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PSYCHOLOGY AND PHILOSOPHY

I.—HR. VON WRIGHT ON THE LOGIC OF INDUCTION * (I.).

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HR. VON WRIGHT is already known to those readers of MIND who are interested in Inductive Logic and Probability by his article on *Probability* in Vol. XLIX. The longer of the two essays now to be discussed is the thesis which he submitted successfully in 1941 for his doctor's degree at the University of Helsingfors; the shorter is an elaboration of Chapter IV of the thesis, contributed by him to a Scandinavian philosophical journal of which I do not know the name. The former is in English, the latter in Swedish. Together they constitute, so far as they go, the best treatment known to me of the problem of Induction. For this reason, and because it is most unlikely that either of them will be generally accessible in England before the end of the non-Japanese part of the present war, I propose to write an article round them rather than a review of them in the ordinary sense of the word.

I will begin by saying that the thesis is written in excellent English. It is not for me to comment on the Swedish of the short article; but I found it perfectly clear and easy to follow, and it is reasonable to suppose that an author who can express himself in a foreign language so well as Hr. von Wright has done would handle his native tongue with even greater skill.

* G. H. von Wright: (1) "The Logical Problem of Induction," *Acta Philosophica Fennica*, Fasc. 3, 1941. Pp. 257. (2) *Några Anmärkningar om nödvändiga och tillräckliga Betingelser*. Pp. 20.

The scope of the thesis is best indicated by the following summary of its contents. Chapter I is a brief introduction. Chapters II and III, entitled respectively *Induction and Synthetic Judgments a priori* and *Conventionalism and the Inductive Problem*, deal with what Hr. von Wright calls attempts to justify induction *a priori*. Chapter IV, entitled *Inductive Logic*, is concerned with attempts to justify induction *a posteriori* as leading to conclusions which are certain. It contains Hr. von Wright's treatment of necessary and sufficient conditions and the formal logic of methods of elimination such as Mill's. It must be taken along with the short article in Swedish on *Necessary and Sufficient Conditions* which carries the matter further. Chapters V, VI, VII, and VIII, entitled respectively, *Induction and Probability*, *Formal Analysis of Inductive Probability*, *Inductive Probability and the Justification of Induction*, and *Induction as a Self-correcting Process*, are concerned with attempts to justify induction *a posteriori* as leading to conclusions which are only probable. Chapter IX is a summary of the results reached in the course of the work. It is followed by 50 pages of *Notes*, and a *Bibliography* containing the names of 188 authors and 292 of their publications. The Notes and the Bibliography bear ample witness to the wide and deep foundation of knowledge on which Hr. von Wright has built his own conclusions, and they should be most useful to anyone who is working at the subject.

(I) PRELIMINARY CONSIDERATIONS.

(1) *Statistical Propositions.*—Let us call any proposition of the form 'p % of the instances of Q are instances of R' a *Statistical Proposition.* Here p may have any value from 0 to 100, both inclusive. Now Hr. von Wright makes a very important point about the distinction between 100 % and 0 % statistical propositions, on the one hand, and Universal Affirmative and Universal Negative propositions, respectively, on the other. If the class of instances of Q is finite (*e.g.*, the counters in a certain bag or the throws which have been made with a certain coin), the proposition '100 % of Q's are R' is logically equivalent to 'All Q's are R' and the propositioon '0% of Q's are R' is logically equivalent to 'No Q's are R'. But, if the class of instances of Q is in principle unlimited (*e.g.*, the series of possible throws with a coin), the meaning of 'p % of Q's are R' has to be carefully defined; and, when this is done, it is found that the equivalences break down and are replaced by one-sided entailments.

Hr. von Wright's definition of the statement 'the proportion of Q's which are R in an unlimited sequence of Q's is p' may be summed up in the following two propositions. Let us first define the phrase 'an initial segment of a sequence' to mean any sub-sequence which consists of all the terms of the original sequence from the first to any given term, both inclusive. Then the two conditions which must be fulfilled if the proportion of Q's which are R in an unlimited sequence of Q's is to be p may be stated as follows:

(i) However small ε may be, every initial segment of the sequence of Q's is contained in a longer initial segment for which the proportion of Q's which are R does not differ from p by more than $\pm\varepsilon$; and

(ii) For any ratio other than p there is a quantity ε and an initial segment S_n such that for every initial segment which includes S_n the proportion of Q's which are R differs from this ratio by more than $\pm\varepsilon$.

Stated colloquially these two conditions come to this. As the sequence of Q's is extended further and further the proportion of them which are R reverts again and again to the immediate neighbourhood of p and does not revert again and again to the immediate neighbourhood of any other fraction.

Now it is important to notice the following facts. Either or both of these conditions might fail to be fulfilled. There might be no ratio to which the proportion of Q's which are R again and again reverts, or it might revert again and again to several different ratios. In either case there would be nothing that could be called *the* proportion of Q's which are R in the indefinitely extended sequence of Q's. Again, it is quite possible that the proportion of Q's which are R in an indefinitely extended sequence should be 100 % and yet that there should be Q's in the sequence which are not R. Indeed, whatever number we choose to mention, there might be more Q's than this in the sequence which are not R. Similarly, the proportion of Q's in the sequence which are R might be 0 %, and yet there might be more Q's in it which are R than any number that we choose to mention. Let us call statistical propositions in which the proportion is 100 % or 0 % 'Extreme Statistical Propositions'. Then the position is that, if the sequence is indefinitely extensible, universal affirmative or negative propositions entail extreme statistical propositions, but the converse does not hold. This is an exceedingly important point which has often been overlooked by writers on Induction, including (I am ashamed to say) myself.

It is worth remarking, as Hr. von Wright does on page 181 of the thesis, that conditions (i) and (ii) together entail the following proposition.

(iii) However small ε may be, there is an initial segment S_m such that for every initial segment which contains it the proportion of Q's which are R differs from p by not more than $\pm\,\varepsilon$.

Stated colloquially this means that, if conditions (i) and (ii) are fulfilled, there is a stage in the sequence after which the ratio of Q's which are R to Q's remains in the immediate neighbourhood of p. This can be proved by showing that the conjunction of (i) with the denial of (iii) entails the denial of (ii). This may be left as an exercise for the reader.

