VII.—CRITICAL NOTICE

Probability and Induction. By WM. KNEALE. Oxford: Clarendon Press. Pp. viii + 264.

THIS very able and interesting book is based on the lectures given in Oxford by Mr. Kneale up to the outbreak of the second world war, and has been prepared by him for the press in the scanty leisure which he has enjoyed since that war changed from 'hot' to 'cold'. It forms an admirable general introduction to the philosophy of the two inter-connected subjects named in its title, but what makes it particularly exciting is certain special doctrines on fundamental questions which Mr. Kneale asserts and defends. These are in conflict with certain philosophical principles or prejudices which are at the moment fashionable and almost orthodox among Mr. Kneale's contemporaries and juniors in this country and the United States. These parts of the book are likely to lead to much valuable discussion. It is a very happy circumstance that doctrines which are at the moment unfashionable should be put forward by a man like Mr. Kneale, who is fully aware of the strength and the weaknesses of the current orthodoxy, and whom no-one in his senses can afford to dismiss as a negligible 'back-number'.

The doctrines to which I refer are the following. Mr. Kneale distinguishes between matters of fact and what he calls 'Principles of Modality'. He rejects the view that all statements which ostensibly record principles of modality are really statements about language couched in a misleading form. He holds that, if there are laws of nature, they are all principles of necessity, although none of them can be known *a priori*. Lastly, he holds that what he calls 'Probability Rules', *i.e.*, propositions of the form 'The probability of an instance of α being an instance of β is p', are also principles of modality, which cannot be known *a priori* but can be reasonably conjectured inductively on the basis of statistics. According to Mr. Kneale, the laws of logic, of phenomenology, and of nature (which are all fundamentally of the same kind), leave open a certain range of possibility for anything which is an instance of α, and they leave open a certain narrower range of possibility for anything which is an instance of $\alpha\beta$. What a probability rule asserts is that the latter range bears a certain proportion to the former.

I shall begin by giving a rough general sketch of the contents of the book as a whole, and shall then try to expound in greater detail (so far as I understand them) these characteristic doctrines of Mr. Kneale's and his reasons for them and against alternatives to them.

(I) GENERAL OUTLINE OF THE CONTENTS. The book is divided into four Parts. The first, which is introductory, treats of *Knowledge* *and Belief*. The second, entitled *The Traditional Problem of Induction*, is concerned with all the main philosophical problems of induction in so far as that process is used to establish *laws*, as distinct from probability-rules. The third, entitled *The Theory of Chances*, discusses the fundamental notions and theorems of the calculus of probability, and considers whether these are relevant to the logic of induction. The answer to the latter question is negative; and the fourth Part, entitled *The Probability of Inductive Science*, deals with the question *whether*, and, if so, *in what sense*, recognized inductive procedures give more or less 'probability' to statements of law and to probability-rules.

(1) *Knowledge and Belief*. Mr. Kneale's conclusions may be summarized as follows. He starts with the antithesis between 'knowing *p*' and 'believing *p*'. He holds, in opposition to some distinguished epistemologists, that 'knowing' is used in an occurrent sense, and not *only* in a dispositional sense. (*Cf.*, *e.g.*, "When it began to pour with rain while I was out walking this afternoon I *knew* that I should get wet through".) He has not met with any satisfactory analysis of 'knowing *p*' in the occurrent sense, and so he takes it provisionally as unanalyzable. It is equivalent to 'noticing that *p*' or 'realizing that *p*'.

The phrase 'believing *p*' covers two different cases, which may be described as 'taking *p* for granted' and 'opining *p*'. The former consists in acting as if one knew *p* when one does not know it. To say that *A* opines *p* with a certain degree of rational confidence means that (i) *A knows* certain other propositions *q*, which in fact probabilify *p* to the degree in question, and (ii) *A knows* that *q* probabilifies *p* to that degree. Opining may be irrational. This covers two cases. *A* may take for granted (instead of knowing) some or all of the evidence for *p*; or he may take for granted (instead of knowing) that the evidence probabilifies *p* to the degree in question. Neither failure in rationality necessarily leads to false belief.

