit.) that 'within a science, ascriptions of zero probability are taken to be indistinguishable from negative universal generalizations'. This, it would seem, is a confusion between what is usually the case (as a matter of fact) and what must necessarily be the case (as a matter of principle).

[12] Darwin, *On the Various Contrivances by which Orchids are Fertilized*, (London 1862), chap. V.

§3. *Remarks about various usages of the term 'induction'. Induction and eduction.*

[1] The relevant passages are *Topics* bk. I, chap. XII and bk. VIII, chap. I-II; *Prior Analytics* bk. II, chap. XXIII; *Posterior Analytics* bk. I, chap. I and XVIII and bk. II, chap. XIX. It is usually said (cf. Keynes, p. 274 and Kneale [1], pp. 24-37) that Aristotle used 'induction' in *two* senses, viz. to mean either summative induction (*Prior Analytics*) or intuitive induction (*Posterior Analytics*). It seems to us, however, that the use of 'induction' in the *Topics* must be counted as a third sense. This opinion is confirmed by the example quoted in the text, which is a clear case of so-called ampliative induction. Cf. Lalande, p. 3 and 6.

[2] [1], 105^a 12.

[3] [1], 105^a 13-15.

[4] [1], 165^a 5.

[5] For a discussion of Aristotle's account of induction in relation to the syllogism see Whewell [4], p. 449ff.

[6] [2], 68^b 19-24.

[7] Cf. Kneale [1], p. 25.

[8] [2], 68^b 29.

[9] [3], 71^a 8.

[10] [3], 100^b 12.

[11] [3], 81^b 6.

[12] Johnson [1], vol. II, chap. IX, §1.

[13] Kneale [1], p. 30.

[14] Johnson [1], vol. II, chap. VIII, §1 and vol. III, chap. II.

[15] Peirce, vol. II, especially par. 680 and 709; Lalande, p. 6; Kneale [1], p. 44.

[16] Johnson [1], vol. II, chap. VIII.

[17] For a good account of recursive induction see Kneale [1], §10.

[18] See Mach [3], p. 306.

[19] Mill, bk. III, chap. III, §1: 'every induction may be thrown into the form of a syllogism by supplying a major premiss'.

[20] Johnson [1], vol. II, chap. X.

[21] For significant examples of the use of summative induction see Mach [3], p. 305f. and Lalande, p. 8ff.

[22] Mill, bk. III, chap. II. The chapter is called 'Of Inductions Improperly So-Called'.

[23] Johnson [1], vol. III, chap. IV.

[24] Mill, bk. II, chap. III, §3 and §7.

[25] Mill, bk. II, chap. III, §5.

[26] Kneale [1], (p. 45) argues against Mill that there can be no 'inference from the observed to the unobserved without at least tacit reliance on laws'. It is not quite clear, how this shall be understood. If 'inference from the observed to the unobserved' is meant to include generalization, then Kneale's statement must be rejected as false. For *all* generalizations cannot be said to 'rely on laws', i.e. rely on some other generalizations. If again 'inference from the observed to the unobserved' means eduction only, then Kneale's statement seems to us unduly dogmatic. It is hardly possible to deny that cases of *genuine* eduction occur. At most one might say that the *rationale* of an eductive inference is a hypothetical general truth. And if this is what is meant by saying that inference from the observed to the unobserved *must* rely, tacitly at least, on laws, then Kneale's position in the matter would seem to coincide with Mill's.

[27] Carnap [11], §44B and §110, especially p. 208 and p. 574f.

[28] Carnap [11], §110F.

[29] Cf. von Wright [10], p. 364.

CHAPTER II. INDUCTION AND SYNTHETICAL JUDGMENTS *A PRIORI*

§2. *Hume's theory of causation.*

[1] See above ch. I, §2.

[2] His definition of 'cause', however, sometimes is given so as to include also necessary conditions. See Hume [3], sect. VII, pt. 2.

[3] Hume [3], sect. IV, pt. 1, and Hume [2], p. 11f.

[4] Hume [1], bk. I, pt. III, §12: 'There is nothing in any object, considered in itself, which can afford us a reason for drawing a conclusion beyond it.'

[5] Ibid.: 'Even after the observation of the frequent or constant conjunction of objects, we have no reason to draw any inference concerning any object beyond those of which we have had experience.'

[6] Hume [3], sect. 2: 'When we entertain, therefore, any suspicion that a philosophical term is employed without any meaning . . . we need but enquire, *from what impression is that supposed idea derived*?'

[7] Hume [1], bk. I, pt. III, §14.

[8] In another part of the *Treatise on Human Nature* there is, however, a most acute discussion of this problem. See Hume [1], bk. I, pt. IV, §2.

[9] Hume [1], bk. I, pt. III, §6.

[10] It has been a matter of philosophical controversy whether the relation between cause and effect is a relation between 'objects' or between 'events'. This controversy is wholly irrelevant to our discussion of the causal relation and so also to Hume's.

[11] Cf. the resemblance between Hume's thesis and the following proposition from Wittgenstein: 'Auf keine Weise kann aus dem Bestehen irgend einer Sachlage auf das

Bestehen einer, von ihr gänzlich verschiedenen Sachlage geschlossen werden.' (Wittgenstein 5, 135.) Also here we are entitled to ask what is meant by two 'Sachlagen' being 'gänzlich verschieden.' Note the qualification 'gänzlich'!

[12] We ought rather to have said: In the case of the billiard-balls, as we want to conceive of it here, and as it was conceived of by Hume, logical and psychological difference are concomitant properties. For as will be seen later (ch. III, §1) it is also possible to conceive of the case in such a way that in it cause and effect become logically connected.

[13] It is not necessary here to embark upon a discussion of the exact nature of the concepts of entailment (the 'follows from') and deducibility. The concepts are open to debate. It is sufficient for our purposes to emphasize the 'formal' or 'logical' (as distinct from 'material' or 'physical') character of the notions concerned.

[14] Cf. Braithwaite [2], pt. I, p. 467.

[15] Outbursts about the absurdity of Hume's theory, based upon typical misunderstanding of the nature of his arguments, are to be found everywhere even among recent authors. A good example is offered by Ewing's criticism of the Humean theory or the so-called 'regularity view' of causality. See e.g. Ewing, p. 111: 'If the regularity view were the whole truth, all practical life would become sheer nonsense.' Similar exclamations are abundant in the works of Reichenbach. See e.g. Reichenbach [9], p. 344ff. (Note the phrase 'intellectual suicide', ibid., p. 344.)

[16] We do not think it necessary here to introduce a definition of the concept 'analytical' or 'logically necessary'. The following three remarks will suffice to make clear the use, which we in this treatise make of the term 'analytical':

(1) Any proposition which is true *per definitionem*, is analytical.

(2) A proposition, the falsehood of which has been excluded on the ground that it would be contrary to the (correct) use of language to say the proposition is false, is analytical. It is clear that (2) includes (1).

(3) Any analytical proposition is (necessarily) true.

[17] Cassirer, E., vol. II, p. 262 — See also Church, R. W., p. 210ff.

[18] Natorp, p. 127-163.

[19] See Revue de Métaphysique et de Morale *40*, 1933, suppl. *1*, p. 15f., where William of Auvergne is quoted, and the possibility that he had influenced Hume is mentioned.

[20] Hobbes, p. 15ff.

[21] Malebranche, Quinzième Éclarcissement. Malebranche also uses the example of the billiard-balls.

[22] Leibniz [2], §26.

[23] Leibniz [1], vol. IV, p. 161f.

[24] Kaila [6], p. 112. Cf. Mill's use of the term 'empirical law', bk. III, ch. XVI.

[25] Mill, bk. III, chap. XII, §1.

[26] Apelt [2], p. 53: 'Wie sind synthetische Urtheile *a priori* möglich? So lautet das Humesche Problem, wenn man es allgemein auffasst.' Cf. Hobart, pt. I, p. 284. Hume himself, however, regarded causality as the only relation by means of which, as he says, 'we can go beyond the evidence of our memory and senses'. (Hume [3], sect. IV, pt. 1.)

§3. *Kant and Hume.*

[1] The development of Kant's views as to these problems, which may be said to constitute the central question in the *Kritik der reinen Vernunft*, can be followed in the *Reflexionen*.

[2] Kant [1], §291, §292, and §726.

[3] See e.g. the definition of 'Wahrnehmungsurtheile' and 'Erfahrungsurtheile' in Kant [3], §18.

[4] See e.g. Kant [3], §17. A slightly different form of the same question is 'Wie ist Natur selbst möglich?'. (Kant [3], §36). Cf. ibid.: 'Die Möglichkeit der Erfahrung überhaupt ist also zugleich das allgemeine Gesetz der Natur, und die Grundsätze der erstern sind selbst die Gesetze der letztern.' In a certain sense, therefore, we are with Kant entitled to use the word 'Natur' as synonymous with 'Erfahrung'.

[5] Another important factor in this change in Kant's opinion was that he always seems to have held mathematical judgments to be synthetical. (Kant [1], §496.) When this had to be reconciled with the aprioristic nature of those judgments he turned to the doctrine of synthetical judgments *a priori.*

[6] Kant [3], Vorwort.

[7] Cf. Cassirer, E., vol. II, p. 526ff. and 534.

[8] Kant [2], p. 232ff.

[9] Whitehead [2], p. 34.

[10] Kant [2], p. 237f.

[11] For a more detailed analysis of Kant's causal theory of time see Mehlberg, especially pt. I, pp. 135-158. For the importance of reversibility and irreversibility, respectively, in series of sensations as regards the logical 'Aufbau' of the physical world, see Kaila [4], p. 29-63.

[12] They are, however, not causal laws according to the terminology followed in this treatise. (See chap. I, §2 and chap. II, §2.)

[13] The idea that the so-called physical world is a system of invariances or laws prevailing in the phenomenal world is very well stressed by Kaila in several of his writings. See Kaila [3], p. 81ff., Kaila [4], p. 14ff., and Kaila [6], p. 89.

[14] Cassirer, E., vol. II, p. 530f.: 'Die Natur ist . . . der Inbegriff ihrer allgemeinen Gesetze.' Helmholtz [2], p. 39: 'Das Gesetzmässige ist daher die wesentliche Voraussetzung für den Character des Wirklichen.'

[15] See Kaila [6], p. 228.

[16] Leibniz [3], vol. VII, p. 319ff.

[17] Actually all that could possibly follow from the 'deduction' as outlined above is that causal laws must exist in order for time to be possible. But this is not the same as stating that the Universal Law of Causation must be true, which is a much 'stronger' assumption. Now this peculiarity of the 'deduction' seems, in a way, to be attached also to Kant's own arguments, and is thus not to be attributed to an incompleteness in our restatement of them.

[18] Kant [2], 1st edn., p. 189. In the second edition (p. 232), we read only: 'Alle Veränderungen geschehen nach dem Gesetze der Verknüpfung der Ursache und Wirkung.'

[19] Kant [2], p. 102ff.

[20] Kant [3], §21.

[21] Kant [2], p. 102ff.

§4. *Kant and the application-problem.*

[1] For the following it is irrelevant whether we have tried to establish this law as a synthetical or as an analytical principle. Actually several philosophers, in insisting upon the aprioristic nature of the Universal Law of Causation, have regarded it as an analytical principle. According to Mach causal relatedness is a kind of functional relatedness. So long as the class of functions, which are to connect cause and effect, is not specified, the Universal Law of Causation becomes tautological. See Jourdain and Mach [3], p. 270-281. See also Helmholz [1], vol. III, p. 26ff. Helmholz's view is an intermediate between Kant's and Meyerson's and illustrates very well how the theory about the

aprioristic nature of causality is driven to conventionalism, i.e. to accept the *a priori* principles as analytical. Cf. later chap. II, §6 and chap. III, §2.

[2] Cf. Cassirer, E., vol. III, p. 92f.: 'Der allgemeine Satz der Ursächlichkeit enthält kein Merkmal und gibt kein Kriterium an, kraft dessen wir die besonderen Fälle seiner Anwendbarkeit erkennen und ihm subsumieren können. Ich weiss aus dem Grundsatze zwar, dass *Objekte der Erfahrung überhaupt* in Kausalverbinding miteinander gedacht werden müssen, keineswegs aber, dass *eben diese Objekte* es sein müssen, die in diesem Verhältnisse stehen.'

[3] Kant [2], p. 165.

[4] Cassirer, E., vol. III, p. 93, observes acutely on this point that 'das *Humesche Problem* steht vor neuem vor uns'.

[5] Cf. Maimon, p. 190f.: 'Kant nimmt den wirklichen Gebrauch der Kategorien von empirischen Objekten als ein unbezweifeltes Faktum an.' This, in our opinion, hits the nail on the head. The application-problem never seriously bothered Kant.

[6] Kant [3], §19, §20, §22, §29.

[7] Kant [3], §18 and §22. Cassirer, E., vol. II, p. 525: 'Auch das Erfahrungsurteil als solches enthält eine eigentümliche "Notwendigkeit".' As to Kant's use of the term 'Notwendig', see Reinach, Kant [2], p. 279ff., and Kant [3], §19.

[8] Maimon, p. 190ff. Maimon uses Kant's example from the *Prolegomena* of the relation between the radiation from the sun and the increase in temperature in a stone touched by the rays. See also Maimon, p. 420: 'Daraus, dass Objekte überhaupt z.B. im Verhältnisse von Ursache und Wirkung gedacht werden müssen, wenn eine Erfahrung überhaupt möglich sein soll, lässt sich noch nicht begreiflich machen, warum z.B. eben das Feuer und die Wärme in diesem Verhältnisse stehen müssen?'

[9] Cassirer, W. H., p. 110. Cassirer emphasizes that Kant's views as to the application-problem had undergone a radical change from the *Kritik der reinen Vernunft* and the *Prolegomena* to the *Kritik der Urtheilskraft*.

[10] Kant [4], Einleitung, IV, and Kant [6], p. 22ff.

[11] Kant speaks of such principles under the names 'lex parsimoniae', 'lex continui in natura' and others. All these are modifications of a more general principle, which he calls the principle of 'die Zweckmässigkeit der Natur'. Kant [4], Einleitung, V, and Kant [6], p. 22ff.

[12] Kant [4], Einleitung, IV, and Cassirer, W. H., p. 109.

§5. *The inductive problem in the school of Fries.*

[1] Kastil, p. 29: 'Dieser vielgepriesene "transzendentale Beweis" . . . läuft also eigentlich darauf hinaus, die synthetischen Urteile *a priori* zu analytischen zu machen.'

[2] Kastil, p. 296.

[3] Cf. Popper [2], p. 52.

[4] Fries [1], vol. I, p. 27 and 35ff.

[5] Fries [1], vol. I, p. 28.

[6] Fries [1], vol. I, p. 21 and Kastil, p. 31ff. For the theory of synthetical judgments *a priori* in its Neo-Friesian form see especially Nelson [1] and [2].

[7] Kastil, p. 31.

[8] Cf. Nelson [2], p. 532: 'Die metaphysische Erkenntnis ist eine Erkenntnis allgemeiner Gesetze, und allgemeine Gesetze werden *a priori* erkannt. Die *Erkenntnis* der allgemeinen Gesetze ist aber nicht selbst wieder ein allgemeines Gesetz, sondern ein individuelles Faktum. Individuelle Fakta aber werden *a posteriori* erkannt. Also wird auch das Faktum der unmittelbaren Metaphysischen Erkenntnis nicht *a priori* sondern *a posteriori* . . . erkannt.'

[9] A theory of synthetical judgments *a priori* very similar to that of Fries and the Neo-Friesians has been held by the Oxford philosopher Cook-Wilson and his adherents.

[10] Apelt [1], p. 44 and p. 56ff.