Before leaving this part of the subject we shall find it worth while to introduce a suitable notation to express the ideas outlined above. Let $f_n(\mathrm{R}\,;\ \mathrm{Q})$ denote the proportion of Q's which are R in the first n of the sequence of Q's. Let $f(\mathrm{R}\,;\ \mathrm{Q})$ denote the proportion of Q's which are R in the unending sequence of Q's. Let $x \overset{\vee}{=} p \pm \varepsilon$ denote that x does *not* differ from p by more than ϵ; and let $x \overset{\wedge}{=} p \pm \varepsilon$ denote that x *does* differ from p by more than ε. Then what we have done is to define $f(\mathrm{R}\,;\ \mathrm{Q})$ in terms of $f_n(\mathrm{R}\,;\ \mathrm{Q})$; and to show that $f(\mathrm{R}\,;\ \mathrm{Q}) = 1$ is compatible with an indefinitely large number of Q's *not* being R, whilst $f(\mathrm{R}\,;\ \mathrm{Q}) = 0$ is compatible with an indefinitely large number of Q's *being* R. The symbolic expressions for propositions (i), (ii), and (iii) are as follows :

$$\left\{\begin{array}{l}\text{(i)}\ \ (\varepsilon, n) : (\exists \mathrm{N}) \,.\, \mathrm{N} > n \;\&\; f_{\mathrm{N}}(\mathrm{R}\,;\mathrm{Q}) \overset{\vee}{=} p \pm \varepsilon \\ \text{(ii)}\ \ q \neq p \,.\, \supset_q : (\exists \varepsilon, n) \,.\, \mathrm{N} > n \supset_{\mathrm{N}} f_{\mathrm{N}}(\mathrm{R}\,;\mathrm{Q}) \overset{\wedge}{=} q \pm \varepsilon \end{array}\right.$$

$$\text{(iii)}\ \ (\varepsilon) : (\exists n) \,.\, \mathrm{N} > n \supset_{\mathrm{N}} f_{\mathrm{N}}(\mathrm{R}\,;\mathrm{Q}) \overset{\vee}{=} p \pm \varepsilon.$$

Hr. von Wright condenses conditions (i) and (ii) into a single formula. This is perfectly legitimate, but I think that it makes things clearer to express them separately. So far as I can see Hr. von Wright's formula contains two small errors of detail. In the first place, an implication-sign is written where the symbol '&' seems to be plainly required; and secondly he writes the symbol '$=$' where the symbol '$\overset{\vee}{=}$' or some equivalent for it is needed.

(2) *Inductive Inferences.*—The premiss of an inductive argument is either (1·1) a *singular* proposition of the form 'This instance of Q is R', or (1·2) a *statistical* proposition of the form 'p % of the n instances of Q which have been observed are R'. This may be either (1·21) *extreme, i.e.*, p may be 100 % or 0 %, or (1·22) *intermediate, e.g.*, $p = 47$ %. The conclusion of an

inductive argument may be either (2·1) a *singular* proposition of the form 'The next instance of Q to be examined will be R'; or (2·2) a *statistical* proposition, which may be either (2·21) *extreme*, or (2·22) *intermediate*, *i.e.*, may state that 100 % or 0 % or some intermediate percentage of the unending sequence of Q's will be R; or (2·3) a *universal* proposition (affirmative or negative), *i.e.*, that all (or none) of the unending sequence of Q's will be R.

If a premiss of the form (1·1) is combined with a conclusion of the form (2·1) we have an inductive inference from *Singulars to Singulars*. If a premiss of the form 'p % of the n observed Q's are R' is combined with a conclusion of the form 'p % of all the Q's will be R', we have a *Statistical Generalisation*, no matter whether p be 0 or 100 or some intermediate percentage. If a premiss of the form (1·21) is combined with a conclusion of the form (2·3), *i.e.*, if we infer a universal proposition about the unending sequence of Q's from an extreme statistical premiss about the n Q's which have been examined, the argument is a *Universal Generalisation*. As Hr. von Wright points out, there has been a tendency among writers on induction to confuse 0 % and 100 % statistical generalisations, on the one hand, with negative and affirmative universal generalisations, respectively, on the other.

The above are the most important types of Inductive Inference. Hr. von Wright subdivides Universal Generalisations into four kinds. His classification does not seem to be very systematic. In point of fact they can be divided, as follows, into four main species; and the third of these can be sub-divided into five sub-species and the fourth into four; so that in all there will be eleven ultimate sub-divisions. The classification proceeds as follows.

Let ρ and σ be two relations. Then (1) we have the simplest type, where the conclusion involves no relations but is of the form 'All Q's are R'. (2) Next we introduce one relation ρ. Then we have a generalisation involving just one quality and one relation, *viz.*, 'Every pair of instances of Q stand in the relation ρ to each other'. (3) Next we have generalisations involving one relation and two qualities. Plainly there are five possibilities here. I will give in words two of them. (3·1) 'All instances of Q stand in the relation ρ to some instance of R'; and (3·4) 'Everything to which an instance of Q stands in the relation ρ is an instance of R'. (4) Lastly, we introduce a second relation σ. This gives rise to four possibilities. I will state the first and the fourth in words. (4·1) 'Everything which has ρ to any instance of Q has σ to some instance of R'; and (4·4) 'To anything to which an instance of Q stands in the relation ρ an instance of R stands in the relation σ'.

These eleven possibilities can be symbolised as follows in Russell's and Whitehead's notation, if we write $\hat{Q}$ for the class of instances of Q and $\hat{R}$ for the class of instances of R.

(1) $\hat{Q} \subset \hat{R}$.

(2) $x, y, \varepsilon \hat{Q} \supset_{x,y} \rho(x, y)$.

(3·1) $\hat{Q} \subset \rho``\hat{R}$. (3·2) $\hat{Q} \subset \breve{\rho}``\hat{R}$. (3·3) $\rho``\hat{Q} \subset \hat{R}$. (3·4) $\breve{\rho}``\hat{Q} \subset \hat{R}$.
(3·5) $x \varepsilon \hat{Q} \,\&\, y \varepsilon \hat{R} \supset_{x,y} x\rho y$.

(4·1) $\rho``\hat{Q} \subset \sigma``\hat{R}$. (4·2) $\breve{\rho}``\hat{Q} \subset \sigma``\hat{R}$. (4·3) $\rho``\hat{Q} \subset \breve{\sigma}``\hat{R}$.
(4·4) $\breve{\rho}``\hat{Q} \subset \breve{\sigma}``\hat{R}$.

The four cases which Hr. von Wright distinguishes are our (1), (2), (3·1) and (3·4). He calls (3·1) 'Existential Hypotheses'. (3·2) is the heading under which Uniformities of Sequence fall. For any such proposition is of the form 'Any instance of Q is immediately followed in time and adjoined in space by an instance of R'.