Probabilification of one proposition to a certain degree by certain other propositions is a purely objective fact. It has certain analogies to the necessitation of one proposition by another, and certain unlikenesses to it.

We talk of the probability of throwing a six with a fair die, and we also say that induction establishes certain laws and certain probability-rules with high probability. Mr. Kneale holds that the word probability is used in different senses in these two applications. But it is not just a single word with several totally disconnected meanings, like the word 'plot', *e.g.* There are real and important analogies between its various applications. A most important common feature is that it is reasonable to take as a basis for action any proposition which is highly probable, in the appropriate sense, on the evidence available to one. Any satisfactory analysis of 'probability' must enable us to see why this is so.

(2) *The Traditional Problem of Induction.* Taking induction for the present as a process by which universal propositions are established, Mr. Kneale points out that the word has been used to cover four different processes, each of which leads to a different kind of universal proposition. These processes may be described as *Summary, Intuitive, Mathematical,* and *Ampliative* induction.

Summary Induction establishes propositions of the form All S is P, where the description 'S' is such that, from the nature of the case, it can apply only to a finite number of instances, and where it is in principle possible to know that one has exhausted the whole set. An example would be: All the chairs in this room on Christmas Day 1946 had red seats. Mr. Kneale points out that such a statement is equivalent to: No part of this room during the period in question was occupied by a chair with a seat which was not red. This is different in kind from such a proposition as: All men are mortal. Summary Induction is a deductive argument, though it cannot be reduced to a syllogism in the technical sense. One premiss has to be what might be called an 'exhaustive' proposition; *e.g.*, This, that, and the other sub-region together make up the space in this room.

The result of Intuitive Induction is knowledge of what Mr. Kneale calls 'Principles of Modality', *i.e.*, of compatibility or incompatibility between characteristics. These are essentially universal and necessary. An example would be: No surface could be red and green all over at the same time, but a surface could be at once red and hot all over. It is a characteristic doctrine of Mr. Kneale's that such propositions are *not* merely linguistic. His arguments on this point will be considered later. We may sum up Mr. Kneale's account of intuitive induction by saying that he considers it to be a valid intellectual process, but not a form of reasoning. What experience does here is to provide *instances*, not premisses.

Mathematical Induction, or argument by recurrence, establishes propositions of the form: All numbers have the property p. Such propositions are necessary, but they differ in kind from principles of modality which are established by intuitive induction. After considering various alternative views as to the nature of propositions about all numbers, Mr. Kneale puts forward the following account of them. To say, *e.g.*, that 2 is a number, is to say that '2' is a *recurrence symbol*, *i.e.*, that it signifies, not an individual nor a character of an individual or a group, but a certain feature, *viz.*, a recurrence in the structure of such facts as are expressed by sentences like 'There are 2 tables in this room'. To say that all numbers have the property p is equivalent to saying: '1 has the property p, and, if c has it, then $c + 1$ has it'. Thus, such propositions depend on the fact that the whole nature of numbers is to form a sequence generated by arithmetical addition.

Mr. Kneale argues that all proofs of universal propositions about numbers involve mathematical induction. For propositions about other kinds of number are reducible to complicated statements about integers, and all proofs of universal propositions about integers depend on mathematical induction. Proofs which seem *prima facie* to be independent of this process involve the principles of algebra, *e.g.*, the associative law, and these can be proved only by recurrence.

Ampliative Induction is concerned to establish natural laws and probability-rules. For the present we will confine our attention to the former. A law of nature is a proposition of the form: All S is P, where the description 'S' applies to a potentially unlimited class of individual existents.

Mr. Kneale distinguishes the following four types of law. (i) *Uniform associations of attributes*. These are the laws which are involved in the existence of those groups of associated properties which mark out natural kinds. (ii) *Uniformities of development in natural processes*. Examples are found in the course of development of an embyro, of a chemical reaction, and so on. The Second Law of Thermodynamics is an advanced instance. (iii) *Laws of functional relationship*. An example would be the gas-law PV = RT. Such laws require that there shall be a uniform relationship between values of the several variables, and that this shall be expressible in a formula of pure mathematics. (iv) *Numerical natural constants*. An example would be the law that gold melts at such and such a temperature. (It will be noted that each of the last three kinds of law involves a reference to natural kinds, *e.g.*, embryos of *mammals*, instances of *gases*, bits of *gold*.)