[11] Apelt [1], p. 73 and p. 91ff.

[12] Apelt [1], p. 92: 'Gesetze . . . sind allgemeine und notwendige Wahrheiten d.h. Wahrheiten *a priori*.' Ibid., p. 106: 'In der Natur sind die physischen Gesetze von gleicher Notwendigkeit wie die mathematischen und philosophischen.'

[13] Apelt [1], p. 74f.: 'Die Induction bringt also nur die Untersätze des theoretischen Lehrgebäudes. In der vollendeten Theorie müssen diese Untersätze auf doppelte Weise festgestellt werden: einmal... deductiv, d.i. als Lehrsätze, die durch systematische Ableitung aus ihren Principien folgen, das anderemal inductiv als Erfahrungssätze.'

[14] Apelt [1], p. 72.

[15] Apelt [1], p. 56: 'Die Induction ist . . . der Weg zu der Verbindung der notwendigen Wahrheiten mit den zufälligen Wahrheiten.'

[16] Apelt [1], p. 101.

[17] Apelt [1], p. 77ff.

[18] Apelt [1], p. 95f.

[19] It is of interest to observe that Maimon, after having shown that Kant's synthetical judgments *a priori* are not sufficient to establish the truth of single inductions, (Maimon, p. 382), tried to 'complete' the theory of Kant in roughly the same way as the Friesians, i.e. by determining the synthetical principles *a priori* so that special inductions follow from them. Thereby he also approached the conventionalistic attitude. For a confirmation of this interpretation of Maimon's view see, e.g. the way in which he tries to prove that iron necessarily must be attracted by a magnet. This seems in some way to follow from the very definition of what a magnet is. Maimon, p. 255: 'In diesem Urtheile z.B.: der Magnet zeiht das Eisen an sich, wird das Eisenziehen . . . als etwas eingesehen . . . zu dessen Bewusstsein wir nicht eher gelangen, als wir zum Bewusstsein des Magnetes an sich gelangt sind; und so ist es mit allen Objekten der Erfahrung der Fall, deren Subjekte gegeben, und deren Prädikate nach und nach durch *Abstraktion* gefunden werden.'

§6. *Some other theories of causation.*

[1] Cf. Whitehead [1], p. 55: 'It is impossible to over-emphasize the point that the key to the process of induction, as used either in science or in our ordinary life, is to be found in the right understanding of the immediate occasion of knowledge in its full concreteness.'

[2] Kerby-Miller, p. 177, Whitehead [3], p. 26 and p. 251, Kelly, p. 22, Meyerson [2], p. 67, Bosanquet [1], vol. I p. 135, Bradley, p. 546f.

[3] Whitehead [2], p. 39.

[4] Hume [3], sect. 4, pt. I.

[5] Kaila [5], p. 27ff.

[6] For a theory of causality slightly approaching that one of Whitehead see Russell [5]. For a criticism of the theory about causal perception see Ayer's rejoinder to Russell's paper, especially p. 274.

[7] We do not maintain that the interpretation in question answered to the intentions of Whitehead himself. See also Robson and Gross.

[8] Meyerson [2], p. 136: 'Nous avons expliqué le phénomène, le changement, en déduisant le conséquent de l'antécédent, en montrant que le conséquent était nécessairement tel qu'il a été, ne *pouvait pas* être différent de ce qu'il a été, parce qu'il se trouvait déjà implicitement contenu dans cet antécédent.'

[9] Kelly, p. 24f.: 'The effect is identical with the cause, for after all it is the cause explicated, brought out from the fields, but itself unchanged.' The meaning of 'identity' here is complicated by the fact that Meyerson often also considers the causal relation from the point of view of *quantitative* equivalence (identity) between cause and effect. One of the reasons why we speak of cause and effect as 'different' from each other, obviously, is that they occur at different time-points. The time-factor involved in causality has presented some difficulties for the doctrine that cause and effect are 'identical'. See Meyerson [1], the chapter called L'élimination du temps. Also Renouvier, p. 26 and Bosanquet [1], vol. I, p. 258.

[10] Cf. Joseph, p. 409: 'For the causal relation which connects a with x connects a cause of the *nature a* with an effect of the *nature x*. The connection is between them *as a* and x, and therefore must hold between any a and x, if they really are a and x respectively.'

[11] Cf. Bosanquet [1], vol. I, p. 174: 'If, in an alleged causal nexus, the alleged effect is sometimes absent while the alleged cause is present, *ceteris paribus*, it is impossible that the alleged cause should be the real cause of the effect in question.' See also ibid., p. 255: 'Same effect, in the same form, same cause.' A similar view upon causality was advocated by Lotze.

[12] Blumberg [2], especially p. 76ff.

[13] Cf. Bosanquet [1], vol. II, p. 2.

[14] Mill's definition of a natural kind is this: 'By a Kind ... we mean one of those classes which are distinguished from all others not by one or a few definite properties, but by an unknown multitude of them: the combination of properties on which the class is grounded, being a mere index to an indefinite number of other distinctive attributes.' (Mill, bk. IV, chap. VI, §4.) The doctrine of Kinds as it occurs with Mill has nothing to do with synthetical judgments *a priori*. Cf. Mill, bk. III, chap. XXII, §7. For the connection between Mill's theory of Kinds and the theory of Concrete Universals see Acton. With the theory of Kinds and Concrete Universals are connected Lachelier's ideas on induction and final causes. The best statement of the theory of Natural Kinds and its relevance to induction is found in Broad [1], pt. I.

[15] Bosanquet [3], p. 8.

§7. *General remarks about synthetical judgments* a priori.

[1] I.e. what follows immediately is that there does not exist an A which is not B. As is well known the equivalence between this proposition and 'all A's are B' has been a matter of controversy among modern logicians. This controversy, however, is of no relevance to the context to which our reasoning applies.

[2] Kastil, p. 248: 'Notwendig heisst einmal das, dessen Gegenteil einen inneren Widerspruch enthält ... Es gibt aber noch eine andere ... Notwendigkeit; nämlich dann, wenn das Gegenteil einer anderen, *sonst schon feststehenden* Wahrheit widerstreitet ... Urteile die im ersten, logischen, Sinne notwendig sind, sind analytische; Urteile, die im zweiten Sinne notwendig sind, sind synthetische. Denn was in diesem zweiten Sinne des wortes notwendig ist ... dessen Gegenteil ist logisch möglich.'

CHAPTER III. CONVENTIONALISM AND THE INDUCTIVE PROBLEM

§1. *The way in which conventions enter into inductive investigations. Some examples.*

[1] Cf. Mill, bk. III, chap. x, §2.

[2] Cf. Schuppe, p. 242.

[3] This is why we do not want to call the use of the word 'phosphorus' *ambiguous* in the strict sense of this term. We have not used the word to *mean* two different things.

We have not expressed any definite opinion as to the 'real' meaning of that word at all, and thus it comes about that it is not clear what the word actually is intended to cover. Cf. Britton's (p. 183ff.) treatment of induction and conventionalism.

[4] Cf. Cornelius [1], p. 291.

[5] Cf. Poincaré [3], p. 189. Poincaré, however, does not see that this leads to a point which is relevant also to the conventionalistic arguments.

[6] Cf. the following observation of Jevons in speaking about classification (Jevons [2], p. 675): 'Now in forming this class of alkaline metals, we have done more than merely select a convenient order of statement. We have arrived at a discovery of certain empirical laws of nature, the probability being very considerable that a metal which exhibits some of the properties of alkaline metals will also possess the others.'

[7] The example of the melting-point of phosphorus was used for the purpose of illustrating conventionalistic lines of thought for the first time by Milhaud (p. 280ff.). The same example is mentioned by Le Roy ([2], p. 517) and discussed by Poincaré in several places. See Poincaré [2], p. 235ff. and Poincaré [3], p. 189f. Cornelius uses for the same purpose a slightly different example, also from chemistry. See Cornelius [1], p. 289ff. and Cornelius [2], p. 211.

[8] This idea is expressed clearly by Schuppe, p. 240: 'Sind die Bedingungen eines Ereignisses erst vollständig erkannt . . . natürlich in aller Vollzähligkeit und ohne behindernde andere Umstände . . . das Ereigniss muss unter allen Umständen eintreten.'

§2. *Conventionalism as an 'elimination' of the inductive problem.*

[1] Mill, bk. III, chap. x, §5 and chap. xi, §1. Whewell [4], p. 453. See also Fowler, p. 14 and Berlin, p. 90.

[2] Whewell [3], p. 36: 'The question really is, how the Conception shall be understood and defined in order that the Proposition may be true.' Ibid., p. 39: 'The business of definition is part of the business of discovery.' Ibid., p. 70: 'Induction is . . . the process of a true Colligation of Facts by means of an exact and appropriate Conception.' Ibid., p. 73: 'Thus in each inference made by Induction, there is introduced some General Conception, which is given, not by the phenomena, but by the mind.' See also Whewell [4], p. 253ff. Although Whewell's ideas can in part be interpreted conventionalistically, he himself was of the opinion that the truth to which induction leads, in so far as it is absolute, is a kind of synthetical truth *a priori*. For Whewell's ideas about synthetical judgments *a priori* see the very lucid account in Whewell [1], vol. I, p. 53-75. (Also Whewell [2], vol. I, p. 57-76.) It is most interesting to see, how easily Whewell's 'fundamental ideas', i.e. the general synthetical and *a priori* principles, can be understood in a conventionalistic way. This was pointed out — without the use of the terms analytical or conventionalistic — already by Boole, in an extremely interesting passage in *The Laws of Thought*. (Boole [1], p. 406.). About the general idea of order and uniformity in nature Boole, quoting Cournot [2], says (ibid.) that 'it carries within itself its own justification or its own control, the very trustworthiness of our faculties being judged by the conformity of their results to an order which satisfies the reason.'

[3] Bacon [2], lib. I, aph. CV: 'Atque huius inductionis auxilio, non solum ad axiomata invenienda, verum etiam ad notiones terminandas, utendum est. Atque in hac certe Inductione spes maxima sita est.' Cf. Ellis [1], p. 37.

[4] Jevons [2], p. 675. For the question about classification, induction, and convention see also Mill, bk. IV, chap. vii. The question is connected with that of the existence of Kinds. See above chap. ii, §6.

[5] Mach [3], p. 307ff.

[6] Sigwart, vol. II, p. 451.

[7] Broad [1], pt. II, especially pp. 17f., 32f., and 34f. The discussion is of Natural Kinds.

[8] Mach [3], p. 307.

[9] See above chap. I, §3 and Keynes, p. 274.

[10] See e.g. Poincaré [1], p. 110ff. and 135ff. for conventions in the foundations of physics, and ibid., p. 92ff. for convention and geometry. For a further development of some of the ideas of Poincaré see Lenzen, p. 259ff.

[11] That Poincaré regarded the essential problem of induction as a question upon which conventionalism had no bearing is seen from several statements of his. (E.g. Poincaré [1], p. 6. and p. 167ff.)

[12] The arguments given here do not pretend to be identical throughout with the opinions of all the philosophers, whom we call 'radical conventionalists'. They are meant to characterize a certain attitude towards the problem of induction, and the philosophers separately mentioned may in minor points differ from the attitude which is here 'typified'.

[13] With this might be compared the ideas of J. R. Weinberg ([1], p. 108 and p. 157ff.). Weinberg's arguments seem to aim at something similar to the lines of thought developed by us here, but are somewhat obscured by his reference to the 'neo-positivistic' idea that general propositions are not 'propositions,' but 'schemes for the construction of propositions'. For this doctrine of general propositions and its bearing on the inductive problem see also Blumberg [1], p. 581, Ramsay, p. 237ff., and Schlick [2], p. 151.

[14] Poincaré was aware of the perplexing features in the not always easily perceptible transition from synthetical to analytical, which takes place when conventions originate. (Poincaré [1], p. 110ff. and *passim*.)

[15] The experiential instances, which recommend the adoption of a new convention and the dismissal of a previous one might be called *renegade instances*. For this name and its use see Aldrich.

[16] See Le Roy [1] and [2] for a fuller account of his ideas. A (partly very good) criticism of Le Roy is to be found in Poincaré [2], p. 213-247.

[17] For the relatedness of the ideas of Ajdukiewicz to those of Le Roy see Ajdukiewicz, p. 260.

[18] Schuppe's theory of what he calls 'rationale Induktion' (Schuppe, p. 310ff.) is a beautiful example of the view that induction *in so far as it is to have scientific value* must lead to absolutely true and consequently analytical propositions. Induction which does not lead to absolute truth, i.e. all induction which attains synthetical generalizations, he calls 'nichtig'. (Ibid.).

[19] Cornelius expounds his system of radical conventionalism in Cornelius [1], p. 277-299 and Cornelius [2], p. 210ff.

[20] See Dingler [1], p. 6; [2], p. 135ff.; [3], p. 178ff. (here on p. 180 it is very clearly stated that the *semper et ubique* of the natural laws is a tautologous property of theirs): [4], p. 25; [5], *passim*; and [6], p. 340ff. For a criticism of Dingler's ideas see H. Weinberg and v. Aster-Vogel.

[21] Dingler and his adherents do not, for reasons which are connected with this peculiarity of his system, want to call it *conventionalistic* at all, but give it the name 'Dezernismus'. (See Krampf, p. 45ff. where the difference between the 'Dezernismus' and conventionalism in the usual sense is stressed.) Nevertheless what is said here about conventionalism and the inductive problem applies *also* to Dingler's theory of induction. Cf. H. Weinberg, p. 40.

§3. *Conventionalism and prediction.*

[1] Poincaré was quite clear as to the importance of this in the question of whether conventionalism can justify induction or not. Laws of nature can always be kept true if we regard them as definitions, but that these laws are also used for making predictions is not justified by this. (Poincaré [2], p. 236.) I may for instance regard Galileo's law concerning falling bodies as analytical, but *this* does not contribute anything to the reliability of my predictions as to how a certain body in a given case is going to fall. The law which gives reliability to such predictions is no convention. 'Il ne me servirait à rien d'avoir donné le nom de chute libre aux chutes qui se produisent conformément à la loi de Galilée, si je ne savais d'autre part que, dans telles circonstances, la chute sera *probablement* libre ou *à peu pres* libre. Cela alors est une loi qui peut être vraie ou fausse, mais qui ne se réduit plus à une convention.' Ibid., p. 237f.)

[2] Poincaré [2], p. 238.

[3] Roughly the same objection against radical conventionalism as the one put forward by us, is the following which has been made by several philosophers. We can guarantee the absolute truth of inductive generalizations by making them definitions. But how do we know that definitions derived from past experiences will be suitable for new experiences also? This is a question which must be decided by the new experiences themselves. These experiences may 'correct' our definitions or suggest the adoption of new ones instead of the old. Nevertheless we know that this process of altering the definitions does not take place from case to case, but that the definitions which we employ have a certain 'stability'. To account for this stability is beyond the power of conventionalism. It is the inductive problem recurring. For these ideas see Sigwart, vol. II, p. 451ff., Mach [3], p. 139f., Meinong [1], p. 637ff., and Lenzen, p. 262. For a conventionalist's way of dealing with these objections see Cornelius [1], p. 292ff.

§4. *Conventionalism and the justification of induction.*

[1] Cf. H. Weinberg, p. 40: 'An der Tatsache, das wir "prophezeien", Naturereignisse voraussagen können, scheitert aller (Konventionalismus und) Dezernismus.'

[2] This observation underlies the following very good remark of H. Weinberg (p. 40) in his criticism of Dingler's theory of induction: 'Gewiss, *wenn* eine auf Minute und Sekunde vorausgesagte Sonnenfinsternis nicht oder nicht pünktlich eintritt, so werden wir diese "Störung" wahrscheinlich "exhaurieren".' ('Exhaurieren' is Dingler's expression for the process of completing the formulations of inductive laws by considering new circumstances.) 'Aber es *war* doch schon unzähligemal möglich eine derartige, sich bestätigende Vorausbestimmung zu treffen.'