The logical problem of justifying the transition from the 100 % statistical premiss based on n observed instances to the universal conclusion is precisely the same for all the eleven cases, so we can confine our attention henceforth to the simplest of them.

(II) ATTEMPTS TO JUSTIFY INDUCTION *A PRIORI*.

When Hr. von Wright calls an attempt to justify the generalisation 'All instances of Q will be R' *a priori* what he means is this. The fact that n instances of Q have been observed and that 100 % of them were R is to be no part of the *premisses* from which 'All Q's will be R' has been *inferred*, whether with certainty or with probability. The observations may have been an indispensable pre-requisite psychologically, *e.g.*, it may be that without them no-one would have had an idea of the characteristics Q and R or would have entertained the notion of their being conjoined. The question is whether, when all these psychological pre-conditions have been fulfilled, a person can know or rationally conjecture that the presence of Q *necessarily* carries with it that of R. If this were possible in any case, he could then infer that every instance of Q would be an instance of R. There are two alternatives to be considered, *viz.*, the claim that causal laws are necessary and synthetic and the claim that they are analytic.

(1) *Causal Laws as Synthetic Necessary Propositions.*—It seems to me that there are two ways of attacking such attempts to justify inductive generalisations. One is analytical, the other is epistemological. The analytical way is to analyse the notions of 'necessary' and 'synthetic', as applied to facts or propositions, and to try to show that the notion of a necessary synthetic fact or proposition is meaningless. This is the more radical method; for, if there can be no such facts or propositions, it follows at once that no-one can know any of them. The epistemological way is to leave the possibility of synthetic necessary facts or propositions an open question; but to argue that, if there are any, we are never in a position to know them. This contention might itself take a more or less radical form. It might be argued (i) that, with regard to all synthetic propositions about nature, we *can* see that they *are not* necessary; or (ii) that, with regard to no synthetic proposition about nature, can we see that it is necessary. The milder form of the epistemological contention would be enough to wreck attempts to justify inductive generalisations *a priori*. Hr. von Wright takes the radical analytic path; and obviously if it is open to traffic it is the shortest and quickest.

Hr. von Wright contends that the proposition 'There can be no necessary synthetic propositions' is itself necessary and analytic. The essence of his argument is as follows. Consider the statement 'All instances of Q are necessarily instances of R'. This means that the proposition 'This is an instance of R' *follows from* 'This is an instance of Q'; and this means that the disjunctive proposition 'This is either not-Q or R' is a *tautology*. On the other hand, to say that 'All instances of Q are instances of R' is synthetic means that the two characteristics Q and R are *not* such that to be R follows logically from being Q. Therefore the statement 'There can be no necessary synthetic propositions' is itself a tautology.

Hr. von Wright realises that no-one who believes that there are or may be necessary synthetic propositions is likely to be much moved by this line of argument. The immediate reaction of such a person will be to say: 'I distinguish between purely formal or logical necessity and another kind of necessity. Even if purely formal necessity can be defined in terms of following logically, and even if the latter can be defined in terms of tautology, I am concerned, not with it, but with what may be called *non-formal* necessity'.

His answer to such a contention might be put as follows: 'Your non-formal necessity will be irrelevant for the present purpose unless you can infer from it that every instance of Q will be R. Now suppose (what you cannot deny to be intelligible and possible) that someone were to allege that an instance of Q which was not R had been found. What are you to say about it? Either (i) you may admit that it is a genuine counter-instance, or (ii) you may deny this. On the first alternative you will have to admit that you were mistaken in thinking that the presence of R follows necessarily but non-formally from that of Q. On the second alternative you will have to say either (*a*) that this was not really an instance of Q, though it seemed to be one, or (*b*) that this was really an instance of R, though it seemed not to be one. Suppose that renewed and more careful observation of the alleged counter-instance shows that it answers all the tests hitherto accepted for the presence of Q and that it fails to answer some of the tests hitherto accepted for the presence of R. At this stage you can save the situation only by refusing to call anything an instance of Q unless it is manifestly an instance of R also; or by insisting on calling a thing an instance of R, in spite of all appearances to the contrary, if it is an instance of Q. But in that case you have made being R part of the definition of being Q, and you have saved your non-formal synthetic necessity only at the price of turning it into a formal analytic necessity.'

Hr. von Wright thinks that the above represents the kernel of Hume's argument about Causation, when stripped of epistemological and psychological features which were non-essential. He next considers Kant's attempt to answer Hume.

According to Hr. von Wright Kant did prove something important in the Analogies of Experience and he did thereby fill a serious gap in Hume's philosophy, but what he proved was quite irrelevant to the question of justifying inductive generalisations. Kant showed that all transition from sense-data propositions to physical-object propositions depends upon certain invariant relations among sense-data. *If* intersubjective intercourse is to remain possible, *if* it is to remain possible to draw a distinction between the temporal order in which each of us happens to get his sensations and an objective temporal order of physical events, certain very general kinds of regularity, which have held in the past, must continue to hold in the future among our sensations. But we have no means of knowing that the antecedents of these hypotheticals will always be fulfilled.

Moreover, as Kant came to recognise when he wrote the *Critique of Judgment*, even if he had established the Law of Universal Causation as an absolutely, and not merely a hypothetically, necessary proposition, he would not have taken us a step towards justifying any particular inductive generalisation.

Hr. von Wright next gives an account of the views of Fries and his school. These philosophers pointed out that no answer can be given to Hume along Kantian lines. They held that certain synthetic propositions are, and can be seen by inspection to be, necessary, and that it is a mistake to ask for a proof of them. This type of theory was most fully stated by Apelt, who held that the fundamental laws of nature are necessary and synthetic but hypothetical propositions which can be known by mere reflection on the characteristics involved in them. They are of the form 'If anything were Q it would necessarily be R'. The only function of experience in natural science is to assure us that there are in fact instances of Q. But, when Apelt faces the possibility of apparent exceptions to these synthetic and necessary laws of nature, his solution is indistinguishable from the doctrine that the laws are true by the definitions of their terms and are therefore analytic.

Under the head of *Some other Theories of Causation* Hr. von Wright gives a brief discussion of Whitehead's theory of Causal Perception, Meyerson's account of scientific explanation, and Bradley's and Bosanquet's theory of Concrete Universals, considered as contributions towards an *a priori* justification of inductive generalisations. His discussion may be summarised as follows.

Whitehead gives a much more satisfactory psychological account than Hume of the conditions under which we do in fact make anticipations and generalisations when confronted with concrete situations. But this provides no *a priori* guarantee that such generalisations and anticipations will hold without exception in the future.