Mr. Kneale gives an interesting historical account of the senses in which the word 'cause' was used by Aristotle, by Bacon, and by Hume and his continuator Mill. In this connexion he gives a critical account of the eliminative methods proposed by Bacon and by Mill for discovering 'the cause' or 'the effect' of a given phenomenon. His general conclusion is that philosophers have tended to exaggerate the importance of the notion of cause in science. It is a vague notion, useful enough in some departments of practical life, but incapable of being made unambiguous and precise. When one tries, as Hume and Mill did, to tie it down to the notion of 'antecedent cause', it develops ambiguities and difficulties; and to describe science as a search for causes and causal laws, in this sense, is to give an inadequate and misleading account of the procedure of the more advanced sciences.

The most important section of this Part is concerned with the logical or ontological nature of laws. I shall expound Mr. Kneale's views and his reasons more fully later. For the present it will suffice to say that he considers and rejects the following views about natural laws, *viz.*, (i) that they are analogous to the restricted universals established by summary induction, (ii) that they are *facts* (as opposed to *principles*) of unrestricted generality, *i.e.*, the *de facto* regularity analysis, and (iii) that they are merely regulative prescriptions. Every alternative has its difficulties, but in the end Mr. Kneale accepts and defends the view that laws are *principles of modality*, *i.e.*, are of the same nature as the propositions which are established by intuitive induction, although, for reasons which he gives, no law can be established in that way. This alternative, he says, is at any rate ' not entirely hopeless ', whilst all the others are so. It is ' the only account of laws which makes sense '.

There are hosts of alleged laws for which there is good inductive evidence, and serious science begins when we try to correlate and explain them. Such explanation may take two forms. (i) We may try to show that a large number of these laws follow logically from one or more others which have themselves been established by direct induction. (ii) We may try to show that they are entailed by one or more propositions which have not been, and from the nature of the case *could never be*, established by direct induction. Mr. Kneale calls the latter ' explanation by means of *Transcendent Hypothesis* '. An example of a transcendent hypothesis is the atomic theory or the wave-theory of light.

The peculiarity of a transcendent hypothesis is that the things and processes in terms of which it is formulated *could not* conceivably be perceived by the senses, and therefore, strictly speaking, could not be imagined either. It is plain that such hypotheses raise certain philosophical questions. Mr. Kneale's main answers are as follows :—

(i) Although we cannot imagine a transcendent entity, we conceive it as having a certain definite logical or mathematical *structure* embodied in a content which we cannot even conjecture. (ii) Any statement about a perceptual object, *e.g.*, a table, can be translated into statements about transcendent objects, *e.g.*, electrons and protons ; but there are many significant statements about transcendent objects, *e.g.*, about what happens inside an atom, which cannot be translated into statements about perceptual objects. (iii) Some of these non-translatable statements about transcendent objects are essential if the hypothesis is to explain known laws about perceptual objects and to suggest others which may be tested experimentally. (iv) For the above reasons Mr. Kneale rejects the view that statements about transcendent objects are merely a new and mathematically more handy terminology for talking about perceptual objects and their laws. He thinks that it would be unintelligible, on that view, that a transcendent hypothesis should enable one to infer laws about perceptual objects which had not as yet been established by direct induction. I do not find this argument very convincing. I suppose, *e.g.*, that the difference between the heliocentric and the geocentric descriptions of the planetary motions is merely a difference in the frame of reference adopted. Yet it is almost inconceivable that Kepler's laws of planetary motion would have been discovered unless the heliocentric description had been substituted for the geocentric ; and, unless they had been, it is almost inconceivable that the law of gravitation would have been discovered.