[3] The word 'compelled' is used here to mean *logical* and not *psychological* compulsion.

[4] It is, for instance, very interesting to note that Whewell who, as was shown above, closely approaches conventionalistic lines of thought in his doctrine of inductive truth, *at the same time* underlines the importance of testing inductions, by making predictions from them. 'It is a test of true theories not only to account for, but to predict phenomena.' (Whewell [3], p. 70.)

[5] This point was emphasized also by Poincaré. See e.g. Poincaré [2], p. 239: 'Quand une loi a reçu une confirmation suffisante de l'expérience . . . on peut l'ériger en *principe*, en adoptant des conventions telles que la proposition soit certainement vraie.'

[6] Cf. above p. 45.

[7] We are here touching upon a point which is at the same time one of the deepest sources of certain metaphysical ideas about language and knowledge. Of these ideas the metaphysical systems of Bergson and Le Roy, for example, are good exponents.

NOTES

CHAPTER IV. INDUCTIVE LOGIC

§1. *Justification* a posteriori *of induction.*

[1] Cf. above chap. II, §1.

[2] A full proof of this statement would take us into general considerations about the nature of logic, which it does not seem appropriate to introduce in this treatise.

§2. *Induction and discovery. Induction and deduction as inverse operations.*

[1] Mill, bk. III, chap. I, §2.

[2] Jevons [2], p. 122ff.

[3] Ibid., p. 139. Jevons's idea that there is no other method of discovering the function in question is, however, false. Actually the function can be written down directly on the basis of the given truth-possibilities according to a rule.

[4] Already before Jevons, Tissot had mentioned induction and deduction as *inverse* operations. (Tissot, p. 248.) Jevons's account of induction has been criticized by various authors, thus Venn ([2], p. 361), Meinong ([1], p. 656), and Erdmann (p. 710ff.). Those critics, however, tend to overlook that Jevons, in calling induction the inverse of deduction, did not intend to attribute to inductive reasoning syllogistic powers. The lengthy criticism of Erdmann is based on a complete misinterpretation of Jevons's opinion on this point.

[5] Cf. Whewell [3], p. 64 and [4], p. 456 and Liebig, p. 20ff. See also Popper [2], p. 207ff.

[6] Whewell [3], p. 103.

[7] Apelt [1], pp. 142-9 and 189-204; Mill, bk. III, chap. II, §4; Whewell [3], p. 75.

[8] For the 'Ökonomie des Denkens' see Mach [1], p. 452ff. For induction and the economic nature of thinking see also Kaila [6], p. 18ff.

[9] For this resemblance see Couturat [1], p. 265ff.

[10] For a further description of this method see Mach [3], p. 252ff. The invention of the method is attributed to Plato.

[11] Cf. Cassirer, E., vol. I, p. 136ff. It appears from this context that the description of the method of science given by Zabarella were almost identical with Galileo's description of his 'resolutive' method.

[12] Galileo, vol. II, p. 21.

[13] See Couturat [1], p. 265ff.

[14] The best account of Whewell's inductive logic is to be found in Whewell [3], bk. II, chap. V and VI. Ideas similar to those of Whewell are expounded by Sigwart (vol. II, p. 434ff.) in his talk of induction as a 'Reduktionsverfahren'. Cf. also Trendelenburg's account of induction. (Trendelenburg, vol. II, p. 316ff.)

[15] Whewell [3], p. 105.

[16] For a description of the inductive tables see ibid., p. 100. An inductive table of astronomy and another of optics is given ibid. after bk. II, chap. IX.

[17] Ibid., p. 114.

[18] Ibid., p. 115.

[19] Ibid., p. 75.

[20] Whewell (ibid., p. 64) speaks of discoveries of science as 'happy Guesses'.

[21] Ibid., p. 98, Whewell explicitly says that his system of inductive logic *justifies* induction.

[22] This most important feature of certain common types of inductive logic has been stressed by Kaila ([6], p. 97). Cf. also above chap. I, §2.

[23] Mill, bk. III, chap. II, §2 and §4. Although Whewell's philosophy of induction on the whole gives a clearer picture of the actual procedure of science than the induc-

tive logic of Mill, one can hardly deny that Mill in his criticism of Whewell was right as regards most points of importance.

[24] This follows from the definition of (ampliative) induction given above (chap. I, §1).

[25] Cf. Mill, bk. III, chap. II, §3. 'The only real induction concerned in the case, consisted in inferring that because the observed places of Mars were correctly represented by points in an imaginary ellipse, therefore Mars would continue to revolve in that same ellipse; and in concluding . . . that the position of the planet during the time which intervened between two observations, must have coincided with the intermediate points of the curve.'

[26] As Stoll rightly points out (p. 91) Whewell never realized the significance of this. He seems to have assumed that the verification, which shows that the given data follow from the law, was sufficient to establish the truth also of the law as a general proposition. This is explicitly stated in Whewell [4], p. 454, where he says of an inductively obtained proposition 'no one doubts its universal truth', but adds parenthetically to this 'except . . . when disturbing causes intervene'. The last clause indicates why Whewell regarded the conclusion as to the *universal* truth of the induction as justified: this truth was to be guaranteed by a convention that if there was a fact apparently contradicting the law, then it was to be 'explained away' in some way. That this was the opinion of Whewell is confirmed from Whewell [3], p. 234ff., where he at some length discusses the use of induction for predicting new facts. The law, it seems, can never be 'falsified', but it can under circumstances be 'corrected', i.e. formulated more accurately. (Ibid., p. 235.) These passages clearly show the above-mentiond relatedness of Whewell's inductive philosophy to conventionalistic lines of thought. (See above chap. III, §2.). See also Whewell's criticism of Newton's view of induction (Whewell [4], p. 196ff.), where he against Newton, who maintained that inductive propositions are never secure of exception (Newton [1], lib. III, reg. phil. IV), says that 'to judge thus would be to underrate the stability and generality of scientific truths'.

§3. *The idea and aim of induction by elimination.*

[1] Bacon [1], vol. I, p. 137.

[2] Ibid.

[3] Cf. Mill, bk. III, chap. III, §3: 'Why is a single instance, in some cases, sufficient for a complete induction, while in others, myriads of concurring instances, without a single exception known or presumed, go such a very little way towards establishing a universal proposition? Whoever can answer this question . . . has solved the problem of induction.'

[4] This distinction is equivalent to that between *induction by confirmation* and *induction by invalidation*. See Nicod, p. 219ff. and p. 222.

[5] Keynes (chap. XXXIII) has made a first attempt to extend the general principles of the eliminative method to Statistical Generalizations.

[6] For the following considerations it is irrelevant whether the individuals are objects or events. Cf. chap. II, §2, fn. 10.

[7] Cf. above chap. I, §2.

[8] It is to be observed that the main task of the inductive logic of Bacon was to devise a method for the discovery of necessary *and* sufficient conditions of given characteristics. According to Bacon the business of induction was to find the 'form' of a given 'nature'. That the 'form' is a necessary and sufficient condition of the 'nature' is seen from the following statement (Bacon [2], lib. II, aph. IV.): 'Etenim Forma naturae alicujus talis est ut, ea posita, natura data infallibiliter sequatur. Itaque adest perpetuo quando natura

illa adest . . . Eadem Forma talis est ut, ea amota, natura data infallibiliter fugiat. Itaque abest perpetuo quando natura illabest'. That is to say: the form implies universally the nature and *conversely*.

[9] C. D. Broad, in [8], was the first author to deal with the logic of eliminative induction within the framework of a Logic of Conditions. Broad's paper marks an important advance in the logical study of induction.

[10] Bacon [1], vol. I, p. 137. Cf. Ellis [1], p. 23: 'Absolute certainty is . . . one of the distinguishing characters of the Baconian induction.' See also Keynes, p. 267.

[11] Bacon [5], vol. III, p. 618. Mill (see bk. III, chap. III, §3; chap. IV, §3; and chap. IX, §6) also attributed absolute certainty to his inductive method. As will be seen later, however, it is not clear in which of the three senses mentioned Mill spoke of inductive conclusions as being 'certain'.

§4. *The mechanism of elimination.*

[1] We do not wish to commit ourselves on the question, whether the totality of properties of a given individual can be said to determine a set. We must not speak of the set X_1, X_2 . . . as *the* set of properties of x (beside A). For the description of the mechanism of elimination given in this section it suffices to assume that X_1, X_2 . . . is *a* set properties of x.

[2] This explanation of the meaning of 'independent' is somewhat unprecise but suffices for our present purpose. Cf. von Wright [11], p. 42f.

[3] For the notions of positive and negative analogy see Keynes, p. 223ff. If we consider the three sets of properties A, B, C; A, B, D; A, C, E, then, according to the definition given, the positive analogy between the sets is (the set, the only member of which is) the property A, and the negative analogy is (the set, to which belong) the properties B, C, D and E.

[4] Nicod, p. 229. Nicod is the first author, who has clearly seen the seriousness of the restrictions to the logical power of the eliminative method which follows from possible Complexity of Conditions.

[5] The possibility that the necessary condition of A is a *conjunction* of properties does not concern us. If the presence of B *and* C is necessary for the presence of A, then the presence of B is necessary for A *and* the presence of C is necessary for A. In symbols: $(x)\ [Ax \rightarrow Bx \& Cx] \equiv (x)\ [Ax \rightarrow Bx]\ \&\ (x)\ [Ax \rightarrow Cx]$. A conjunctive necessary condition is thus a case of Plurality and not of Complexity of Conditions.

[6] Mill did not distinguish between Plurality and Complexity of Conditions (Causes). From failure to make this distinction arise several mistakes in his account of induction. It is noteworthy that at least in one place (bk. III, chap. x, §3) he considers the possibility (in our terminology) of a disjunctive necessary condition. He, however, mistakenly describes it as a case of Plurality of Conditions. Cf. von Wright [11], p. 161f.

[7] A detailed description of the working of the logical mechanism of elimination in this case is given in von Wright [11], pp. 102-16.

[8] Cf. von Wright [11], p. 94f.

[9] The corresponding 'typical' case never occurs in the search of necessary conditions. For it is a characteristic logical difference between sufficient and necessary conditions of a given phenomenon A that the presence of A entails the presence of *all* its *necessary* conditions, but that A may very well occur in the absence of some (or maybe even all) of its *sufficient* conditions. For a more detailed account of the Logic of Conditions see von Wright [11], pp. 66-77.

[10] See above p. 64 and the present section fn. 1.

[11] The possibility that the sufficient condition of A is a disjunction of properties does

not concern us. If the presence of *B or C*, 'no matter which one', is sufficient for the presence of *A*, then the presence of *B* is sufficient for the presence of *A and* the presence of *C* sufficient for the presence of *A*. In symbols: $(x)\ [Bx \vee Cx \rightarrow Ax] \equiv (x)\ [Bx \rightarrow Ax]\ \&\ (x)\ [Cx \rightarrow Ax]$. A disjunctive sufficient condition is thus a case of Plurality and not of Complexity of Conditions. Cf. above fn. 5.

[12] Cf. Nicod, p. 229.

[13] Mill's recognition of complexity in sufficient conditions (causes) enters in the form of the reservation which he (sometimes but not always) makes when, speaking of his Method of Difference, he says that the circumstance, in which alone the instances differ, is the cause or a *part* of the cause of the investigated phenomenon. See Mill, bk. III, chap. VIII, §2.

[14] A detailed description is given in von Wright [11], pp. 116-19.

[15] See Mill, bk. III, chap. VIII, §4 and von Wright [11], pp. 97-102 and 119-26. Mill also describes two further methods, called by him those of Residues and of Concomitant Variations. These two methods, however, do not contribute anything new to the logical mechanism, as such, of eliminative induction. (Cf. Nicod, p. 220 fn. and von Wright [11], p. 160f.) Mill's description of his methods was anticipated in Herschel's *Discourse on the Study of Natural Philosophy* which appeared thirteen years before Mill's *Logic*. (See especially Herschel, p. 151ff.) As Herschel's greatest contribution to the theory of induction, however, must be regarded the emphasis which he laid on what he called 'residual phenomena', i.e. phenomena not accounted for by known laws of a given context. (Ibid., p. 156ff.) An important class of such residual phenomena are so-called 'counteracting causes'. The theory of residual phenomena is connected with conventionalism. (See above chap. III, §1 and §2.) For residual phenomena in inductive logic see also Jevons [2], p. 558ff.

[16] Cf. Broad [8], p. 311.

[17] It is noteworthy that in the search of necessary-and-sufficient conditions (use of the Joint Method) there is no corresponding restriction to the 'direction of elimination' among possible complex conditions. See von Wright [11], p. 123.

[18] See Kaila [6], p. 97ff.

[19] Mill, bk. III, chap. VIII, §2. Cf. above fn. 13.

[20] Lachelier (p. 31) speaks of 'l'induction scientifique' as opposed to 'l'induction vulgaire' intending, as far as we can judge, precisely the distinction between eliminative and enumerative induction.

[21] Cf. Mill, bk. III, chap. x, §2: 'A single instance eliminating some antecedent which existed in all the other cases, is of more value than the greatest multitude of instances which are reckoned by their number alone.'

[22] Cf. Keynes p. 217. Keynes makes the acute observation (p. 218f.) that Hume apparently on this point misrepresented the nature of inductive argument in his criticism of it. See also Stocks, p. 202.

§5. *Remarks about the comparative value of the methods of Agreement and Difference.*

[1] Mill, bk. III, chap. VIII, §2 and §3.

[2] See e.g. Fowler, p. 157f.

[3] Mill, bk. III, chap. VIII, §3: 'It thus appears to be by the Method of Difference alone that we can ever, in the way of direct experience, arrive with certainty at causes.'

[4] Ibid.

[5] See e.g. ibid., bk. III, chap. v, §3: 'The cause, then . . . is the sum total of the conditions . . . which being realized, the consequent invariably follows.' That is to say: *A* is the cause of *B*, if *A* precedes *B* in time and universally *implies* it.

[6] Ibid., bk. III, chap. VIII, §1 and §3.

[7] Ibid., bk. III, chap. VIII, §1.

[8] Ibid.

[9] This is the point were the fallacy enters. Italics mine.

[10] Ibid. The most natural interpretation of the term 'unconditional antecedent' would be 'necessary condition in time'. This interpretation, however, is excluded both by the use which Mill otherwise makes of the term 'cause' (cf. above fn. 5) and on the ground that the method of Agreement *can* prove an antecedent to be *unconditional* in the sense of being the (only possible) necessary condition. Thus under this 'natural' interpretation Mill's statement would be simply false. It must, therefore, be interpreted as is done by us in the text.

[11] Mill himself (ibid.), erroneously, connected the formulation mentioned with 'the impossibility of assuring ourselves' (in the given example) 'that A is the *only* immediate antecedent common to both the instances'.

[12] Mill, bk. III, chap. VIII, §3.

[13] Mill, bk. III, chap. VIII, §3 and Fowler, p. 158.

[14] Cf. above p. 65.

§6. *The general postulates of induction by elimination.*

[1] See Ellis [2], p. 84, where the postulate of Limited Variety is called 'The fundamental principle in virtue of which alone a method of exclusions can necessarily lead to a positive result.'