Hr. von Wright interprets Meyerson's theory that scientific explanation consists in showing the 'identity' of cause and effect to mean that it consists in showing that the consequent of a causal proposition is logically entailed by its antecedent. He admits that this is very often true (it is in fact the element of truth in the Conventionalist Theory, which has yet to be discussed); but, where it holds, causal laws give no justification for making predictions. I should doubt whether the above interpretation of Meyerson's theory is altogether adequate. I think that he was more concerned with another sense in which cause and effect may be said to be 'only psychologically different', *viz.*, where there is quantitative identity in some important respect (*e.g.*, conservation of mass or of energy) together with qualitative dissimilarity. This kind of identity between cause and effect does not make causal laws analytical, though it certainly does nothing towards providing an *a priori* justification for inductive inference.

The interpretation which is put on Bosanquet's theory of Concrete Universals in relation to inductive inference is as follows. A concrete universal is identified with a Natural Kind, in Mill's sense of the word. The theory assumes (i) that all instances of a given Natural Kind K have the same causal properties, and (ii) that the presence or absence of a few easily recognisable qualities in an individual is a conclusive test of whether it does or does not belong to a certain Natural Kind. We may admit the importance of the facts which are expressed by the doctrine of Natural Kinds, but we are faced with the usual dilemma if an instance should be met which answers all the tests for belonging to the Natural Kind K and yet does not have the effects which members of that Kind have hitherto been found to have. (That this is not a purely imaginary case is well illustrated by the discovery of isotopes.) In presence of such cases we have either to give up the alleged causal law or to save it by making it an analytical proposition.

(2) *Causal Laws as Analytical Propositions*.—Hr. von Wright introduces the subject of the part played by conventions in science by discussing two examples, the melting-point of phosphorus and the impact of billiard balls.

We have become familiar with instances of a kind of matter which have had certain fairly easily recognisable characteristics, X, Y, and Z, severally common and collectively peculiar to them. We have given the name 'Phosphorus' to matter of this kind, without necessarily committing ourselves to *defining* 'Phosphorus' as matter which has the properties X, Y, and Z, or indeed in any other way. We find that all the instances of Phosphorus on which we have tried the experiment melt at 44° C., and we generalise this into the law 'Phosphorus melts at 44° C.'. Then we meet with a bit of matter which has the properties X, Y, and Z but does not melt at 44° C.

If our generalisation means 'Everything that has the qualities X, Y, and Z melts at 44° C.', it has been refuted. But we may admit the counter-instance and yet save the law 'Phosphorus melts at 44° C.' in at least two ways. (i) We may set about defining 'Phosphorus', and we may decide to make melting at 44° C. part of the definition. (ii) Another course, which we are more likely to follow, is to say that 'Phosphorus' is a substance of a certain molecular and atomic structure; and that melting at 44° C. is an infallible sign of this structure, whilst the conjunction of qualities X, Y, and Z, though in general a trustworthy indication, is not an infallible sign of it. In either case the generalisation has been saved by being made analytical. (I can well remember from the days when I did organic chemistry the following situation. We were told that so-and-so melts at n° C. We tried an alleged sample of so-and-so and found that it did not melt exactly at n° C. This was explained by saying that these samples were not 'chemically pure' so-and-so. And, finally, when one asked for a criterion for determining when a sample of so-and-so *is* chemically pure, one was told that the most reliable and convenient test was to see whether it melted at n° C.)

Consider now the law that a billiard ball starts to move when hit by another ball. So soon as we meet with counter-instances we find that all kinds of qualifying conditions, some positive and some negative, have to be inserted. The ball must not be stuck to the table, the moving body which strikes it must not be too light, and so on. Most of these conditions are generally fulfilled or are so obvious that we do not need to mention them. Now we are liable to say that, when all relevant circumstances have been explicitly introduced into the statement of a law, it will hold without exception. But this can be guaranteed only in one way, *viz.*, by adopting the convention that, when all the known relevant conditions are fulfilled and yet the consequent does not follow, we shall say that there must be some unknown relevant condition which is unfulfilled in the present case.

Generally the considerations which have been mentioned in the example of the melting-point of Phosphorus and those which have been mentioned in the example of impact are both involved together. *E.g.*, we talk of *the* melting-point of a substance; but we very soon find that the melting-point of any substance varies with the pressure, that Phosphorus exists in different allotropic forms with different melting-points under the same pressure, and so on. Take, *e.g.*, the 'law' that water boils at 100° C. under normal atmospheric pressure. This might be regarded as part of the definition of 'pure water', or again as the definition of '100° C.'.

There is, then, no doubt that generalisations which begin by being synthetic and contingent, very often end by becoming analytic and necessary. As the transition is gradual, one is very liable to combine in a muddled way the synthetic character of their earlier phases with the necessary character of their later phases, and so to think of them as being both synthetic and necessary throughout their history.

Some philosophers have thought that the fact that scientific laws tend to become analytical propositions disposes of the problem of justifying inductive inferences. Hr. von Wright has no difficulty in showing that they are mistaken. Suppose that the proposition 'Phosphorus melts at 44° C.' has become analytic. Associated with it is the synthetic proposition 'Anything that answers to all the tests for Phosphorus other than that of melting at 44° C. will also melt at 44° C.'. Undoubtedly we are strongly inclined to believe this proposition, and to act on our belief. But the mere fact that we should not call a substance 'Phosphorus' if it failed to melt at 44° C. is no ground for this belief. What causes and what seems to justify belief in the synthetic proposition is, not the analytic proposition, but the mass of empirical facts which have given rise to the convention which has made 'Phosphorus melts at 44° C.' analytic. So we are back, where we started, at the problem of justifying a synthetic universal generalisation on the basis of a 100 % statistical proposition about a limited class of observed instances.

(III) ATTEMPTS TO JUSTIFY INDUCTION *A POSTERIORI*.

(A) DEMONSTRATIVE.

An attempt to justify an inductive generalisation is *a posteriori* if it uses as a premiss the fact that such and such instances have been observed and have been found to have such and such characteristics. Such an argument may claim to be either demonstrative or only problematic. In the former case the observations, either alone or in conjunction with certain other premisses, are alleged to *entail* the generalisation. In the latter case they are alleged only to make the generalisation *highly probable*. At present we are concerned only with demonstrative arguments. Hr. von Wright distinguishes two kinds of attempt at demonstrative justification, *viz.*, the theory of induction as an Inverse Process of Deduction and the theory of induction as an Eliminative Process.

(1) *Induction as the Inverse of Deduction.*—This method is associated with Jevons and particularly with Whewell, but both Galileo and Leibniz had already made statements about induction which seem to imply this theory of it.