Mr. Kneale uses the term 'secondary induction' for the kind of reasoning by which a transcendent hypothesis, as distinct from an ordinary law about perceptual objects, is experimentally verified or refuted.

Suppose that a hypothesis H entails a number of laws, $L_1, L_2, \ldots$, for each of which there is direct inductive evidence, $e_1, e_2, \ldots$, respectively. Then each of these laws is supported indirectly by the direct evidence for all the others. For e_r, in supporting L_r, supports the hypothesis H, which entails L_r. And, in supporting H, it indirectly supports any other law, L_s, which is entailed by H. This is called by Mr. Kneale 'consilience of primary inductions'. It plays an important part in every advanced science.

The last topic dealt with in this Part is the relative importance of confirmation and elimination in induction. Mr. Kneale points out that elimination can lead to no positive conclusion unless it can be combined with *some* affirmative universal premiss. Now, even if some general principle of determinism could be formulated and were found to be self-evident, it would be far too abstract to serve as a useful premiss in an eliminative argument. In fact when scientists use such arguments they employ fairly concrete positive premisses, such as, *e.g.*, the proposition that all samples of a pure chemical substance have the same melting-point. Now these have to be established in the end by positive confirmatory inductive argument. So the fundamental problem of induction is confirmation by positive instances, and not elimination by negative instances.

(3) *The Theory of Chances.* Mr. Kneale defines a 'probability-rule' as a statement of the form : The probability of an instance of α being β is so-and-so. He symbolizes such a rule by the formula $P(\alpha, \beta) = p$. The calculus of chances is described as the procedure for deriving new probabilities from others which are given.

Mr. Kneale states the axioms and develops the theorems. All this is well done, but it raises no points of special interest. As might be expected, Mr. Kneale is under no illusions about the nature of Bernoulli's limit theorem, which he proves without using the differential calculus. He points out that it, like all theorems in the calculus of probability, merely derives one probability from another. On the other hand, it is a *necessary* proposition, and it is absurd to treat it as a law of nature which might be supported or refuted by experiments with coins or dice. It will be worth while, in this context, just to mention the notation which Mr. Kneale introduces for stating and proving theorems about the probability of a set of individuals having a certain composition. He uses the symbol $P({}^{\alpha}\sigma_n, {}^{\beta}\kappa_m)$ to denote the probability that a set of n instances of α contains exactly m instances of β. He uses a similar symbol, with ${}^{\beta}\rho_p$ substituted for ${}^{\beta}\kappa_m$, to denote the probability that such a set contains a proportion p of instances of β.

I think that these symbols betray an inadequacy which was already latent in the notation $P(\alpha, \beta) = p$. What Mr. Kneale in fact wants to symbolize is the probability that a set of n instances of α will contain exactly m instances of β, *given that* it is selected under certain conditions which might be called 'Bernoullian', and *given that* the probability of an instance of α, so selected, being β is p. He has to state all this separately in words, and is unable to embody these conditions in his symbols. Yet, in the absence of some explicit reference to the first of these conditions, the symbol $P({}^{\alpha}\sigma_n, {}^{\beta}\kappa_n)$ has no definite *meaning*; and, in the absence of some explicit reference to the second of them, it has no definite *algebraical form*, such as, *e.g.*, ${}^nC_m p^m (1-p)^{n-m}$.

Before leaving this part of my exposition I will mention that Mr. Kneale states and proves two interesting theorems of Poincaré's, one about the results of spinning a roulette-wheel, and the other about those of repeatedly shuffling a pair of cards. These he calls 'equalization theorems'.

The philosophically interesting contents of this Part begin in §32, where Mr. Kneale starts to investigate the Frequency Theory of the meaning of probability rules. He takes von Mises' form of this theory as the best available for discussion. This defines $P(\alpha, \beta)$ as the limit which the proportion of instances of β in a succession of instances of α approaches as the number of terms increases indefinitely, provided that the succession is of the kind which von Mises calls a 'collective'. This condition is that, if any endless sub-class be selected from the original succession, in accordance with any rule of place-selection, no matter how fantastic, the limiting proportion of β's in it will be the same as that in the original succession.