[2] Bacon [3], vol. III, p. 243 and Bacon [4], vol. III, p. 357. Cf. Ellis [1], p. 28: 'The view . . . that it is possible to reduce all the phenomena of the universe to combinations of a limited number of simple elements is the central point of Bacon's whole system.' It is, however, not quite clear what the principle of Limited Variety, as it occurs in Bacon's philosophy, really means and to what extent it is covered by the idea that the number of different properties of an individual is finite. For a lucid discussion of the Baconian idea of limited variety and different possible meanings of it see Broad [6], p. 35ff. Bacon's ideas on this point are probably related to another favourite idea of his, viz. that of a complete and definite collection of all human knowledge. (See Spedding, vol. I, p. 369ff.) All these ideas might be regarded as kindred to the *Mathesis-Universalis*-idea, characteristic of the systems of Descartes, Leibniz and other philosophers of the seventeenth century. The relatedness between the scheme of an inductive logic and the *Mathesis Universalis* becomes quite apparent in Robert Hooke's treatise *A General Scheme of the present state of Natural Philosophy*, which was published posthumously in 1705. Hooke tried to develop the general method of Bacon, without, however, contributing anything essentially new to it, into a 'Philosophical Algebra' (note the mathematical analogy!) which makes it 'very easy to proceed in any Natural Inquiry, regularly and certainly'. (See Hooke, p. 6f.)

[3] Keynes, p. 251. Keynes introduces his postulate for the purpose of securing a finite *a priori*-probability in favour of each one of all concurrent hypotheses as to a conditioned property of a given characteristic. See below chap. VI, §5.

[4] E.g. if the properties are sense-qualities. Cf. Kaila [6], p. 203.

[5] A similar objection to the use of the postulate of Limited Variety for the purpose of assigning finite *a priori*-probabilities to the concurrent hypotheses was made by Nicod against Keynes. See Nicod, p. 278 fn.

[6] Whereas Bacon resorts to the postulate of Limited Variety, the inductive logic of Mill may be said to be based on the postulate of Completely Known Instances, although

it is never explicitly stated by him. (Keynes, p. 270f.) From the tacit assumption of this postulate Mill's Method of Difference obtains part of its illusory strength. (Cf. Jevons [3], p. 295ff. and Kaila [6], p. 99.)

[7] See Kaila [6], p. 98ff.

[8] It is uncertain whether the term 'property' here is not used to denote a relation.

[9] For this idea see Maxwell, chap. I (the end), Schlick [2], p. 147f., Hempel [1], p. 31.

[10] I.e. it is conceivable that it were an analytical truth.

[11] Bacon [3], vol. III, p. 242.

[12] Mill, bk. III, chap. XIX. Mill also says (bk. III, chap. IX, §6) with reference to Whewell, that his inductive methods 'are methods of discovery'. But at the same time he adds that they are *also* methods of 'proof'. It remains, however, uncertain whether 'proof' here means a demonstration that a certain general proposition is true or a demonstration that a certain general proposition is, in a given context, the only possible generalization.

[13] Hibben, p. 166. The author states that we may by the inductive method detect that *A* has been the cause of *B* in given instances, but that from this it does not necessarily follow that we can generalize as to the constant conjunction of *A* and *B*. In this statement we find ourselves confronted with the source of much ambiguity in discussions about the aim and power of the inductive logic, viz. the failure to distinguish sharply between the following two meanings of the word 'cause': (1) A characteristic with the 'power of producing' another characteristic *universally*, i.e. *always* when certain conditions are fulfilled, and (2) A characteristic which is the only one that *in given cases* has been conjoined with another characteristic.

[14] Cf. the above quotation from Bacon (p. 63) and Mill, bk. III, chap. III, §3: 'That all swans are white, cannot have been a good induction, *since the conclusion has turned out erroneous.*'

[15] For this term see Nicod, p. 223. Nicod, however, states the principle only in the weaker form that every characteristic has a *sufficient* condition. In Keynes again there is (p. 226) a corresponding postulate of eliminative induction called the principle of Uniformity of Nature. (Cf. Broad [1], pt. II, p. 13ff.) Keynes has not observed that, since the method of elimination which he describes is roughly that of Agreement, his principle applies only to *necessary* conditions.

[16] For this formulation of the Deterministic Assumption and its implications see Broad [8], p. 307 and von Wright [11], pp. 72-7 and 131-5. If the law is of the form (2) of §3 there must be added some qualification as regards the correlating function *F*. Those possible qualifications will not be considered here.

[17] It must furthermore be assumed that Mill's Universal Law of Causation applies only to sufficient conditions, to judge from the definition of cause given by him. Cf. above chap. IV, §5, fn. 5.

[18] Mill, bk. III, chap. XXII, §4: 'To overlook this . . . was, as it seems to me, the capital error in Bacon's view of inductive philosophy.'

[19] The Baconian 'forms' and 'natures' have, however, also peculiarities other than those of being simultaneously existing necessary and sufficient conditions of each other. Bacon has rightly seen that other peculiarities must be demanded in order to distinguish the characteristics *as* forms and natures respectively. Thus he requires the 'form' to be something of the kind of a *differentia specifica* of a *genus proximum* (Cf. Bacon [2], lib. II, aph. IV and XV). This connects his doctrine of induction with the Aristotelian doctrine of definition (Kotarbinski, p. 111ff). Bacon also seems to have thought, at least at a certain stage in his development, that the 'form' ought to be an external physical property, the 'nature' again a phenomenological feature (Kotarbinski, p.

113ff). Therewith his doctrine of forms and natures becomes connected with the distinction between 'primary' and 'secondary' qualities (Ellis [1], p. 28ff).

[20] See Nicod, p. 229ff.

§7. *The justification of the postulates of eliminative induction.*

[1] Above chap. II, §7.

[2] Cf. above chap. II, §2, fns. 13 and 16.

[3] See chap. II, §2, fn. 16.

[4] These findings constitute the so-called 'paradoxes of strict (necessary) implication'. These and other principles of modal logic, mentioned here, are explained in Lewis-Langford, *Symbolic Logic* (New York, 1932) or in the Author's *An Essay in Modal Logic* (Amsterdam, 1951).

[5] No reason in support of this assumption will be given here. The idea appears highly plausible and it has, as far as we know, never been contested.

[6] For a similar argument see Broad [12], pt. I, p. 23f.

[7] See above chap. IV, §1.

[8] One might make the objection to the use of enumerative induction that it cannot even *confirm* the Deterministic Assumption, and correspondingly Mill's Universal Law of Causation. For the assertion that *A* has a sufficient or necessary condition is a universal proposition and hence *unverifiable* (since it would be circular to suppose it to have been verified through the eliminative process itself). This important point seems to have escaped Mill's notice. Mill constantly speaks about the confirmation of the Universal Law of Causation through enumerative induction, as if it were actually possible to verify single causal uniformities.

[9] Cf. above chap. IV, §1.

[10] The metalogical proof of this will be omitted here.

[11] From the above argument on p. 82.

[12] This qualification is necessary in order to avoid circularity.

§8. *The eliminative method and the justification of induction.*

[1] This task the Author has tried to accomplish in his book *A Treatise on Induction and Probability* (London, 1951), especially chaps. IV and V.

CHAPTER V. INDUCTION AND PROBABILITY

§1. *The hypothetical character of induction.*

[1] Newton [2], p. 31. See also the fourth of Newton's *Regulae Philosophandi* in Newton [1], lib. III. This must not be confused with Newton's own use of the term 'hypothesis'. Cf. Lalande, p. 123ff.

[2] Huyghens, vol. XIX, p. 454. See also Lalande, p. 146.

[3] If they were not hypotheses, incidentally, of what *use* and of what *interest* would it then be to draw the conclusions and make the tests?

[4] This is particularly true of Whewell. Cf. Stoll, p. 91

[5] See Jevons [2], p. 152 and p. 218ff. Jevons uses 'hypothetical' and hypothesis' in several senses which are not always clearly kept apart. (Cf. Johnson [1], vol. III, chap. II, §12.) By the 'hypothetical' character of induction we here mean simply the fact that ampliative induction does not yield demonstrative certainty. When Jevons, however, says ([2], p. 265ff.) that 'hypothesis' is the first stage in the inductive process of thought, the second and third stage being deduction and verification respectively, what he has in mind is 'hypothesis' in the sense of the so-called hypothetico-deductive method. See Note at the end of this section.

[6] For a 'typical' misunderstanding see Fowler's criticism of Jevons. (Fowler, p. 9ff.) Fowler is wholly unable to see the *significance* of the fact that inductive generalizations, used for predictions, are hypothetical. The following quotation (ibid., p. 10f.) is interesting as it illustrates the very difficulty on this point which Fowler entirely over-looks: 'I maintain as against Mr. Jevons that many of our inductive inferences have all the certainty of which human knowledge is capable. Is the law of gravitation one whit less certain than the conclusion of the 47th Proposition of the First Book of Euclid? Or is the proposition that animal and vegetable life cannot exist without moisture one whit less certain than the truths of the multiplication table? Both these physical generalizations are established by the Method of Difference, and as *actual* Laws of Nature' (cf. above chap. IV, §6, fn. 13), 'admit, I conceive, of no doubt. But it may be asked if they will always continue to be Laws of Nature? I reply that, unless the constitution of the Universe shall be changed to an extent which I cannot now even conceive, they will so continue, and that no reasonable man has any practical doubt as to their continuance.'

[7] *Note on the rôle of Induction and Hypothesis in Science.*
'Hypothesis' and 'hypothetical' are used in many different senses in connection with induction. The hypothetical character of induction as understood above and as emphasized by Jevons ought not to be confused with the use of the hypothetical or hypothetico-deductive method in the inductive sciences.

Some modern authors (K. Popper, J. O. Wisdom) emphasize, *against* induction, the role of hypothesis and the hypothetico-deductive method. Wisdom ([2], p. 7) goes as far as saying 'that induction plays no part whatever in science — that there is no inductive method and that nothing approximating to inductive inference is used'. Novelty is claimed (Wisdom [2], p. 49) for the approach of Popper's, who is said to be the first to give to the hypothetico-deductive method 'serious attention in metascience'.

In face of these modern exaggerations it may be useful to remember:

(i) That the 'metascientific' appreciation of the hypothetico-deductive method is clearly manifested in the remarks on scientific method which we find in the works of many of the champions of modern natural science such as Galileo, Pascal, Huyghens, or Leibniz (see above chap. IV, §2 and Lalande, pp. 83-109 and 146-71), and

(ii) That the role of hypothesis in science and the relation between induction and the hypothetico-deductive method has been ably and extensively studied by authors on scientific method in the 19th century, foremostly by William Whewell and E. F. Apelt. (See Whewell [3], bk. II, chap. IV-VII and bk. III, chap. V, VI and IX; Whewell [4]; Apelt [1], especially pp. 56-64.) It is particularly regrettable that the writings of Whewell on the philosophy of science have largely fallen into oblivion among modern authors, probably under the influence of a text-book tradition in inductive logic which has been nourished mainly by Mill's theory of the canons of elimination.

Anyone who maintains that 'induction plays no part whatever in science' is advised to study the examples of the use of inductive methods given in Mill's *Logic* (particularly in bk. III, chap. IX). Against these examples it could conceivably be objected that they are all of a rather 'primitive' kind. This rejoinder should *not* be met by assuming offhand that examples of a more 'advanced' nature could be added. It is worth considering, whether the nature of the examples, which are found in Mill's or Bain's works on inductive logic and similar books, do not indicate some essential *limitation* to the use of induction as a 'method' in scientific investigation. To this extent the 'critics of induction' may be right. Here the following observation suggests itself (see also above chap. IV, §2):

Situations, in which generalizations are framed, may be divided into two types. In situations of the first type it is clear or 'practically clear' which are the features (characteristics, properties) that possibly lend themselves to generalization. Of this type are many, or perhaps most, of the situations in which we look for the cause (condition) of a phenomenon among a number of antecedent or coexistent phenomena. (Perhaps the best examples are found in the branch of medicine called aitiology.) In such cases the generalization emerges from the observation of a regular concomitance in the occurrence (or variations) of two features (or groups of features). The 'method' of generalization is either induction by simple enumeration or induction after the elimination of concurrent possibilities.

That such situations are frequent in every-day thinking is obvious and that they occur in 'science' too can hardly be disputed. But it would be a mistake to believe that *all* cases of generalization are of this type. We know of no author on inductive logic, who had held this belief; but some authors may be said to have overemphasized the importance of this kind of situation in science. (Cf. Mill, bk. III, chap. XIV, §§4-7 on the hypothetical method.)

In situations of the second type, the possible generalizations cannot be 'read off' by merely inspecting some experiential data. The *introduction of a new concept* is required which, as Whewell says, 'colligates' the facts, gives them a uniting feature. As a prototype of such cases, may be regarded the tracing of a curve through a number of points, the coordinates of which (in a diagram) are given by observation. Here the colligating concept is the curve (or rather, its mathematical 'law'). It is, usually, first introduced as a 'hypothesis' which the points are subsequently shown to fit. In this 'verification'-procedure deduction plays an essential part. The 'method' involved is thus: hypothesis plus deduction.

What has been said so far, however, is only *one* aspect of the 'hypothetical method'. Another aspect consists in the *conjectural* character of the hypotheses. They ought to make *prediction* possible. On this the classics of scientific method agree (with the reservation, however, that the idea of conjectural hypotheses seems to be peculiar to Western science; it is not prominent in Ancient science). Whewell says ([3], p. 85f.): 'Thus the hypotheses which we accept ought to explain phenomena which we have observed. But they ought to do more than this: our hypotheses ought to *foretell* phenomena which have not yet been observed.' And the fact that a hypothesis has been verified to fit given data is no guarantee that it will not be falsified when predictions from it are confronted with future observations. (Cf. chap. IV, §2, fn. 26.)

Thus hypothesis in science frequently has a double function. It introduces a new concept or idea to *account* for observed data. And it makes *conjectures* about the unobserved. The first function presupposes that an invention or discovery has been made. And inventions are, as Whewell said, 'happy guesses' or 'leaps which are out of the reach of method'. (See above chap. IV, §2.) To fulfil the second function, is to reason inductively. Thus induction enters as an ingredient in the hypothetico-deductive method itself. Whether, in this connection, induction should be called a 'method' or not, is a matter of nomenclature.

The difference between the two types of situation, just described, may be used for distinguishing between 'primitive' and 'advanced' generalizations in science. And one may suggest that use of induction as a 'method' is confined to the 'primitive' cases. But even if it were true, which it is *not*, that the hypothetico-deductive procedure were 'the actual method of science' (Wisdom [2], p. 46), this would not, in view of the *conjectural* character of scientific hypotheses, minimize the seriousness of the problem of the justification of induction in science.

These remarks do not claim to give an exhaustive account of the typical uses of hypothesis in science. Not all 'advanced' generalizations can appropriately be put under the title of 'colligation of facts', and not every use of hypothesis for conjecturing is 'anticipation from experience' or can appropriately be termed 'inductive'. We have only against certain 'anti-inductivist' claims, wanted to show that theory of induction cannot, in the name of the hypothetico-deductive method, be banished from holding a prominent place within the methodology of science.

On the notion of hypothesis and the hypothetical method the following works, now largely forgotten, may also be profitably consulted: Biedermann, *Die Bedeutung der Hypothese* (Dresden, 1894); Hillebrand, *Zur Lehre von der Hypothesenbildung* (Sitzungsberichte der Wiener Akademie, Philosophisch-historische Classe, Bd. 134, Wien 1896); Mach, *Erkenntnis und Irrtum* (Leipzig, 1905); Naville, *La Logique de l'Hypothèse* (Paris, 1880). The best historical survey, known to the Author, of the problems discussed in this note is Lalande, *Les Théories de l'Induction et de l'Expérimentation* (Paris, 1929).

§2. *Hypothetical induction and probable knowledge.*

1 Cf. Reichenbach [13], p. 98: 'Belief can be the *motive* of action, but belief as such can never *justify* an action; only a *justified* belief can do that.'