In essence the account which it gives of induction is that we first brood over a mass of observed data and try to conjecture a generalisation which will fit them all, and then we see whether the data can be deduced from the conjectured generalisation. If they can, we say that the generalisation has been 'verified'. The classical instance of this is the discovery and verification of the law that the planets move in ellipses about the sun as focus, which was the subject of so much controversy between Mill and Whewell.

The main points which Hr. von Wright makes are these. (i) Opponents of Whewell were inclined to confine their attention to very simple cases where there is no difficulty in guessing a suitable generalisation and no doubt that the generalisation proposed fits all the data. They overlooked the fact that in advanced sciences it may be very difficult to think of any simple generalisation that covers the data, and that when one has done so elaborate deduction may be needed to show that it fits them. (ii) On the other hand, Whewell's theory gives no justification for believing that the generalisation which fits all the data extends beyond them, *e.g.*, that the unobserved intermediate positions of the planet fell on the curve which fits the observed positions or that its future positions will fall on the same curve as its past ones.

In fact all that Whewell's method will prove is that a certain generalisation is *one* of those which is *consistent with* all the *observed instances.* In order to justify inductive inference demonstratively we should have to prove that there is one and only one generalisation consistent with the data and that this will apply also to instances not included among the data.

(2) *Induction as an Eliminative Process.*—Hr. von Wright treats eliminative induction in terms of the notions of Sufficient and of Necessary Conditions. I am quite sure that he is correct in this, and I welcome his elaborate discussion all the more because I adopted the same line of approach in the first of my two articles on *The Principles of Demonstrative Induction* in Vol. XXXIX of MIND. Hr. von Wright's treatment has one great advantage over mine. By recognising the possibility of *disjunctive* necessary conditions, beside that of *conjunctive* sufficient conditions, he introduces a symmetry and completeness

which were lacking in my treatment. I propose now to give, in my own way and in my own notation, an account of necessary and sufficient conditions based on what Hr. von Wright has put forward in the Thesis and with greater elaboration in the Article.

I will first make some remarks about notation. I shall use small letters, such as p, q, and r, to denote characteristics which are *simple*, *i.e.*, involve neither negation, conjunction, nor disjunction. I shall use large letters, such as P, Q, and R, to denote characteristics which *may* be simple but may also be synthesised out of simple characteristics by single or repeated or combined applications of negation, conjunction, and disjunction. Thus, *e.g.*, P would cover such cases as p, $\bar{p}$, $p \vee q$, $p \,\&\, \bar{q} \,.\vee.\, r$, and so on. I shall denote the proposition 'x is P' by $P(x)$, the proposition 'x is P and Q' by $P \,\&\, Q(x)$, and the proposition 'x is P or Q' by $P \vee Q(x)$. I shall use Q to denote a characteristic of which we are seeking either the sufficient or the necessary conditions. I shall denote its possible sufficient conditions by P_1, P_2, etc., and its possible necessary conditions by R_1, R_2, etc.

The statement that P is a sufficient condition of Q means that every instance of P is an instance of Q. It may be symbolised by $P\sigma Q$; so we have

$$P\,\sigma\,Q = P(x) \supset_x Q(x) \ \mathrm{D}f.$$

The statement that R is a necessary condition of Q means that every instance of Q is an instance of R. It may be symbolised by $R\,\nu\,Q$; so we have

$$R\,\nu\,Q = Q(x) \supset_x R(x) \ \mathrm{D}f.$$

The following propositions follow immediately from these definitions.

(i) $R\,\nu\,Q \equiv Q\,\sigma\,R$. (ii) $P\,\sigma\,Q \equiv \bar{Q}\,\sigma\,\bar{P}$. (iii) $R\,\nu\,Q \equiv \bar{Q}\,\nu\,\bar{R}$. (iv) A sufficient condition of a sufficient condition of Q is a sufficient condition of Q. (v) A necessary condition of a necessary condition of Q is a necessary condition of Q. (vi) If P is a sufficient condition of Q it is a sufficient condition of every necessary condition of Q. (vii) If R is a necessary condition of Q every sufficient condition of Q is a sufficient condition of R.

The next point to notice is a certain lack of symmetry between necessary and sufficient conditions. (i) It is plain from the definition of $R\,\nu\,Q$ that, if Q has any necessary conditions, they must *all* be present in *every* instance of Q. If we contrapose this, we get the equivalent negative proposition 'No characteristic which is absent from *any* instance of Q can be a *necessary* condition of Q'. An immediate consequence of this is that, if Q has several necessary conditions, they must all be logically and causally compatible with each other. (ii) On the other hand, it is plain from the definition of PσQ that, even if Q has *some* sufficient condition in every instance in which it occurs, there is no need for all its sufficient conditions to be present in even a single instance of it. In some instances the sufficient condition P_1 might be present, in others this might be absent and another sufficient condition P_2 might be present, and so on. It is plain, then, that Q might have a number of sufficient conditions which are logically or causally incompatible with each other. (iii) What corresponds, in the case of sufficient conditions, to proposition (i) about necessary conditions is the following. If Q has any sufficient conditions, they must *all* be *absent* from *any* instance from which Q is *absent*. If we contrapose this, we get the equivalent negative proposition 'No characteristic which is present in any instance from which Q is absent can be a sufficient condition of Q'. It is on these two principles that eliminative induction rests.

We pass now to the distinction between simple and composite conditions. It follows at once from our definitions that *disjunctively* composite *sufficient* conditions and *conjunctively* composite *necessary* conditions are of no particular interest. For it is immediately obvious that

$$(p_1 \vee p_2)\sigma Q \,.\, \equiv : p_1\sigma Q \,.\, \& \,.\, p_2\sigma Q$$

and

$$(r_1 \,\&\, r_2)\, \nu\, Q \,.\, \equiv : r_1\, \nu\, Q \,.\, \& \,.\, r_2\, \nu\, Q.$$

On the other hand, *conjunctively* composite *sufficient* conditions and *disjunctively* composite *necessary* conditions are interesting and important. It may be that p_1 is not a sufficient condition of Q and that p_2 is not a sufficient condition of Q, but that the conjunction $p_1 \,\&\, p_2$ is a sufficient condition of Q; *i.e.*, there may be instances of p_1 which are not Q and instances of p_2 which are not Q, but no instances of $p_1 \,\&\, p_2$ which are not Q. Again, it may be that r_1 is not a necessary condition of Q and that r_2 is not a necessary condition of Q, but that the disjunction $r_1 \vee r_2$ is a necessary condition of Q; *i.e.*, there may be instances of Q which are not r_1 and instances of Q which are not r_2, but no instances of Q which are not either r_1 or r_2.