There are several well-known and obvious *prima facie* objections to this definition, and von Mises or his followers have attempted to answer them. Mr. Kneale gives a clear and fair account of these objections and the attempted answers. We may pass over this and consider what he has to say on his own account.

(i) The frequentists have often defended their notion of limiting frequencies by alleging that they are analogous to certain limiting notions which are constantly used in science, and to which no-one objects. Mr. Kneale complains that it is not clear what precisely they have in mind here. Is it the ideal figures of pure geometry in contrast with the imperfect straight lines, circles, etc., which occur naturally or can be constructed artificially? Or is it such notions as frictionless fluids, perfect gases, and so on? I should have suspected that it was neither of these, but rather the notions which are expressed by such phrases as 'density-at-a-point', 'velocity-at-an-instant', and so on. However this may be, Mr. Kneale objects that pure geometry is not a natural science and is quite indifferent to whether there are perfect circles, etc., in nature. Again, physicists know very well that there are no frictionless fluids or perfect gases. But the frequentists *define* such statements as '$P(\alpha, \beta) = p$' in terms of collectives and limiting frequencies, and they believe that many probability statements apply within the actual world. Therefore they cannot afford to be indifferent to the question whether there actually are collectives with limiting frequencies, still less can they afford to admit that there are none.

(ii) The definition of a 'collective' involves the notion of *laws* in the strict sense, *i.e.*, propositions of the form: Every instance of S (where the extension of S is potentially unlimited) is P. But these laws are of a very odd kind, and it is very difficult to see why anyone should think he has good reason to accept them. For they are of the form: *Every* infinite selection made by *any* rule of place-selection from the endless succession of α's contains the same limiting proportion of β's as the original succession.

(iii) The notion of a collective of α's in which the limiting proportion of β's is 1 covers two cases which common-sense sharply distinguishes. One is that of law, *viz.*, Every instance of α is β. The other is the case where, although the limiting ratio is 1, yet there are many (it may be infinitely many) instances of α which are not β. If the frequentist thinks that he can get rid of the notion of law and reduce all instances of unitary probability to the second heading, it is plain from what has been said above that he is mistaken.

(iv) Consider, *e.g.*, the following application of Bernoulli's Theorem. If the chance of throwing a 5 with a certain die is 1/6, then there is a very high probability that the percentage of 5's in a set of 1000 throws with that die is in the near neighbourhood of 16·66 per cent. Now let us interpret this in terms of the frequency theory. It will run as follows. If in an endless succession of *single throws* with this die the limiting ratio of the number of 5's to the number of throws is 1/6, then in an endless succession of *sets of 1000 throws* with it the limiting ratio of the number of such sets with about 16·66 per cent. of 5's in each of them to the number of such sets is not far short of 1. Now would a knowledge of the antecedent proposition about the properties of an endless succession of *single throws* give you any good reason to bet on a non-5 rather than a 5 at *the next throw*? And would a knowledge of the consequent proposition about the properties of an endless succession of *sets of 1000 throws* give you any good reason to bet on a percentage of 5's near to 16·66 per cent. in *the next set of 1000 throws*? The answer in both cases seems plainly to be No. Yet a satisfactory analysis of probability-rules ought to account for the fact that we think it reasonable to use them as guides to action in making decisions about particular cases and particular sets of many cases.

For such reasons as these Mr. Kneale rejects the frequency theory of the meaning of the probability-rules.

Mr. Kneale approaches his own theory of the meaning of probability rules by way of a discussion on the notions of Equiprobability and Indifference. He rejects, on the usual and quite conclusive grounds, the Principle of Indifference, *i.e.*, that alternatives are equally probable relative to a person's state of information if he knows of no reason for accepting one rather than another of them. But the fact that this principle gives no satisfactory *criterion* for judging whether several alternatives are equiprobable does not show that it is a mistake to *define* the measure of a probability in terms of equiprobable alternatives.