CHAPTER VI. FORMAL ANALYSIS OF INDUCTIVE PROBABILITY

§1. *The Abstract Calculus of Probability.*

1 As a definition of probability, the so-called frequency-view would seem to be the most ancient. It goes back to Aristotle. But the frequency-definition was not used as a basis for the mathematical study of probability until the nineteenth century. See chap. VI, §2, fn. 11.

2 Braithwaite [6], p. 118.

3 This, roughly, is the view taken by Carnap, who distinguishes *two* (main) concepts of probability, viz. probability$_1$ which is a ratio of possibilities (strictly speaking, of *measures of ranges*, see below p. 101) and probability$_2$ which is a frequency. This distinction does not seem to us a very happy one. The fact, on which it is based, is the existence of two *models* of mathematical probability of an identical or at least closely similar structure. The question, how these models are related to the actual *use* (applications) of mathematical probability, is very complicated. It would be an oversimplification to think that probability is sometimes used to 'mean' a ratio of range-measures and sometimes to 'mean' a relative frequency, and that these two usages of it can be sharply separated.

4 As first attempts in this direction may be regarded Bohlmann (1901) and Bernstein (1917). Neither paper is mentioned in Keynes's Bibliography.

5 Similar systems for probability have been proposed by Tornier (in [1] and [2]) and by Cramér (in [1]).

6 Cf. Cramér [2], p. 151.

7 The classification of Keynes's system along with more recent abstract theories of probability presents some difficulties. It dates from a period when the general ideas of axiomatic and formalized systems were much less developed than nowadays. Keynes presents it, not as an abstract calculus, which is supposed to be 'neutral' with regard to various interpretations of it, but as a form of what is sometimes also called a belief-theory of probability. (See chap. VII, §1.)

8 And further developed in Reichenbach [4].

9 This does not exhaust the list. An abstract calculus, in line with Reichenbach's

rather than Keynes's, was proposed by the Author in [7] and further developed in [11]. See also below fn. 16.

10 In Reichenbach [4] the probability-relation is first (p. 56) said to be between *events* (*Ereignisse*) and then (p. 57) said to be between *propositions* (*Sätze*). In the further development of the system the relation is actually one between propositional-functions. On the question whether probability is appropriately attributed to events or to propositions see Ancillon, p. 4, Boole [1], p. 247f., and Reichenbach [11], p. 57ff. See also below fn. 11.

11 To these entities may also be counted their 'linguistic counterparts', i.e. sentences as counterparts of propositions and (some kind of) names as counterparts of attributes. The interpretation of probability in terms of such 'linguistic counterparts' will not be considered here.

12 That probability is always *relative* to some evidence has been energetically stressed by Keynes, who rightly says (p. 6) that 'a great deal of confusion and error has arisen out of a failure to take due account of this *relational* aspect of probability'.

13 See above chap. v, §3.

14 In Keynes's system it is explicitly assumed that the datum-proposition h must not be self-contradictory. (See Keynes, p. 116ff.)

15 This was maintained, e.g., by adherents in the nineteenth century of the dichotomy between *mathematical* and *philosophical* probability. See later, this chapter §8, fn. 9.

16 Keynes (chap. III) took the view that there are non-numerical (as distinct from *unknown* numerical) probabilities and that not all non-numerical probabilities are comparable. An axiom system of comparative probability has been given by Koopman (in [1]). See also Carnap [11], §§79-85 and [15] and Shimony.

17 See above p. 16.

18 For a fuller treatment of the notion of independence the reader is referred to von Wright [11], p. 193f.

19 For a fuller treatment see von Wright [11], p. 199ff.

20 ${}^{n}C_{m}$ is the so-called binomial coefficient. Another symbol for it is $\binom{n}{m}$. It stands for the value $\frac{n!}{m!\,(n-m)!}$. $n!$ again means the product $1 \times 2 \times 3 \times \ldots \times n$.

21 The proof is not given here as it is of no relevance to the epistemological problems under discussion. The proof makes no further use of principles of probability, but relies on considerations of a purely arithmetical nature. It may be found in any text-book on probability-mathematics.

22 See fn. 21. It was this second part of the theorem which, save for a minor difference, was proved by James Bernoulli ([1], pp. 236-8). An elegant proof both of the maximum principle and of the limit theorem, which uses only elementary means and closely follows Bernoulli's own deductions, is found in Kneale [1], §28 and §29.

§2. *The interpretation of formal probability.*

1 See von Wright [1], p. 6f.

2 See Reichenbach [3], p. 404.

3 As an example of a model which falls under neither category may be mentioned the geometrical model given in Reichenbach [2], p. 588ff.

4 The term *truth-frequency* appears originally to have been suggested by Whitehead (see Keynes, p. 101). It is used also by Carnap, Reichenbach, and others.

[5] See Reichenbach [4], §18. For a thorough presentation of the Finite Frequency-Model see also Russell [7].

[6] On the notion of a limiting frequency or proportion see above chap. I, §2. Also later chap. VIII, §1.

[7] This important point, it seems to us, has not received sufficient attention either from von Mises or from Reichenbach, not to speak of earlier proponents of the frequency-theory of probability. It is emphasized by Braithwaite ([6], p. 125) and put forward by him as an objection against the Frequency-Limit-Model as a proposed analysis of the meaning of probability.

[8] See von Wright [11], p. 80f.

[9] See Reichenbach [4], §18.

[10] Important mathematical contributions to the problem of random distribution are to be found in the papers of Church, Copeland [2], Feller, and Wald. For a discussion of the epistemological aspects of the notion of randomness the reader is referred to von Wright [2], [7] and [11].

[11] As mentioned above (chap. VI, §1, fn. 1) the frequency-view of probability goes back to Aristotle. A probability, says Aristotle in [2], 70ᵃ4, is 'what men know to happen or not to happen, to be or not to be, for the most part thus and thus'. (See also Aristotle [4], 1357ᵃ.) A similar view of probability was taken by writers of the seventeenth and eighteenth centuries, other than those who studied probability in connection with games of chance. Thus Locke (bk. IV, chap. xv, §1) says that 'probability is nothing but the appearance of such an agreement — whose connection is not constant — but is, or appears, for the most part to be so'. (See also below chap. VII, §2, fn. 5.) In connection with a mathematical theory of probability the frequency-view made its first appearance in three publications from the same year 1843, viz. Cournot [1], Ellis [3], and Mill. (See von Wright [6].) Mill, however, in later editions of his *Logic* (see bk. III, chap. XVIII, §1) 'recanted' his earlier criticism of the view of probability taken 'by Laplace and by mathematicians generally' and withdrew from the frequency position. None of the three authors mentioned attempted a rigorous construction of the mathematical calculus on the basis of a frequency-model. The first to do this was Venn in 1866. Venn also was the first to make use of the notion of a limiting frequency. An improved version of the frequency-theory was presented by von Mises in 1919. The modern form of the theory is best studied in von Mises [2] and in Reichenbach [4]. A very good semi-popular account is found in Nagel [5].

[12] The English word 'range' may be regarded as a translation of the German *Spielraum* which was introduced in probability theory by J. von Kreis (1886).

[13] It is usually not clear which alternative is intended by those authors (particularly of an earlier epoch), who speak of probability as an attribute of *events*.

[14] A range model, in which the terms are attributes, is given in Kneale [1]. In the most elaborate account of the range-theory which exists, viz. Carnap [11], the terms (arguments of Carnap's probability-function) are *sentences*. The choice of sentences rather than propositions as terms has certain technical advantages, but fundamentally there is no great difference between the two possibilities. See Carnap [11], §10 and §52.

[15] I.e. we substitute for h a logically identical proposition h' which overtly has the form of a disjunction of n propositions of the required nature. That h' *overtly* has this form should mean that the *sentence* 'h'', expressing the proposition h', is a disjunction-sentence of n atomic sentences, each of which expresses a proposition which entails either a or $\sim a$ and no two of which express compatible propositions. If h itself overtly has the required disjunctive form no analysis is needed.

[16] Laplace's view of the philosophical nature of probability is best studied in [6].

[17] For the notion of a truth-function and of a normal form see chap. I, §2, fn. 1 or consult any modern text-book on logic.

[18] In the limiting case, the set σ may consist of the propositions a and h themselves and no other propositions.

[19] This essentially answers to the definition of probability within a theory of truth-functions proposed by Wittgenstein (5.15ff.). It is also substantially the same as Bolzano's (§§147, 161 and 167) definition of probability as 'relative validity' (*relative Gültigkeit*). Our definition might therefore be called the Bolzano-Wittgenstein definition.

In the theories of Bolzano and Wittgenstein the set σ is identified with the set of atomic propositions, of which a and h are *overtly* truth-functions. This is the set of propositions expressed by the atomic sentences of which the *sentences* 'a' and 'h' are molecular complexes. (See above fn. 15.) Our definition is thus somewhat more general than the definitions actually proposed by Bolzano and by Wittgenstein.

As specialized forms of the Bolzano-Wittgenstein definition may be regarded the definition of probability on the basis of hypothetico-disjunctive judgments, given by Sigwart (vol. II, p. 314) and Czuber ([3], vol. I p. 5); the definition on the basis of disjunctive judgments, given by Lange (p. 99ff.) and Stumpf; the definition on the basis of hypothetical judgments, given by Fick (p. 12ff.); and finally also the definition given by Mendelssohn (vol. II, p. 248) and by Hailperin.

[20] This essentially answers to Carnap's concept of range in [11], §18 D. Our set σ plays a role corresponding to Carnap's choice of a 'language'. For a brief account of the essentials of Carnap's theory see von Wright [10].

[21] These units essentially answer to Carnap's *state-descriptions.* ([11], §18A.)

[22] This definition was, neglecting notational differences, first proposed by Waismann and essentially answers to Carnap's definition in [11], §55A of a *regular confirmation function.* It may therefore be called the Waismann-Carnap definition of probability.

[23] See Waismann, p. 236.

[24] In Carnap's terminology: on the choice of a *language* and a *regular measure function.*

[25] The best discussion of the principle is probably the one found in Keynes, chap. IV. See also Kneale [1], §31, §34 and §35.

[26] J. Bernoulli [1], p. 224: 'nulla perspicitur ratio cur haec vel illa potius exire debeat quam quaelibet alia'.

[27] See Waismann, p. 242.

[28] The re-birth of the frequency-theory in the nineteenth century (see fn. 11 above) was intimately connected with a criticism of the idea of *equipossibility* in the theory of Laplace. It was alleged that the *equality* of alternative possibilities must ultimately consist in the equal frequencies of their realization 'in the long run'. See Ellis [3] and Mill, bk. III, chap. XVIII, §1.

§3. *The doctrine of Inverse Probability.*

[1] See Bernoulli [1], p. 224ff., where the author is discussing the determination of probabilities, as he puts it, '*a posteriori*', i.e. the nature and use of the *inverse* Law of Great Numbers. Of this inverted form of the theorem he says, not only that it is provable (ibid., p. 226 and Bernoulli [2], p. 2f.), but also that he himself had proved it after twenty years of effort and was going to give the proof in the *Ars Conjectandi* (Bernoulli [1], p. 227). Thereupon he gives the proof of the *direct* theorem (ibid., p. 236ff.), and here the book suddenly ends. It remains uncertain, whether Bernoulli

regarded the proof given as a proof of the inverted form of the theorem which he had mentioned before, or whether he intended the real proof to follow in a later chapter. It appears to us (cf. ibid., p. 239) that the former alternative is the true interpretation of Bernoulli's own opinion. See also Todhunter, p. 73 and M. Cantor [2], vol. III, p. 334f. Curiously enough Cantor seems to believe that Bernoulli really proved the inverted form of his theorem.

[2] See e.g. De Moivre, p. 251: 'As, upon the Supposition of a certain determinate Law according to which any Event is to happen, we demonstrate that the Ratio of Happenings will continually approach to that Law, as the Experiments or Observations are multiplied: so, *conversely*, if from numberless Observations we find the Ratio of the Events to converge to a determinate quantity, as to the Ratio of $P : Q$; then we conclude that this Ratio expresses the determinate Law according to which the Event is to happen. For let that Law be expressed not by the Ratio $P : Q$, but by some other, as $R:S$; then would the Ratio of the Events converge to this last, not to the former: which contradicts our *Hypothesis*.' From this interesting quotation is clearly seen that it is the confusion of maximum probability with certainty that makes the inversion of Bernoulli's theorem appear self-evident. The conversion would be self-evident if we had proved by Bernoulli that, supposing an event's probability to be p, the proportion of that event's happening on all occasions is *certainly* p. But what we really have proved is that the probability that the proportion is p on some n occasions, approaches 1 as n approaches ∞. For a similar confusion see G. Cantor, p. 362.

[3] Keynes, p. 148f. Keynes's Inverse Principle is of a somewhat more general content than our *T10*.

[4] It should be noted that the binomial coefficient ${}^{n}C_{m}$ cancels out.

[5] By an adaptation of the arithmetical considerations underlying the Direct Law of Maximum Probability. Cf. chap. VI, §1, fn. 21.

[6] By an adaptation of the arithmetical considerations underlying the Direct Law of Great Numbers.

[7] The name 'Bayes's Theorem' is used very ambiguously in literature. Sometimes it is used for what we have called the Inverse Principle, i.e. for *T10* or some of its generalizations. (See Kolmogorov, chap. I, §4. and Nagel [5], p. 29.) Sometimes it is used to denote the Inverse Law of Great Numbers. (See von Mises [2], p. 147f.) Actually, Bayes's chief achievement consists in having proved the theorem which we here call the Inverse Law of Maximum Probability. To this end he had, of course, first to prove a form of the Inverse Principle. Bayes's considerations apply to 'continuous probabilities'. i.e. they assume that the Bernoullian probabilities of the problem cover the whole range from 0 to 1 inclusive. Bayes, however, did not use integration but relied on 'geometrical' considerations. (See Bayes [1], p. 388ff.) The first to prove the formulae involved in the problem by the use of integration was Laplace in a Mémoire of the year 1774. It appears that, in 1774, Laplace was not acquainted with the essay of Bayes. Concerning the history of Bayes's Theorem see the next fn. and fn. 3 of the next section.

[8] The asymptotic property of the expressions (D) and (D'), i.e. the Inverse Law of Great Numbers, was not known to Bayes. It was noted by Price, who communicated Bayes's paper for publication. (See Bayes [1], p. 418 fn.) It was proved for (D'), i.e. on the assumption of equal initial probabilities, by Laplace in the Mémoire of 1774. The corresponding proof for (D), which is 'practically independent' of initial probabilities, is of later date. (See Bachelier [1], vol. I, p. 472f. and p. 488; Edgeworth, p. 228; von Mises [2], p. 148f.) We have not been able to discover, who was the first to produce it and thus to complete the proof of that part of Bayes's Theorem or the inversion of Bernoulli's Theorem which we here call the Inverse Law of Great Numbers.

[9] The name 'Rule of Succession' appears originally to have been suggested by Venn. (See Venn [1], p. 190) The use of the name in literature is ambiguous. Often it refers only to the formula $\frac{n+1}{n+2}$. It seems to us useful to distinguish between a *numerical* and a *non-numerical* form of the law.

[10] See Laplace [1], vol. VIII, p. 30f., [3], vol. X, p. 325ff., [6], vol. VII, p. 16ff. Prior to Laplace, Price (see Bayes [1], p. 405ff.) had used a somewhat different formula for the determination of 'the probability of future events'. See also Mendelssohn, vol. II, p. 264ff., where is given the formula $\frac{n}{n+1}$ for the probability that if two events have been observed n times in conjunction, they will be constantly conjoined.