This leads to the notion of what I called a 'Smallest Sufficient Condition' in my article on *Demonstrative Induction*. I see now that this must be supplemented by the parallel notion of what I will call a 'Smallest Necessary Condition'. The definitions of these notions are as follows. P is a smallest sufficient condition of Q if it is a sufficient condition of Q, and either (i) it is a simple characteristic p, or (ii) it is a conjunctive characteristic $p_1 \& p_2 \& \ldots p_n$ such that if any of the conjuncts be omitted what remains is not a sufficient condition of Q.

R is a smallest necessary condition of Q if it is a necessary condition of Q, and either (i) it is a simple characteristic r, or (ii) it is a disjunctive characteristic $r_1 \vee r_2 \vee \ldots r_n$ such that if any of the alternants be omitted what remains is not a necessary condition of Q.

Even if Q has a sufficient condition in every instance in which it occurs, it is possible that none of its sufficient conditions is simple. And, if Q has necessary conditions, it is possible that none of them are simple. I propose to call any simple characteristic or conjunction of such characteristics which is part of any smallest sufficient condition of Q a *Contributory Condition* of Q. Thus every smallest sufficient condition of Q is either simple or is a conjunction of a number of simple contributory conditions. I propose to call any simple characteristic or disjunction of such characteristics which is part of any smallest necessary condition of Q a *Substitutable Requirement* of Q. Thus every smallest necessary condition of Q is either simple or is a disjunction of a number of simple substitutable requirements.

It is evident from the definition of a sufficient condition that, if P is a sufficient condition of Q, then the conjunction of P with *any* other characteristic is also a sufficient condition of Q. Similarly, if R is a necessary condition of Q, the disjunction of R with *any* other characteristic is also a necessary condition of Q. The notions of smallest sufficient and smallest necessary conditions are important in cutting out the trivialities which would otherwise arise from these facts.

It would be possible to arrange simple contributory conditions in a kind of hierarchy of what I will call 'Dispensability' in the following way. (i) Suppose that Q has one and only one smallest sufficient condition. Then we can say that all its simple contributory conditions are 'equally indispensable'. (ii) Suppose that Q has several smallest sufficient conditions. It may be that some of its simple contributory conditions are present in all of these, that some are present in all but one of them, that some are present in all but two of them, and so on. Then we could say that those of the first kind are 'indispensable', that those of the second kind have 'dispensability of the first degree', that those of the third kind have 'dispensability of the second degree', and so on.

Lastly, it is worth remarking that several of Q's smallest sufficient conditions might be present together in the same instance of Q. In that case I should say that Q was 'over-determined'. *E.g.*, a person may believe that a certain decision would be right and also that it would give pleasure to himself. Either of these beliefs, in conjunction with his conative and emotional dispositions, might suffice, in the absence of the other, to determine this decision. If so, the decision is over-determined.

We come now to a very important point which Hr. von Wright makes, and which I had also made in my article on *Demonstrative Induction*. There is nothing in the definition of a sufficient condition, to guarantee either (i) that every characteristic has a sufficient condition, or (ii) that, even if Q has one or more smallest sufficient conditions, there may not be instances in which Q occurs without any of these sufficient conditions being present. Similarly, there is nothing in the definition of a necessary condition to guarantee that every characteristic has a necessary condition. An immediate consequence of this is that it is logically possible that P should be an *indispensable contributory* condition of Q without being a *necessary* condition of Q. For to say that it is an indispensable contributory condition of Q is to say that it is a conjunct in all the smallest sufficient conditions of Q, whilst to say that it is a necessary condition of Q is to say that it is present in every instance of Q. Now, if there can be instances of Q in which none of its smallest sufficient conditions are present, it is plain that there may be instances of Q in which none of its indispensable contributory conditions are present.

The minimum assumption that will avoid these consequences is the following. Let us assume that, whatever characteristic Q may be, every instance in which it occurs is characterised by some sufficient condition of it. This is what I called the *Postulate of Smallest Sufficient Conditions* and it is one form in which the Law of Causation might be stated. It follows at once from this postulate that the disjunction of all Q's smallest sufficient conditions is a necessary condition of Q. And from this it follows immediately that any indispensable contributory condition which Q may have is a necessary condition of Q.

It is easy to show, as Hr. von Wright does, that it follows from the same postulate that the conjunction of all Q's necessary conditions is a sufficient condition of Q. For, as we have seen, it follows that *one* of Q's necessary conditions is the disjunction of all its smallest sufficient conditions. But this disjunction is also a sufficient condition of Q. Therefore the conjunction of it with anything else (and therefore with the rest of Q's necessary conditions) is a sufficient condition of Q.

I very much doubt whether this proposition is what people have in mind when it seems self-evident to them that a conjunction of all Q's necessary conditions must be a sufficient condition of Q. I suspect that when people talk of 'necessary conditions' they are often thinking of contributory conditions. Every contributory condition of Q is necessary, not indeed to Q itself, but to at least one of Q's smallest sufficient conditions. And it is an analytical proposition that a conjunction of all Q's contributory conditions would constitute a sufficient condition of Q; for between them they would constitute *all* Q's smallest sufficient conditions, and would thus in general over-determine Q.

This is as much as I need say about the formal logic of sufficient and of necessary conditions. It remains to consider the application of it to eliminative induction.

As Hr. von Wright points out, it is plain that there are two and only two fundamental 'Methods' of eliminative induction. One is concerned with eliminating possible sufficient conditions, and cannot be used directly for dealing with possible necessary conditions; the other is concerned with eliminating possible necessary conditions, and cannot be used directly to deal with possible sufficient conditions.

Suppose we want to find the *necessary* conditions of Q. We rely on the principle that no characteristic which, absent in any instance of Q, can be a necessary condition of Q. We therefore take a number of instances of Q which agree in as few respects as possible except the presence of Q. We find what is common to all of them other than Q itself. Then this common part contains all the necessary conditions of Q. It may, of course, contain characteristics which are not necessary conditions of Q; and further and more variegated instances of Q might enable us to eliminate some of these. It is evident that this is in essence the *Method of Agreement*.

Hr. von Wright does not tell us in detail how to perform the process of finding all the possible necessary conditions which are consistent with a given set of instances of Q, so I will take an example to illustrate the Method of Agreement. The rule may be stated as follows. 'Take the disjunction of all the conjunctions of characteristics other than Q, in each of the instances. Express this as a conjunction of terms in which each conjunct is either (*a*) simple, or (*b*) a disjunction of simple terms. Then each conjunct is a possible necessary condition of Q.' Now for an example.