In developing his own theory Mr. Kneale begins with the case of a characteristic which has a finite range of application, *e.g.*, the concept of undergraduate of Oxford in 1949. To say that two alternatives under such a concept are ' equipossible ' is equivalent to saying that either (i) both are *ultimate*, *i.e.*, non-disjunctive, relative to that concept, or (ii) that each consists of a disjunction of the same number of ultimate alternatives under it. An example under the first heading would be the alternatives of being this or that Oxford undergraduate in 1949. An example of alternatives which are not equipossible, relative to the sizes of the two colleges, are those of being an undergraduate of Christ Church or an undergraduate of Merton. If α is a characteristic with restricted application, the measure of P(α, β) is simply the ratio of the number of ultimate possibilities under [being-an-instance-of-$\alpha\beta$] to the number of ultimate possibilities under [being-an-instance-of-α]. *E.g.*, the chance that an Oxford undergraduate in 1949 will be an undergraduate of Christ Church is simply the ratio of the number of Christ Church undergraduates to the number of Oxford undergraduates in that year.

Mr. Kneale contrasts this with the indifference theory as follows. On his theory, in order that alternatives may be equipossible they must *be* indifferent in a certain way in relation to the characteristic under which they fall, whether this fact is known or believed or not. On the indifference theory the *person* who makes the judgment of equipossibility must be indifferent in a certain way in *his attitudes* towards them.

We can pass now to Mr. Kneale's account of the much more difficult and important case where α is a characteristic which applies to a potentially unlimited class of individuals, *e.g.*, the property of being a throw with a certain die. This seems to me to be much the most difficult part of the book, and I can only state in my own way what I believe to be Mr. Kneale's doctrine.

I shall begin by introducing the term ' specialization of a characteristic '. To be red is a specialization of being coloured ; it may be called a ' determinate ' specialization. To be a cat is a specialization of being a mammal ; it may be called ' specific ' specialization. To be red and round is a specialization of being red (and equally, of course, of being round) ; it may be called a ' conjunctive ' specialization. Any characteristic A can be conjunctively specialized by conjoining it with any other characteristic B which A neither entails not excludes. Similarly AB can be further conjunctively specialized by conjoining it with C, provided that it neither entails nor excludes C. (We must remember, in this connexion, that there is for Mr. Kneale no difference in principle between *nomic* entailment or exclusion, *e.g.*, water cannot flow uphill, and entailment or exclusion of the phenomenological or logical kind, *e.g.*, a surface cannot simultaneously be red and green all over.) Starting with any generic characteristic, we can think of it as first being specialized specifically till we come to the notions of the various lowest species under the genus. Then we can think of each conjunct in the notion of each lowest species being specialized by becoming perfectly determinate in every possible way. Finally, we can think of each perfectly determinate specialization of each such lowest specific specialization being conjunctively specialized by combining it conjunctively with every other characteristic which it neither entails nor excludes. In this way we conceive of a set of *ultimate* specializations of the original characteristic. This, if I am not mistaken, is what Mr. Kneale means by the *Range* of a characteristic. Any possible individual instance of a characteristic must be an instance of *one* and *only* one of the ultimate possibilities in its range; and any two individual instances of it must be instances of *different* ultimate possibilities in its range.

Now at a certain stage in the descending hierarachy of increasingly specialized alternatives under a characteristic there will be alternatives which are *completely* specialized both specifically and determinately and can therefore be further specialized *only* conjunctively.