The problem behind the Law of Succession can be put quite generally as follows: If an event has occurred m times on n occasions, what is the probability that the event will occur i times on the k next occasions? On the same assumptions as those made in proving Bayes's Theorem, we get the value:

$$^{k}C_{i} \times \frac{\int_{0}^{1} p^{m+i} \times (1-p)^{n+k-m-i} \times q_p \, dp}{\int_{0}^{1} p^{m} \times (1-p)^{n-m} \times q_p \, dp}$$

On the additional assumption that all the initial probabilities q_p are equal we get, after integration, the expression:

$$\frac{k! \times (m+i)! \times (n+k-m-i)! \times (n+1)!}{i! \times (k-i)! \times (n+k+1)! \times m! \times (n-m)!}$$

If $i = k = 1$, we obtain from this the value $\frac{m+1}{n+2}$ for the probability that the event will occur on the next occasion, if it has occurred m times in n occasions. If, further $m = n$, we obtain the value $\frac{n+1}{n+2}$ for the probability that the event will occur on the next occasion, if it has occurred on every one of some n occasions.

[11] This is but a consequence of the following arithmetical truth: If $m_1 < m_2 < \ldots < m_s$ and $M_1 + M_2 + \ldots + M_s = M'_1 + M'_2 + \ldots M'_s$ and $M_s < M'_s$, then $m_1 \times M_1 + m_2 \times M_2 + \ldots + m_s \times M_s < m_1 \times M'_1 + m_2 \times M_2 + \ldots + m_s \times M'_s$. The m- and M- and M'-values are assumed to be in the interval from 0 to 1 inclusive.

[12] Cf. von Wright [11], p. 213f. for a fuller treatment of the problem. The Non-Numerical Law of Succession, naturally, can be proved in a more general form which deals, not with the question of the probability of the event's occurrence on the next occasion, but with the question of the probability of its occurrence on the k next occasions. It is a mathematical fact, which can be deduced from either the non-numerical or the numerical version of the law and which has much impressed the philosophical imagination, that the greater this number k is, the smaller is the calculated probability. See Quetelet [2], p. 21 and de Morgan [2], p. 213f. It was on similar considerations about diminishing probabilities that Craig based his notorious calculations concerning the dying-out of belief in Christianity, on the supposition that this belief was based solely on oral or written tradition. See Craig, pp. 21 and 24.

[18] This, as far as we know, has first been shown by Broad. See Broad [7], pp. 4-9 and [12], p. 196f., and von Wright [11], p. 215.

§4. *Criticism of Inverse Probability.*

[1] For good illustrations of this idea see Kneale [1], §27 and Nagel [5], p. 30f.

[2] A characteristic use of the Inverse Principle (Formula), was for determining, with probability, whether a phenomenon was due to a cause *or* to chance. This meant a probability-judgment on the respective alternatives that the probability of the phenomenon, on a given condition, was 1 and that it was, on the same condition, $\frac{1}{2}$. Kirchhoff determined the probability that the occurence of the 60 dark rays, observed by him at characteristic places in the spectrum of the sun, were by chance coinciding with the rays in the spectrum of iron by this use of inverse probability. (See Kirchhoff, p. 79.) Hartmann (p. 24-35) has offered abhorrent instances of the uncritical use of the formulae for determining the probability of causes, calculating *inter alia* the probability for a non-material cause operating in a given case to 0.9999985! For formal treatment of the formulae for estimating the likelihood of causes and critical remarks about their applicability see Bertrand, p. 142ff.

[3] Bayes himself never spoke of his theorem as one of the *probability of causes*. His own treatment of the problem is purely mathematical and free from philosophical aspirations. Price, however, who communicated Bayes's paper to the Royal Society, regarded the formula as being relevant to the estimation of probable causes and to inductive conclusions in general. (See Bayes [1], p. 402ff.) With Laplace's above-mentioned Mémoire (see especially vol. VIII, p. 29ff.) the use of the formula being an instrument for determining the probability of causes was ultimately established. The literature, predominantly from the late eighteenth century, illustrating this use of it is very extensive. As examples, apart from the writings of Laplace, may be mentioned, Quetelet [1], p. 123ff. and [2], p. 24ff., the paper of Lhuilier and Prevost called 'Sur l'art d'estimer la probabilité des causes par les effets' (1799), and Trembley's paper 'De probabilitate causarum ab effectibus oriunda' (1795).

[4] Keynes, p. 82.

[5] For various usages and acceptances of the formula see, e.g. Quetelet [1], p. 128 and [2], p. 18; de Morgan [2], p. 213ff.; Jevons [2], p. 257ff.; Lotze [1], p. 425; Bobek, p. 207f.; Whittaker, p. 163ff.; Edgeworth, p. 234.

[6] See Keynes, p. 383 and Bobek, p. 207f.

[7] Among such authors Jeffreys and Carnap are most prominent. Carnap's Quantitative System of Inductive Logic ([11], §110) may be regarded as a revival, with modifications and further developments, of essential aspects of the classical doctrine of inverse probability.

[8] See Boole [1], p. 368ff. for very acute criticisms. See also Bryant and Broad [1], pt. I, pp. 393-400 and [7], p. 19-23.

[9] Keynes, p. 377f. The alleged inconsistency which Keynes sees in the formula is removed if due consideration is paid to the data relative to which the various probabilities of the problem exist.

[10] See especially Fisher [2], p. 10 and [7], p. 6f. and the minor publications listed in the Bibliography.

[11] For an attempt at a logical and epistemological clarification of the problems of inverse probability see von Wright [7], pp. 60-6 and [11], chap. x. This attempt, we feel, still stands in need of improvement.

[12] Laplace [2], p. 419: 'Lorsqu'on n'a aucune donnée *a priori* sur la possibilité d'un

événement, il faut supposer toutes les possibilités, depuis zéro jusqu'à l'unité, également probables.'

[13] For the idea of 'equal distribution of ignorance' see Boole [1], p.370 and Donkin, p. 354. In the first edition of the present work (p. 121) an attempt was made to account for the equality of *a priori* probabilities as a consequence of what we *mean* by 'equal ignorance'. For an appraisal of this attempt see Broad [12], p. 119. Edgeworth and Pearson ([1], p. 365ff. and [2], p. 143ff.) tried to justify the assumption of equality on an empirical basis. All the possible hypotheses about the value of a proportion (probability) are initially equally probable, because experience is alleged to show that all proportions (probability-values) as a matter of fact occur equally often in nature. As Edgeworth (p. 230) put it: 'The assumption that any probability-constant about which we know nothing in particular is as likely to have one value as another is grounded upon the rough but solid experience that such constants do, as a matter of fact, as often have one value as another'. For a criticism of this idea see Keynes, p. 381ff. and von Wright [11], p. 283.

§5. *Confirmation and probability.*

[1] See above chap. v, §1, especially fn. 7.

[2] For example the theory developed by Rudolf Carnap. A degree of confirmation means with Carnap the same as a degree of probability, defined as a relative measure of ranges. (See above §2 of the present chapter.) The proposition conferring a degree of probability or confirmation upon another proposition need not be entailed by the latter, nor need it be, in any obvious sense of the word, an 'instance' of it. The probabilified (confirmed) proposition again need by no means be a generalization. It is, on the contrary, characteristic of Carnap's theory that any (numerically unrestricted) Universal Generalization possesses a *zero*-probability, relative to any (finite) number of confirming instances of it. (See Carnap [11], §110f.) In an important sense this theory is *incapable* of evaluating the bearing of individual instances upon general conclusions and is, already for this reason, no Confirmation-Theory at all in our sense of the word. This incapacity, in our opinion, must be considered a serious defect of Carnap's treatment of induction, irrespective of whether one wishes to restrict the use of the *term* 'Confirmation-Theory' to the treatment of *converse entailment-relations* (as is done by us), or use it in some wider sense (as is done by Carnap). See also above chap. I, §3.

[3] For the idea that induction by simple enumeration, although unable to reach certainty, yet contributes to an increase of probability in a generalization see Huyghens, vol. XIX, p. 454, Bayes [1], p. 406, Mendelssohn, vol. ii, p. 267f., Poisson, p. 161f., Lotze [2], p. 70. For the idea that the strength of support which a confirmation affords to a law is inversely proportionate to the confirmation's initial probability, see Herschel, p. 170f., Broad [1], pt. I, p. 402, Russell [4], p. 194f., and Kaila [6], p. 105f.

[4] Cf. Keynes, p. 237ff.

[5] Cf. Nicod, p. 248 and 252-4.

[6] This is the form in which the condition of convergence towards maximum probability was stated by Keynes (p. 236f.) and Nicod (p. 276). Keynes, moreover, substitutes for it a somewhat stronger condition (op. cit., p. 238). The reason, why the authors mentioned do not discuss the condition in the simple form, first introduced in the text, is probably that their proof of the Principal Theorem is more complicated than the simple proof given here.

[7] For a precise definition of this type of the notion of a limit see von Wright [11], p. 54f.

[8] For a statement of this axiom in a more developed symbolism see von Wright [11], p. 176f.

[9] Cf. von Wright [11], p. 248.

§6. *The Paradoxes of Confirmation.*

[1] For discussions of the notion of a confirming instance the reader is referred to Hempel [6] and Carnap [11], §§86-8.

[2] Hempel [6], p. 9ff. and Carnap [11], §87. See also Nicod, p. 219.

[3] Hempel [6], p. 124 and Carnap [11], §87.

[4] The Paradoxes of Confirmation were first hinted at by Hempel in [3] and discussed by Hosiasson-Lindenbaum in [3] and by Hempel in [6]. Hempel, however, does not make a clear distinction between the paradox which results from the clash between the Nicod- and Equivalence-Criteria, and the paradoxes which are special cases of the familiar Paradoxes of Implication. The discussion with Hempel and Carnap mainly centres round the first type of paradox. In von Wright [11], pp. 254-6 the second type of paradoxes is discussed.

§7. *Confirmation and elimination.*

[1] Cf. Keynes, p. 226, 234 and 236.

[2] Cf. Nicod, pp. 249-65 and pp. 269-73.

[3] In the first edition of the present work the Author sided with Nicod against Keynes both on the question, whether the increasing probability of a generalization approaches 1 as a limit, and on the question, whether confirmation contributes to probability independently of elimination. That this was a mistake was shown in von Wright [7], chap. III, §§3 and 4 and von Wright [11], chap. IX, §§3 and 4 and chap. X, §5.

[4] For a more detailed development of this idea see von Wright [11], chap. IX, §4.

[5] 'Co-presence with A' thus means 'not-absence in the presence of A'. B is co-present with A in x, if either A and B are both present in x, or A is absent and B present, or A and B are both absent. This definition of co-presence is made necessary by the fact that we regard anything which satisfies the propositional function $Ax \rightarrow Bx$ as affording a confirming instance (either 'genuine' or 'paradoxical') of the law $(x)\ (Ax \rightarrow Bx)$.

[6] 'Co-absence with B' thus means 'not-presence in the absence of B'. A is co-absent with B in x, if either A and B are both present in x, or A is absent and B present, or both A and B are absent. We notice that the two phrases 'B is co-present with A' and 'A is co-absent with B' mean exactly the same. Cf. fn. 5 above.

[7] The Author has also worked out a Range-Model of the Principal Theorem, using the general theory of measuring ranges which is developed in Carnap [11]. It turns out that in this model the condition $p_{n+1} < 1$, which in the Frequency-Model means elimination, is tantamount to certain measures of ranges becoming zero. No conditions for the existence of such zero-measures can be deduced within the model. But it becomes reasonable to assume that the ranges in question acquire a zero-measure ('become extinct') because of the incompatibility of a new confirming instance i_{m+1} with some or other of a number of *alternatives*, covered by the evidence e, to the generalization g. And this supports the idea that increase in probability is, in the case of the Range-Model too, effected by elimination.

§8. *Probability, scope, and simplicity. Reasoning from analogy. Mathematical and philosophical probability.*

[1] Cf. Keynes, p. 224ff. Keynes's reasoning in this place is neither very clear nor in every detail correct.

[2] This must not be confused with Keynes's use of the term 'analogy' or with our use of it above in chap. IV, §4.

[3] For a fuller examination of the argument from analogy within a theory of scopes see von Wright [11], chap. IX, §7. In Carnap [11], §110 D there is an outline of an analysis of reasoning from analogy within a Range-Model of probability. Carnap's analysis resembles ours in that it relates analogy to scope of propositions (in Carnap's terminology 'width' of properties). The main difference seems to lie in the fact that Carnap does *not* relate reasoning from analogy to considerations about nomic connections between characteristics. (Cf. our idea that the analogy contains some factor or conjunction of factors which is 'causally responsible' for the occurrence of the property, whose presence we know in one thing and conjecture in another thing.) The outline given in Carnap [11] is too sketchy to make possible a more detailed comparison of the two attempts to clarify the logical nature of reasoning from analogy. Although analogy may be said to belong to the traditional topics of inductive theory and the methodology of science, there are evidently very few attempts at a formal treatment of the subject. The only formal examination, beside Carnap's and ours, known to us, is in Hosiasson-Lindenbaum [4] and J. R. Weinberg [2].

[4] A forceful early expression of this idea is Leibniz's comparison between discovering laws of nature and finding the key to a cryptogram. See Couturat [1], p. 254f. and [2], p. 175 and p. 232. See also Kaila [6], p. 103ff., where an interesting suggestion is made, relating the idea of the cryptogram and of simplicity of curves to ideas of inverse probability. Leibniz's thought on the topic calls for a more thorough examination than has been given to them in literature.

[5] Cournot ([2], vol. I, p. 82) saw in the comparison of curves with regard to their simplicity the basis of all probability of inductions, and in curve-fitting the basic type of all making of theories and hypotheses in science. He says (ibid.): 'En général, une théorie scientifique quelconque . . . peut être assimilée à la courbe que l'on trace d'après une définition mathématique, en s'imposant la condition de la faire passer par un certain nombre de points donnés d'avance.'

[6] In the first edition of the present work and in von Wright [11] the theory of scope and probability was regarded as a special case of a general theory of simplicity and probability. For similar ideas see Bolzano, vol. II, §151, Broad [1], pt. I, p. 402, and Kaila [2], p. 139f.

[7] For contributions to a clarification of the notion of simplicity see Goodman [3] and [4], Kemeny [4], and Lindsay. For discussions of probability in relation to simplicity of curves see Cournot [2], vol. I, p. 82, Weyl, p. 155f., Jeffreys [1], p. 43ff. and [2], Braithwaite [3], Popper [2], p. 87ff., and Kaila [6], p. 103ff.

[8] In the first edition of the present work a treatment of probability and simplicity of curves was attempted. The treatment, however, was most unsatisfactory and contained a bad error. For a conclusive criticism of it with some interesting positive suggestions see Broad [12], pt. III, p. 199ff.