Suppose that we have the three instances $Q \& r_1 \& r_2 \& r_3$ (a), $Q \& r_1 \& r_2 \& \bar{r}_3$ (b), and $Q \& r_1 \& \bar{r}_2 \& \bar{r}_3$ (c) to begin with. We take the disjunction

$$r_1 \& r_2 \& r_3 \,.\, \mathsf{v} \,.\, r_1 \& r_2 \& \bar{r}_3 \,.\, \mathsf{v} \,.\, r_1 \& \bar{r}_2 \& \bar{r}_3.$$

It is very easy to show that this boils down to $r_1 \& \,.\, r_2 \mathsf{v} \bar{r}_3$. So at this stage the possible necessary conditions of Q are r_1 and $r_2 \mathsf{v} \bar{r}_3$, *i.e.*, one simple condition and one composite disjunctive condition. Suppose now that a further instance $Q \& \bar{r}_1 \& \bar{r}_2 \& \bar{r}_3$ (d) is observed. We must now take the disjunction of $\bar{r}_1 \& \bar{r}_2 \& \bar{r}_3$ with what was left standing by the first three instances, *viz.*, $r_1 \& \,.\, r_2 \mathsf{v} \bar{r}_3$. This works out to $r_1 \mathsf{v} \bar{r}_2 \,.\, \& \,.\, r_1 \mathsf{v} \bar{r}_3 \,.\, \& \,.\, r_2 \mathsf{v} \bar{r}_3$. It is interesting to note that the additional instance has not reduced the *number* of possible necessary conditions of Q. It has in fact increased it from two to three. But it has reduced the *strength* of the conditions. For the first three instances left open the possibility that the simple characteristic r_1 might be a necessary condition of Q. The addition of the fourth instance has eliminated this possibility and shown that Q has no simple necessary condition, but at most disjunctive ones.

Suppose next that we want to find the *sufficient* conditions of Q. We rely on the principle that no characteristic which is present in any instance from which Q is absent can be a sufficient condition of Q. We also use the postulate that in every instance of Q there is a sufficient condition of it. In this case we take (i) an instance in which Q is present, *e.g.*, $p_1 \& p_2 \& p_3 \& Q$ (a). (ii) A number of other instances which between them resemble the first as much as possible except in the fact that Q is absent in all of them. *E.g.*, they might be $p_1 \& p_2 \& \bar{p}_3 \& \overline{Q}$ (b) and $\bar{p}_1 \& p_2 \& p_3 \& \overline{Q}$ (c). The argument would run as follows. In virtue of our postulate we know that the first instance must contain a smallest sufficient condition of Q. But this might be either $p_1 \& p_2 \& p_3$ or $p_1 \& p_2$ or $p_2 \& p_3$ or $p_3 \& p_1$ or p_1 or p_2 or p_3. The first counter-instance eliminates the possibility that it is $p_1 \& p_2$ (and therefore also the possibilities that it is p_1 and that it is p_2). The second counter-instance eliminates the possibility that it is $p_2 \& p_3$ (and therefore also the possibilities that it is p_2 and that it is p_3). So at this stage the possibilities which remain are that the sufficient condition of Q in the first instance was either $p_1 \& p_3$ or $p_1 \& p_2 \& p_3$. Suppose now that another counter-instance $p_1 \& \bar{p}_2 \& p_3 \& \overline{Q}$ (d) were found. This would eliminate the possibility that p_1 & p_3 is a sufficient condition of Q. We should be left with the conclusion that nothing less than $p_1 \& p_2 \& p_3$ was sufficient to produce Q in the first of our instances, and therefore that p_1, p_2, and p_3 were all indispensable contributory conditions.

It is evident that the reasoning just described is in essence the *Method of Difference.* The following important difference between the ranges of the two Methods should be noted. Since *all* the necessary conditions of Q must be present in *every* instance of Q, a single instance gives us the field within which all possible necessary conditions of Q are contained. The further instances used by the Method of Difference simply serve to reduce this field. But we have no guarantee that all the smallest sufficient conditions of Q are present in any one instance of it. So the positive instance in the Method of Difference gives us only the field within which the smallest sufficient condition of Q *in that instance* must lie. The counter-instances may between them reduce that field to a single possibility, as they did in the example given above ; but even so they leave open the possibility that Q may have many other smallest sufficient conditions in other instances of its occurrence.

Hr. von Wright points out the following important limitation of eliminative methods, whether applied to finding sufficient or necessary conditions. Either the process of elimination leaves several alternative possible necessary or sufficient conditions, as the case may be, still standing ; or, if not, the one alternative left is, in the case of necessary conditions, the disjunction of *all* the simple characteristics in the various instances of Q, and, in the case of sufficient conditions, the conjunction of *all* the simple characteristics in the single instance of Q. Thus one always knows beforehand what the end of the process of elimination must be if it is to succeed in eliminating all alternatives but one. The reason for this is plain. Suppose, *e.g.*, that the positive instance in the Method of Difference is $p_1 \& p_2 \& p_3 \& Q\ (a)$. Then evidently the whole conjunction $p_1 \& p_2 \& p_3$ is *one* candidate for the office of smallest sufficient condition of Q. Now all that the Method of Difference can do is to eliminate the claims of selections from it, such as $p_1 \& p_2$ or p_3, to be sufficient conditions. Therefore so long as any other candidate remains standing the most complex conjunction remains as a possible candidate. The argument is precisely similar in the case of necessary conditions and the Method of Agreement, with ' disjunction ' substituted for ' conjunction '.

We have seen that it follows from the definition of ' P is a sufficient condition of Q ' that this is equivalent to ' $\overline{P}$ is a necessary condition of $\overline{Q}$ '. This fact enables us to use the

Method of *Agreement* indirectly for finding the *sufficient* conditions of Q. For this purpose we should have to take a number of instances which agree in the *absence of Q* but in other respects differ among themselves as much as possible. Any characteristic other than $\bar{Q}$ which is common to all of them is a possible *necessary* condition of $\bar{Q}$. Therefore the *negation* of any such characteristic is a possible *sufficient* condition of Q.

The following point is worth noting about this indirect application of the Method of Agreement. *All* the necessary conditions of $\bar{Q}$ must be present in any instance of $\bar{Q}$. Therefore the negations of these will include *all* the sufficient conditions of Q. Thus this method gives us a field which includes all the sufficient conditions of Q, whilst the direct application of the Method of Difference for finding sufficient conditions is concerned only with the smallest sufficient condition of Q in the particular instance of Q under investigation.