If I understand him aright, Mr. Kneale calls any such alternative a 'Primary' alternative. Now suppose that α, α_2, . . . α_r, . . . are a set of mutually exclusive and collectively exhaustive *primary* alternative specializations of α. Since each is primary, any further specialization of any of them, *e.g.*, of α_r, must be of the form $\alpha_r\theta$, where θ is a characteristic which is neither entailed nor excluded by α_r, or, as we may say for shortness, α_r is 'contingent to' θ. Suppose now that it were the case that every characteristic to which *any* of the alternatives α_1, α_2, . . . is contingent is a characteristic to which *all* of them are contingent. Then it is plain that to every specialization of any of these alternatives there would correspond one and only one specialization of each of the others. For any specialization of α_r must be of the form $\alpha_r\theta$ (since α is primary), where α_r is contigent to θ. But if α_r is contingent to θ, then any other alternative in the set, *e.g.*, α_s, will also be contingent to θ, by hypothesis. Therefore there would be a specialization $\alpha_s\theta$ of α_s, corresponding to the specialization $\alpha_r\theta$ of α_r. Plainly there could not be more than one. And, since all the alternatives in the set are primary, none of them can have any specializations which are not of this conjunctive form. It follows that any set of alternatives under α, answering to the above conditions, would subdivide the range of α into sub-ranges, each of which covers exactly the same number of ultimate specializations of α. Accordingly, Mr. Kneale gives the name '*Primary* set of equipossible alternatives under α' to any set of mutually exclusive and collectively exhaustive primary alternatives under α, which are such that *all* are contingent to any characteristic to which *any* is contingent. Given a set of *primary* equipossible alternatives, it is of course easy to form sets of equipossible alternatives which are not primary, *viz.*, by taking as the new alternatives disjunctions of equal numbers of the old ones without overlapping, *e.g.*, $\alpha_1 \vee \alpha_2$, $\alpha_3 \vee \alpha_4$, ...

So far we have confined our attention to the range of a single characteristic α. But, if we wish to define $P(\alpha, \beta)$, we must now introduce a reference to β. The next stage is this. Suppose there is a primary set of equipossible alternative specializations of α, such that each of them either entails or excludes β. (In general some would entail it, and the rest would exclude it.) Now, if α_r entails β or if it excludes β, it is plain that the conjunction of α_r with any other characteristic θ will also entail or exclude β, as the case may be. Thus we might say that θ in the alternative $\alpha_r\theta$ is 'superfluous' in respect of its entailing or excluding β. If there is a set of equipossible alternative primary specializations of α, each of which either entails or excludes β, it is plain that there must be such a set composed of alternatives which are *minimal* in this respect, *i.e.*, which contain nothing superfluous to entailing or to excluding β, as the case may be. If I understand Mr. Kneale aright, he gives the name '*Principal* set of alternatives under α with respect to β' to a primary set of equipossible alternatives under α, each of which either entails or excludes β, and each of which is *minimal* in that respect.

At length we come to Mr. Kneale's account of the *meaning* of the statement '$P(\alpha, \beta) = p$'. If I am not mistaken, it is as follows. The *meaning* is the same in all applications, *viz.*, the ratio of the *measure* of the range of $\alpha\beta$ to the *measure* of the range of α. But in different types of application the ranges have to be measured in characteristically different ways. (i) If α determines a *closed* class, *e.g.*, contemporary Oxford undergraduates, then the measure of the range is simply the number of individuals in the class. (ii) If α determines an open *class*, *e.g.*, possible throws with a certain die, the first move is to introduce the notion of a primary set of equipossible alternative specializations of α, each of which either entails or excludes β. Two possibilities then arise. (*a*) Although the range of α is infinite, it may be that the principal set of equipossible primary alternatives under α with respect to β is finite. In that case $P(\alpha, \beta)$ is the ratio of the number of alternatives in this set which entail β to the total number of alternatives in it. (*b*) It may be that the principal set of equipossible primary alternatives under α with respect to β is itself infinite, *e.g.*, they may involve the different values of a *continuous* variable. Mr. Kneale says that, in such cases, the measure of a range has to be conceived as the measure of 'a region in a configuration-space', *i.e.*, by analogy with the length of a line or the area of a surface or the volume of a solid, and $P(\alpha, \beta)$ has to be regarded as the ratio between the measures of two such regions.

Mr. Kneale does not give us much help in connexion with the *general* notion of a configuration-space in probability or with the question how regions in it are supposed to be measured. He does discuss very elaborately certain well-known paradoxes of 'geometrical' probability. His discussion of Bertrand's Paradox about the probability of a chord 'drawn at random in a circle' being longer than the side of the inscribed equilateral triangle seems to me very illuminating.