[9] The distinction between mathematical and philosophical probability was, as far as we know, made for the first time by Fries in his *System der Logik* (1811). It was later developed by the same author in the *Kritik der Prinzipien der Wahrscheinlichkeitsrechnung* (1842). Fries regarded philosophical probability essentially as an attribute of inductions. (See Fries [3], p. 16ff.) The distinction between the two kinds of probability was current with many authors on induction, probability, and scientific method in the nineteenth century. See Apelt [1], p. 38ff.; Beneke, vol. II, p. 101ff.; Cournot [1], p. 440 and [2], vol. I, p. 71ff. and vol. II, p. 386; Drobisch, p. 177; Grelling, p. 459ff. and p. 478. For the notion of philosophical probability in relation to the notion of simplicity

see especially Cournot [2], vol. I, p. 71ff.; Poirier, p. 107ff.; and Picard [2], p. 436. For a distinction resembling that between mathematical and philosophical probability see also Peirce, vol. II, p. 416ff. In recent times the distinction between probability as an attribute of inductive conclusions and as an attribute of 'events' finds favour with many authors. Some prefer not to call the former 'probability' at all. Thus Carnap, Hempel, Hosiasson-Lindenbaum, Popper, and others call the probability of inductions (hypotheses) *degree of confirmation*; Kneale ([1], §36) speaks of *acceptability*; Braithwaite ([6], p. 120f. and pp. 354-60) speaks of *reasonableness*. These notions with some modern authors cannot, however, all be equated with the notion of *philosophical probability* with the authors mentioned of the nineteenth century (or Grelling). Kneale's concept of acceptability comes very near the classical notion of philosophical probability. But it is important to observe that the distinction made by Carnap and his followers between 'probability$_1$' (degree of confirmation) and 'probability$_2$' is *not* directly comparable with the classical distinction between philosophical and mathematical probability. Carnap's distinction is between probability within a Range-Model (probability$_1$) and probability within a Frequency-Model (probability$_2$) of abstract probability. To call probability$_1$ by the name of 'degree of confirmation' is, in our opinion, somewhat misleading. (See the present chapter §1, fn. 3 and §5, fn. 2.) Mention should also be made of the distinction which Russell makes in [7] between *credibility* and *mathematical probability*. By the former he means the probability of an individual event on *all relevant information* as data. Russell's notion of credibility, it would seem, is thus a limiting case of the generic notion of mathematical probability. (Cf. von Wright [11], p. 302f.)

[10] The possibility of such a formalism is mentioned by Popper ([2], p. 245) with a reference to Hosiasson-Lindenbaum.

CHAPTER VII. PROBABILITY AND THE JUSTIFICATION OF INDUCTION

§1. *Probability and degrees of belief.*

[1] This well-known saying comes from Bishop Butler. (See Butler, p. 3 and *passim.*)

[2] Ramsey, p. 169.

[3] This doctrine is expounded by Ramsey, p. 170ff.

[4] This must not be confused with the fact that, on the above second way of defining degrees of beliefs, if my belief in *a* to degree *p* and in *b* to degree *q* are *true* beliefs, then belief in *a&b* to degree $p \times q$ would be true too.

[5] Consider the well-known psychological phenomena underlying the arguments from 'maturity of odds'. If I toss ten successive times 'heads' with a homogeneous coin I am likely to expect 'tails' rather than 'heads' in the following toss. Various less successful attempts have been made to justify such arguments on the ground of their not having a purely psychological foundation. See Marbe, Sterzinger, and Kammerer. For a criticism of these attempts see v. Mises [2], p. 166-72.

[6] That this is a true interpretation of the view taken by the adherents of the 'psychological' theory is confirmed from the following statements of De Morgan ([2], p. 172f.): 'By degree of probability we . . . ought to mean degree of belief . . . I . . . consider the word (*sc.* "probability") as meaning the state of the mind with respect to an assertion . . . on which absolute knowledge does not exist. "It is more probable than improbable" means . . . "I believe that it will happen more than I believe that it will not happen". Or rather "I *ought* to believe &c.".'

[7] From the quotation in the preceding footnote it is apparent that De Morgan did not realize this consequence of the regulative function of formal probability.

[8] Donkin (p. 354), after mentioning the principle of 'equal distribution of ignorance' as an expression of how beliefs are distributed on a set of alternative propositions, says: 'This being admitted as an account of the way in which *we actually do* distribute our belief in simple cases, the whole of the subsequent theory follows as a deduction of the way in which we must distribute it in complex cases *if we would be consistent*.' This statement, it must be noted, is false. It appears uncertain to the Author whether Ramsey (see especially p. 180, 182 and 188) clearly apprehended this when he described the laws of probability as laws of consistency for partial beliefs.

§2. *Rationality of beliefs and success in predictions.*

[1] This answer, interpreted as here is done, seems to be in exact accordance with Keynes's opinion as to the basis on which rational degrees of belief are determined. See Keynes, p. 17: 'I assume then that only true propositions can be known . . . and that a probable degree of rational belief cannot arise directly but only . . . out of the knowledge . . . of a . . . probability-relation in which the object of the belief stands to some known proposition.'

[2] It might be suggested that a statement concerning symmetry and homogeneity in a coin involves an inductive element. This we are not going to dispute. Our argument only requires the not unplausible *fiction* that the question as to the properties mentioned were settled by reference to *known* (i.e. not-inductive) data concerning weight, shape and centre of gravity of the coin.

[3] According to A_1 on p. 93.

[4] Keynes explicitly expresses the opinion that the criterion of rationality in beliefs is altogether independent of any reference to success in predictions. (See Keynes, p. 107 and p. 322f.) *In spite of this* he strongly insists that degrees of probability, as rational degrees of belief, justify induction. 'The validity of the inductive method does *not* depend on the success of its predictions.' (P. 221.) 'The importance of probability can only be derived from the judgment that it is *rational* to be guided by it in action; and a practical dependence on it can only be justified by a judgment that in action we *ought* to act to take some account of it. It is for this reason that probability is to us the "guide of life".' (P. 323.) As already observed there is no objection to such statements as these, if one is clear about their philosophical implications. Otherwise they might be thoroughly misleading.

[5] This important truth has been clearly apprehended and expressed in Venn [1], p. 150f.; Peirce, vol. II, p. 394; and Ramsey, p. 188, 196 and 202. (Ramsey, p. 199: 'Reasonable degree of belief = proportion of cases in which habit leads to truth.') The idea of probability as a 'guide of life' being a 'guide to success' is contained already in Locke's opinions on probability. (See especially Locke, bk. IV, chap. xv, §1 and §4.) Probability, according to Locke, is 'to supply the defect of our knowledge, and to guide us where that fails . . . the proof being such as for the most part carries truth with it.'

[6] Cf. Kahle, p. 22 and Price, p. 410.

[7] Cf. Jevons [2], p. 261: 'All that the calculus of probability pretends to give, is *the result in the long run*, as it is called, and this really means in *an infinity of cases*. During any finite experience, however long, chances may be against us.' See also Ramsey, p. 207.

[8] What we call the Cancelling-out of Chance is roughly equivalent to the basic principle of inductive arguments which Kaila ([1], p. 9, p. 41 and *passim.*) calls 'Kontingenzprinzip' and Bruns (p. 13) 'gleichmässige Erschöpfung aller möglichen Fälle'.

§3. *The Cancelling-out of Chance and the theorem of Bernoulli.*

[1] For the idea that the Laws of Great Numbers were laws of nature assuring order and uniformity, see e.g. Laplace [4], p. 360f., [6], p. 47; Poisson, p. 7; Quetelet, [3], p. 15, [4], p. 38f.; Lacroix, p. 261. The natural philosophy of the school of Laplace was much inspired by Hume's philosophy of the causal relation and the similar theories of Condillac. (See e.g. Lacroix, p. 3ff.; also Ellis [3], p. 1 and G. Cantor, p. 366.) To the philosophers of the school mentioned the principles of probability served as a kind of 'substitute' for the uncritical belief in the uniformity of nature, which was 'destroyed' by Hume. A much later, and naïve example, of the use of probability as a weapon against Hume is offered by the little book by Masaryk — late president of the Republic of Czechoslovakia—entitled *David Hume's Skepsis und die Wahrscheinlichkeitsrechnung*. (See especially Masaryk, p. 14f.).

[2] De Moivre (p. 252f.) saw in the Laws of Great Numbers a proof of the prevalence of 'Intelligence and Design' in nature. Kant ([5], vol. VIII, p. 17) found them to have some bearing on the problem of the freedom of the will, so also Quetelet ([3], p. 70).

[3] The error in relating the theorem of Bernoulli to the fact called the Cancelling-out of Chance consists, in its gravest form, in that a statement about a probability is believed, as a consequence of the theorem mentioned, to *imply* a statement about a proportion. Distinguished mathematicians and philosophers have been guilty of this error. Some very significant examples are offered by De Morgan ([1], p. 114f. and [2], p. 184). In the latter place the author says that 'it is a remote, but certain, conclusion from the theory . . . that events will, in the long run, happen in numbers proportional to the . . . probabilities'. See also Lambert, vol. II, p. 322.

[4] For a detailed analysis of the relation of probability to possibility see Meinong [1]. James Bernoulli ([1], p. 212) calls probability a *degree of certainty*. He does not speak of *degree of belief*, as does later Laplace. n this point the account given in Kneale [1], p. 124 is in error.

[5] We have already said (fn. 3) that the gravest error in relating the theorem of Bernoulli to the Cancelling-out of Chance consisted in the interpretation of the relation of maximum probability as one of implication. We might now say that the next-gravest error consists in uncritically giving to the theorem the interpretation just mentioned in the text, according to which the increasing probability is taken to be a frequency-constant. Of this error Czuber ([3], vol. I, p. 154) presents a nice example. He says: 'Der Sinn des Bernoullischen Theorems ist dahin zu verstehen, das mit wachsender Versuchszahl die absolute Differenz zwischen der relativen Häufigkeit und der Wahrscheinlichkeit . . . *im allgemeinen* abnimmt.' It is to be noted that Czuber does not *interpret* probability in terms of frequencies but in terms of 'Spielräume'. One must accuse Keynes of a similar error to that made by Czuber, although Keynes on the whole adopts a very guarded attitude to the use of Bernoulli's theorem for predicting averages. See Keynes, p. 109, p. 337, p. 344 and *passim*. Cf. also Cournot [2], vol. I, p. 61 and v. Kries, p. 81.

[6] It might be suggested that one of the reasons why we are apt tacitly to assume a great possibility — in the sense in which it occurs in the theorem of Bernoulli — to be realized frequently, and again a small possibility very seldom, is connected with the following mathematical 'picture'. Take two events such as 'heads' and 'tails' in tossing with a coin. In each toss there are two possibilities — we shall call them *possibilities of the first order* — one of which will be realized. We can denote them with 1 (= 'head') and 0 (= 'tail'). In two tosses there are four such possibilities of the first order, 11, 10, 01, 00. By a *possibility of the second order*, again, we shall mean the class of all possibilities of the first order, containing the same number of digits, which contain, 'heads' and 'tails'

in a given proportion. (E.g. the class [10, 01] is such a possibility of the second order.) Continuing the construction of such rows of digits, each one corresponding to a certain possibility of the first order for the occurrence of 'heads' and 'tails', we shall find that the longer the rows become, the greater is the proportion (among rows with a given number of digits) of rows which contain 'heads' and 'tails' in roughly the same proportion, whereas rows where the distribution of 'heads' and 'tails' differs considerably from the value $\frac{1}{2}$ become the rarer, the longer the rows are. Thus to each possibility of the second order there corresponds a relative frequency for the occurrence of its class-members within the class of *all* possibilities of the first order with the same number of digits. These relative frequencies, furthermore, are found to equal the magnitudes of those possibilities for the occurrence of 'heads' and 'tails' in a given proportion, which are determined by the probabilities of the second order in the theorem of Bernoulli. Consequently this mathematical picture of the possible ways in which 'heads' and 'tails' may be realized contains a frequency-interpretation of the possibilities determined by the theorem of Bernoulli, and it seems highly plausible to assume that this frequency-interpretation will influence our inclination to assume that also the actual realization of those possibilities will take place with frequencies proportionate to their relative frequencies in the 'picture'. Cf. Ellis [3], p. 4.

[7] See e.g. Drobisch, p. 191; Czuber, [3], vol. I, p. 210; Waismann, p. 242. Note also the remark by Quetelet ([2,], p. 12) that if a coin fell regularily twice on one face, while it fell but once on the other, the tossing of the coin might be considered as presenting three possibilities, two of them in favour of one face, and one in favour of the other.

[8] See e.g. v. Kries, p. 5ff. and compare it with the analysis which follows in the text.

[9] Or, if *general* propositions are also included in the class of propositions mentioned, a statement about proportions cannot follow from them unless one at least of those general propositions is itself a statement about proportions. This trivial truth is of far-reaching importance. From it follows, for example, that from the knowledge of the physical constitution of a die, together with the known or assumed laws of Newtonian mechanics we can never deduce that the proportion of, say, aces will be such and such. This would be possible only if this bulk of knowledge itself contained some assumption about proportions, e.g. the assumption that certain causes, operating according to the rules of classical mechanics, are divided into different categories in determinate proportions. (See also v. Mises [2], p. 90ff.)

[10] It is to be observed that the suggested measurement on the basis of properties of symmetry merely served as an illustration. What is said here applies to all conceivable ways of measuring degrees of possibility.

[11] On the other hand the theorem of Bernoulli, when probability is interpreted statistically, ought not to be taken as asserting the Cancelling-out of Chance itself. For the Cancelling-out of Chance only implies that events will be realized in the long run with frequencies proportionate to their probabilities, whereas the theorem of Bernoulli contains an assertion about the frequencies with which frequencies of the event, in finite series of occasions, will be realized, *supposing that the Cancelling-out of Chance takes place*. This difference is sometimes overlooked. (See e.g. Poisson, p. 7 and Charpentier, p. 35 for such confusions. For a clarification of this important point see v. Mises [2], p. 129f. and p. 136.)

[12] See Leibniz [4], vol. III, p. 79-98 and M. Cantor [2], vol. III, p. 339f. Leibniz (in the letter from 3. XII, 1703) in opposition to Bernoulli's idea that one could determine the probable value of human life with ever-increasing probability on a statistical basis, makes the very acute observation that '*novi morbi inundant subinde humanum genus, quodsi ergo de mortibus quotcunque experimenta feceris, non ideo naturae rerum limites*

posuisti, ut pro futuro variare non possit'. Bernoulli (the letter from 20. IV, 1704) was wholly unable to grasp the epistemological significance of this ingenuous remark of Leibniz.

It is interesting to observe that also mathematicians of the school of Laplace were not altogether unaware of the necessity of making certain inductive assumptions if the calculus of probabilities were to be applicable to proofs concerning future events, although they never realized the epistemological significance of this necessity. (See e.g. Laplace [6], pp. 14, 48, and 53f., and Condorcet, p. 10.)

The knowledge that the theorem of Bernoulli and other propositions of the probability-calculus can be applied to inductive predictions solely on the condition that we have already made *some* assumptions as to the future, is related to the well-known statement that 'probability presupposes causality'. (See Mach [3], p. 283, and Struik, p. 51 and p. 65f.) For analyses of the causal conditions for this applicability see Poincaré [3], p. 64-94 and Hopf.

§4. *The idea of 'probable success'*.

1 By the truth-frequency of a proposition we mean the proportion of values of a variable, which satisfy a certain propositional-*function*, among all values of the variable in question. (See above chap. VI, §2, p. 98f.)

2 See chap. II, §7.

3 See above p. 143f.

4 Zilsel is of the opinion that the fact which impresses us as the '*Ausgleich des Zufalls*' obtains its character of an unquestionable truth from a *convention*. (See Zilsel, especially p. 123.) Zilsel's analysis appears to us essentially right but, as Kaila ([1], p. 69ff.) acutely observes, the statement on the Cancelling-out of Chance, *when used as a basis for induction*, must be taken as synthetical. It deserves mention that Ellis, who gave the first substantially correct detailed criticism of Bernoulli's theorem as a bridge from probabilities to empirical frequencies, believed the Cancelling-out of Chance to be a *synthetical* truth *a priori* roughly in a Kantian sense. See Ellis [3] and also Fick, p. 2 and p. 46.

5 Cf. E. J. Nelson, p. 580: 'No "justification" is worthy of respect unless it is based upon principles the application of which will in theory *probably* lead to success.'