In a similar way the Method of *Difference* may be used indirectly for finding the smallest necessary conditions of Q. This depends on the fact that, if Q is a necessary condition of R, then $\bar{R}$ is a sufficient condition of $\bar{Q}$, and conversely. For this purpose we should have to take (i) an instance in which Q was *absent,* and (ii) a number of instances which between them resemble the first as much as possible except that Q is *present* in all of them. Let us suppose that the Postulate of Smallest Sufficient Conditions applies to negative characteristics, like $\bar{Q}$, as well as to positive characteristics like Q. Then the conjunction of characteristics other than $\bar{Q}$ in the first instance must either be or contain a smallest sufficient condition of $\bar{Q}$. Any factor or combination of factors common to this and to one or more of the instances in which Q is present can be rejected from the class of possible sufficient conditions of $\bar{Q}$. What remain are possible sufficient conditions of $\bar{Q}$. Therefore the negations of these are possible necessary conditions of Q.

This completes the account of the formal logic of methods of elimination.

The question that remains is this. Is it possible to infer with certainty from data such as we have been considering, by means of Methods of Elimination, general propositions about the necessary or the sufficient conditions of a given characteristic Q ?

Hr. von Wright points out that this question may be divided into three, which are often not clearly distinguished, but which form a hierarchy. They may be formulated as follows. (i) Is it ever possible to show that, *if* Q has necessary or has sufficient conditions, then the only hypothesis about these conditions which is consistent with our empirical data is so-and-so ? (ii) Is it ever possible to show that, *if* certain general propositions about nature be granted in addition to the empirical data, then it would follow from the principles and the data together that the necessary or the sufficient conditions of Q are so-and-so ? (iii) If so, can we know that these principles are true and therefore infer with certainty that the necessary or the sufficient conditions of Q *are* so-and-so ?

The answer to the first question is Yes. But our enthusiasm over this answer is damped when we remember that we can be certain beforehand that, if the Method of Elimination does leave only one hypothesis standing, this will necessarily be the most complex one that is consistent with the data.

The answer to the second question is as follows. (*a*) It is quite evident that some general principle about nature must be added to the data if the Eliminative Method is to lead to any positive categorical conclusion. For the Method applied to the data alone leads directly only to negative results, *viz*., that such and such hypotheses about the conditions of Q must be rejected as inconsistent with the data. Even if in this way we can eliminate all the alternative hypotheses but one, we have no right to accept the one survivor unless we are granted the premiss that Q has necessary conditions and that it has sufficient conditions not only in this instance but in every instance. As we have seen, the latter premiss carries the former with it. So what I have called the 'Postulate of Smallest Sufficient Conditions' and what Hr. von Wright calls the 'Deterministic Assumption' is certainly needed ; but is it enough ?

(*b*) It is certain that something else is needed too. In our examples of the Methods we have made it appear as if the instances under consideration have, and are known to have, only a small number of characteristics, all of which have been distinguished, recognised, and labelled with p's or r's. When we remember that the characteristics may include, beside pure qualities, relational properties both non-dispositional and dispositional, it is plain that this appearance is misleading. So, although it might be possible for an angel with a microscopic and a telescopic eye to fulfil the conditions for answering question (ii) in the affirmative without any other postulate but the Deterministic Assumption, it is certain that beings like ourselves are not in a position to do so unless some further postulate is granted. It is plain that the postulate needed is one that will place some limitation on the number of characteristics which need to be considered in reference to the question: 'What are the necessary or the sufficient conditions of Q?'

Hr. von Wright points out that it would not be enough to know that the number of independent characteristics is finite, *i.e.*, that there is some number or other which it does not exceed. The smallest assumption on these lines which would be of any use is that the number of independent characteristics does not exceed a certain assigned number, *e.g.*, 1000. An alternative postulate would be that certain classes or characteristics can be ruled out as irrelevant to a given characteristic Q, and that we can have exhaustive knowledge of all the other characteristics of each instance of Q under consideration. In general what is wanted is some principle in virtue of which it is possible to *know* when we have exhaustive knowledge of all the characteristics of our instances which are relevant to Q. Hr. von Wright calls such a postulate, no matter what particular form it may take, the postulate of 'Completely Known Instances'.

So the third question depends for its answer on the answer to the question whether we know or have rational grounds for believing the Deterministic Assumption and some form of the postulate of Completely Known Instances.

Now there are two alternatives to be considered, *viz.*, (i) that these postulates are *a priori* propositions, or (ii) that they are themselves empirical generalisations. I am not sure that I understand Hr. von Wright's argument about the consequences of supposing that the postulates are *a priori* propositions. I propose therefore to substitute for it the following argument, which appears to lead to much the same conclusions and may really be the same as his.

Suppose that A, B, and C are any three propositions such that A & B entails C. (*E.g.*, A might be 'All men are mortal', B might be 'Socrates is a man', and C might be 'Socrates is mortal'). Then it follows that A entails $\bar{B} \vee C$. (*E.g.*, it follows that 'All men are mortal' entails 'Either Socrates is not a man or Socrates is mortal'.) Of course it equally follows that B entails $\bar{A} \vee C$; but we need not consider both these consequences for our present purpose. Now suppose that A is a *necessary* proposition. Then (i) it is true, and therefore anything which it entails is true. And (ii) anything which it entails is necessary. Therefore if A & B entails C and A is necessary, it follows that $\bar{B} \vee C$ is necessary, or, what is equivalent, that the conjunction B & $\bar{C}$ is impossible. Now this is equivalent to 'Either B is impossible or C is necessary or B entails C'.

Let us now apply this bit of formal logic to the supposition that the Inductive Postulates are *a priori*, *i.e.*, necessary propositions. It is alleged that the postulates A in conjunction with the instantial propositions B entail the inductive generalisation C. Suppose that the postulates are necessary propositions. Then it follows that either the instantial propositions are impossible or the inductive generalisations is necessary or that the instantial propositions entail the inductive generalisation. Now all these three alternatives are palpably absurd. Therefore we must reject one at least of the premisses which led to them. Now one premiss was that the postulates are *a priori*, and the other is that the postulates together with the instantial propositions entail the inductive generalisation. So there is no hope in this direction of justifying inductive generalisations as deductive inferences from instantial propositions and general postulates about nature.

It is even more obvious that the other alternative, *viz.*, that the postulates are themselves inductive generalisations, leads to nothing but a vicious circle or a vicious infinite regress. So we may conclude that no justification of inductive generalisation along these lines is possible.

(*To be continued.*)