6 Cf. chap. VI, §8, p. 136 and fn. 9, p. 217.

7 Keynes' chief objection against the frequency-interpretation is that it makes probability-statements *inductive* and consequently deprives probability of the power of justifying induction. 'The Calculus of Probabilities', he says (p. 96 fn.), 'thus interpreted, is no guide by itself as to which opinion we ought to follow.' Therefore, according to Keynes (p. 95) the statistical interpretation can at most only cover *part* of what we mean by probability, the other sense in which probability is used being that justifying induction. 'It is, in my opinion', he continues (p. 96), 'this other sense *alone* which has importance'. See also Broad [11], p. 487 for the suggestion that, since probability-statements in frequency-interpretation are inductive, another kind of probability might be needed for judging the likelihood of those statements' truth.

8 It is questionable whether all supporters of 'philosophical' probability have clearly apprehended this. Both Fries ([3], p. 18) and Cournot ([2], vol. II, p. 386) decisively state that philosophical probability cannot be estimated *numerically*. The latter author for example says (ibid.) that *'la probabilité philosophique répugne tout à fait à une évaluation numérique'*. It is, however, plausible to assume that these authors wished to deny only a *metrical*, and not a *topological* quantification of philosophical probability, although they did not explicitly state the necessity of the latter.

[9] Chap. VII, §2.

[10] Cf. Popper [2], p. 4 and Feigl [2], p. 25.

[11] Probability taken as a 'Grundbegriff', in other words, cannot justify induction as leading to 'probable success'. It is questionable whether authors such as Keynes and Jeffreys, who regard probability as a kind of undefinable fundamental idea, have clearly apprehended how 'empty' of empirical content is this undefinable probability-concept.

[12] One might object to our view that it moves in the grooves of the 'classical' two-valued logic, and that the nature of inductive argument cannot be grasped until we have left this logic for the more comprehensive system of a 'probability-logic'. (Cf. Reichenbach [4], p. 360 and p. 377.) To this objection the following rejoinder will be sufficient: It is possible to develop a formal system, treating degrees of probability, which exhibits certain analogies to the formalized systems of two-valued logic. This system we might then call 'probability-logic'. (For the development of such a system see e.g. Reichenbach [4], p. 379ff. and [5].) But from the mere fact that such a formalism can be developed nothing follows as to the justification of induction. For its capacity to justify induction the probability-*logic* is dependent on the same conditions as *any* formalism of probability, and those conditions, we have tried to show, are such as to make any argument that probability were the 'guide to success' circular. (See also Tarski, p. 174ff. and Hertz.)

[13] It is, in our opinion, evident that this fallacy of thought underlies the whole of Keynes's reasoning as to probability and the justification of induction. Consider, for example, the following passage: Keynes states (p. 309) that although it is not *certain* that we shall ultimately succeed in preferring the more probable to the less probable, the success may nevertheless be *probable*. That success is 'probable' implies—this seems to be Keynes's opinion here—that we shall 'generally' or 'on the whole' succeed in preferring the more probable to the less probable. Concerning this implication as to future frequencies he states that it need not be certain. It is, evidently, sufficient only to assume it to be itself 'probable'. On this point Keynes breaks the hierarchy of superimposed probabilities. This is fatal, because his argument then becomes supported by a new statement on frequencies which remains unformulated but is nevertheless implicitly contained in the reasoning.

[14] Hume [2], p. 15: 'Nay, I will go further, and assert, that he could not so much as prove by any *probable* arguments that the future must be conformable to the past. All probable arguments are built on the supposition, that there is this conformity betwixt the future and the past, and therefore can never prove it. This conformity is a *matter of fact.*' See also Hume [1], pt. IV, §1 and E. Cassirer, vol. II, p. 264.

[15] Hume [1], bk. I, pt. IV, §1: 'As demonstration is subject to the control of probability, so is probability liable to a new correction by a reflex act of the mind . . . Here then arises a new species of probability to correct and regulate the first . . . and so on *ad infinitum.*'

[16] Hume [2], p. 15.

§5. *Logical and psychological, absolute and relative justification of induction with probability.*

[1] This truth is usually expressed by saying that all induction proceeds upon the principle that the future will resemble the past, and that this principle must be taken for granted without proof, since it is itself the basis of all proofs concerning the future. Cf. Hume [2], p. 15, Poirier, p. 216 and Ölzelt-Newin.

[2] Cf. chap. VI, §2.

[3] See e.g. Mill, bk. III, chap. IV, §2: 'Experience testifies, that among the uniformities which it exhibits or seems to exhibit, some are more to be relied on than others; and

uniformity, therefore, may be presumed, from any given number of instances, with a greater degree of assurance, in proportion as the case belongs to a class in which the uniformities have hitherto been found more uniform.'

[4] For attempts in the same direction see Poirier, p. 94ff., and Reichenbach [4], §71, [7], p. 274ff., and [9], p. 348-73.

[5] Popper [2], p. 192.

[6] For an argument in support of this attitude see von Wright [11], p. 243f. See also Nagel [5], pp. 60-75.

CHAPTER VIII. INDUCTION AS A SELF-CORRECTING OPERATION

§1. *Induction the best mode of reasoning about the unknown. The ideas of Peirce.*

[1] The characterization of the use of induction as being the adoption of a *policy* seems to be of recent origin. It is used by Kneale (especially [1], pt. IV) and Braithwaite [5] and [6]. With the authors mentioned and some other recent authors (Reichenbach, J. O. Wisdom) the problem of the justification of induction may be said to have undergone a transformation. From having been a problem of ascertaining the conditions of truth or of probability in single inductive conclusions, it has become a problem of showing the superiority of the inductive policy, *as such*, over rival policies. The change in attitude is expressed by Kneale ([1], p. 225f.) as follows: ' . . . in order to justify induction we must show it to be rational without reference to the truth or even to the probability of its conclusions, we must conceive of it as a policy to be adopted or rejected and then make clear that no one who understands his situation . . . can fail to choose this policy.' For a critical examination of this 'pragmatic' or 'practicalist' approach to the problem of induction see Black [2], pp. 157-90.

[2] Peirce, vol. I, p. 28. For Peirce's opinions on induction and their development see Braithwaite [4], p. 500ff. and Goudge.

[3] Peirce, vol. II, p. 455f.

[4] Peirce, vol. II, p. 501f.

[5] Chap. I, §2.

[6] Ellis ([4], p. 49f.) gives an interesting analysis of this assumption. He appears to have been of the opinion that the statement 'on a long run of similar trials, every possible event tends ultimately to recur in a definite ratio of frequency' were a kind of synthetical truth *a priori*, following from the nature of *genera* and their *species*. (Cf. chap. VII, §4, fn. 4.) Marbe has tried to show that there exist series of statistical observation *not* having definite proportions, i.e. limiting-frequencies, for the occurrence of their characteristics. It appears however, as though Marbe had drawn unwarranted conclusions from his experiments. See von Mises [2], p. 166ff.

[7] Reichenbach [4], p. 396. Reichenbach himself does not seem to be aware of the intimate relationship between his solution of the inductive problem and the ideas of Peirce. Actually almost everything that is true and essential in the views of Reichenbach on the justification of induction has already been explicitly stated by Peirce.

[8] The proof is roughly as follows: Suppose the number m did not exist. This would imply that over and over again it happened that the proportion of A's which are B fell outside the interval $p \pm \epsilon$, for some ϵ. From this again it follows that there must exist a value p' outside the interval such that the frequency over and over again falls in the interval $p' \pm \epsilon$, for any ϵ. But this contradicts (3), which asserts that there exists one *and only one* value p such that the relative-frequency of A's which are B for any ϵ over and over again falls in the interval $p \pm \epsilon$. Thus if (4) is false (3) would also be false. Conversely, if (3) is true, (4) must be true.

[9] This is Reichenbach's main addition to the argument of Peirce. Cf. Rieichenbach [4], p. 415.

§2. *Reichenbach's Method of Correction.*

[1] The full description is in Reichenbach [4], §77.

[2] This is, roughly speaking, the content of Reichenbach's Rule of Induction. See Reichenbach [4], §§76 and 80.

[3] The posit of the first order consists, strictly speaking, in the assumption that the limiting-frequency falls within certain limits $\pm\varepsilon$ of p. Reference to the ε will be omitted from our simplified account of the method.

[4] That is, falls within some interval $\pm\varepsilon$ round a 'mean'. For the sake of simplicity, however, we shall assume that we need only consider exact coincidences of values.

[5] See Reichenbach [4], p. 399ff.

[6] This problem is substantially the same as the well-known problem of inverse probability which we treated in outline above in chap. VI, §3. The values $f(q_i)$ answer to the *a priori* probabilities, the values $f(q_i, q_k)$ answer to the eductive probabilities, and the calculated values $F_{i,k}$ to the *a posteriori* probabilities.

[7] Sometimes there may, for a given i, exist more than one value i', such that $F_{i,i}$ $F_{i,i'}$ equals $F_i max$. It is not clear, how the correction should be carried out, if this possibility happened to be true. Reichenbach does not consider the case. (It might be added that, for sufficiently great values of n, this possibility does no longer occur.)

[8] To say that a posited value is corrected, comes therefore to the following: A sequence in which the limiting frequency of A is the *same* as the recorded relative frequency of A, is regarded as *less usual* than a sequence in which the limiting-frequency *differs* (in a certain assigned way) from the recorded relative frequency of A in the observed initial sequence of n members. We may, of course, be mistaken. The sequence may, after all, be of the unusual kind, and the correction unwarranted. But if this be the case, we can be sure that continued use of the method of correction will finally 'put things right', i.e. make us revert to the value first posited — in accordance with the inductive principle — on the basis of the recorded relative frequency of A in the intial segment of n members of the sequence. That such will happen follows from the fact that, independently of the values $f(q_i)$, the values $F_{i,i}$ converge towards 1 with increasing n. (Cf. above chap. VI, §3, p. 107ff.) We can thus be sure that, for a sufficiently large n, the value of $F_{i,i}$ will equal $Fmax$. (See Reichenbach [4], p. 401f.)

[9] It is not quite clear from the exposition given in Reichenbach [4], whether the author has realized the significance of this point. See, for example, the discussion in Reichenbach [4], p. 413f. of the question, whether another policy than induction (e.g. that of consulting an oracle about the true values of the proportions) might lead to a quicker approximation to the truth.

[10] On this point it will be useful to remember a passage in the dispute between Leibniz and James Bernoulli on the epistemological value of the inverse Law of Great Numbers. Bernoulli assumed that the records of statistical observation supply us with approximate values of probabilities, which values may be corrected by extended observations. This process of correcting the values he compares to the calculation of new digits of π, i.e. to a calculation which can correctly be called an approximation to the truth. (Leibniz [4], vol. III, p. 91f.) To this Leibniz (ibid., p. 94) acutely observes that the analogy is fallacious. In calculating the digits of π, each new digit is *known* to take us *nearer* the true value. But whether new observations will take us nearer to the true values of the proportions about which we generalize is uncertain. That they will do so is an *assumption* which is essential to the use of induction, but whether it is true or not we cannot know, not even with 'probability .

§3. *The goodness of inductive policies reconsidered.*

[1] The idea that inductive policies are self-correcting has been severely criticized by Black ([2], pp. 168-73). According to Black (ibid., p. 170) the term 'self-corrective' is a misnomer. A *modification* which experience may lead us to make in our generalizations, can properly be called a *correction* only if there is some assurance that the modifications will progressively take us nearer to the truth. As we have seen (this chapter, §1) such an assurance can exist only *relative to* the (unprovable) assumption that the proportions, about which we generalize, do really exist. Of the necessity of making this assumption Peirce, as mentioned above (p. 161), was not even aware. Reichenbach ([4], §80) explicitly avowes it and calls it the assumption that the world is 'predictable'. J. O. Wisdom introduces a related assumption, which he calls the assumption of a 'favourable' universe. (Wisdom [2], p. 226ff.)

[2] See above chap. I, §1 and §2 on the notion of a generalization.

[3] We shall not here substantiate this doubt with further reasons for it. If it is well-grounded, it puts a serious limitation upon the value of the Peirce-Reichenbach approach to the problem of induction. For the Peircean idea of induction as a self-correcting approximation to the truth has no immediate significance, it would seem, for other types of inductive reasoning than statistical generalization.

[4] Cf. Black [2], p. 158 and p. 172.

[5] For the notion of a counter-inductive policy see Black [2], p. 171ff.

[6] See above chap. I, §2 and chap. VIII, §1.

[7] See above chap. I, §2, fn. 8.

[8] The possibility of a policy for purposes of prediction and generalization about oscillating frequencies shows that material assumptions concerning the constitution of the universe such as those made by Reichenbach and Wisdom (see above fn. 1) are not needed in order to warrant successful use of induction.

[9] Cf. Reichenbach [4], §80.

[10] Cf. above chap. V, §2, fn. 1.

CHAPTER IX. SUMMARY AND CONCLUSIONS

§1. *The thesis of the 'impossibility' of justifying induction.*

[1] Whitehead [1], p. 30.

[2] 'Inductive Reasoning . . . the glory of Science . . . the scandal of Philosophy'. This often quoted characterization is from the concluding sentence in Broad [6]. See also Ramsey, p. 197. Ramsey's remarks on the nature of the problem of Hume seem to us to 'hit the nail on the head.'

[3] Russell [4], p. 167.

[4] Russell [3], p. 14. See also Russell [5], p. 481. For a criticism of some of Russell's earlier opinions on induction see Smart, and for a critical appraisal of some of his later views on the topic see Edwards, Hay [1], McLendon and Reichenbach [14].

§2. *The logical nature of Hume's 'scepticism'.*

[1] For the idea that a contradiction or antinomy is inherent in the demand for a justification of induction see the acute analysis in Oxenstierna, especially p. 27ff.

[2] Although Hume's results as to the impossibility of justifying induction are, in our opinion, fundamentally right and expressed with extraordinary clarity and convincingness, it is obvious that he himself did not take the view that they were 'grammatical' in nature. This is clearly seen from Hume [1], bk. I, pt. IV, §7, where he considers the consequences of his results for practical life.

In Keynes, readers will find an extensive bibliography of mathematical and philosophical writings on probability and related subjects such as induction and statistics, up to 1921. Carnap [11] lists most of the relevant publications on inductive logic and the foundations of probability and statistical methods up to 1950.

The primary aim of the present Bibliography is to list books and papers on the *epistemological* problems of induction and probability. It does not include works on probability mathematics and statistical theory, with the exception of a few works of a historical or synoptic nature and some major contributions to the axiomatic foundations of probability and to the doctrine of inverse probability. Nor does it list the rapidly growing literature on the problem of conditionals, a topic related to inductive theory, nor literature on the causal *versus* statistical nature of physical laws, nor on scientific explanation and scientific method in general. But it aims at including all more important contributions from recent years to Confirmation-Theory.

The titles of books and longer publications are in *italics*, the titles of papers are in quotes. The classification of a work as belonging to the one or to the other of these two categories is sometimes a matter of taste.

Following continental usage, we have listed names with the prefix *de*, *del*, *van*, or *von* under the first letter after the prefix.

The following abbreviations of the names of some of the best known periodicals in the field of the present work have been used: *AJP* for the *Australasian Journal of Philosophy*, *AP* for *Année Philosophique*, *BJPS* for *The British Journal for the Philosophy of Science*, *JP* for *The Journal of Philosophy*, *JSL* for *The Journal of Symbolic Logic*, *JUS* for *The Journal of Unified Science*, *PAS* for *Proceedings of the Aristotelian Society*, *PPR* for *Philosophy and Phenomenological Research*, *PR* for *The Philosophical Review*, *PS* for *Philosophy of Science*, *RM* for *The Review of Metaphysics*, *RMM* for *Revue de Métaphysique et de Morale* and *RP* for *Revue Philosophique*.

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