Reverting to the general topic of the Range Theory, we must note that Mr. Kneale is perfectly well aware that no-one can produce an example of a principal set of equipossible primary alternatives falling under any natural characteristic, such as *human*. He is claiming only to *analyze the meaning* of '$P(\alpha, \beta) = p$'. He does not imagine that a knowledge of this will enable one to *determine the value* of $P(\alpha, \beta)$ *a priori* when α, *e.g.*, stands for *human*, and β, *e.g.*, stands for *male*. All probability-rules about open classes resemble laws of nature, in that they can be inferred only by ampliative induction. The Frequentists are quite right in saying that the evidence for such rules is observed frequencies. Their mistake is to hold that what is inferred is definable in terms of frequency. This mistake is analogous to that of thinking that a *law* is a 100 per cent. *de facto* association. The assumption at the back of both mistakes is that the conclusion of an inference must be a proposition of the same type as the premisses. If Mr. Kneale is right, the conclusions of all ampliative inductions are different in kind from their premisses. For the premisses are in all cases about *matters of fact*: whilst the conclusions, according to him, are *principles of modality*, whether they be laws or probability-rules.

The next important question discussed by Mr. Kneale is whether it can be shown, by means of the calculus of probability, that ampliative induction leads in favourable cases to conclusions which are highly probable in the sense contemplated by that calculus. After examining the so-called 'inversion' of Bernoulli's Theorem, Laplace's Rules of Succession, and Keynes's Principle of Limited Variety, with unfavourable results, Mr. Kneale brings forward what he considers to be a fundamental objection to all attempts to justify ampliative induction within the theory of chances.

His argument may be put as follows. The propositions which we try to establish by ampliative induction are either laws or probability-rules. Let us begin with the laws. Suppose that the law to be established is All S is P. We have observed n instances of S, say S_1, S_2, . . . S_n, and have found that all of them are P. It is claimed that we can show by using Bayes's Theorem that the probability that All S is P, given the conjunctive proposition S_1 is P-and-S_2 is P-and . . . S_n is P, approaches to 1 as n is indefinitely increased. Now it is admitted that the argument requires the fulfilment of the following two conditions. (i) That the *antecedent* probability of All S is P is greater than some number which is itself greater than 0. (ii) That the probability of the conjunctive proposition, given that the law is *false*, approaches indefinitely to 0 as n is indefinitely increased. It is argued that the second condition is fulfilled because the probability of this conjunctive proposition, on this hypothesis, is the product of n terms, each of which is a proper fraction in a sequence whose successive terms do not tend to unity as n is indefinitely increased. Now suppose, if possible, that the law All S is P were just an endless factual conjunction of singular propositions *i.e.*, that it was the proposition S_1 is P-and-S_2 is P-and . . . S_n is P-and . . . . Then by precisely the same argument which proves that the second condition is fulfilled we could prove that the first condition is *not*. On this interpretation of law the antecedent probability of any law would be 0. Therefore, unless the argument is to break down at the first move, it must assume (what Mr. Kneale claims to have shown independently) that laws are *not* endless conjunctions of singular propositions. This is the first step in Mr. Kneale's argument.

The next step is this. The only acceptable alternative analysis of laws is that they are modal principles of necessary connexion between attributes. But it is meaningless to assign a probability, in the sense in which that term is used in the theory of chances, to a modal principle. Probability, in that sense, presupposes real objective alternative possibilities; and it is plainly meaningless to regard a principle of necessary connexion as one alternative possibility among others. Therefore a law has no antecedent probability (and of course no consequent probability) in the sense required by the above attempt to apply Bayes's Theorem.

Now, on Mr. Kneale's view, probability-rules are also modal principles concerning the possibilities that are left open by laws. Therefore they too can have no probability in the sense required in the theory of chances; and therefore there can be no question of showing that the process of ampliative induction from observed frequencies confers upon probability-rules a high probability in that sense.

The last topic which Mr. Kneale discusses in this Part is the theory of sampling from finite populations. Here the conclusion that the population as a whole contains a certain proportion of instances of a given characteristic has a probability in the sense required for the application of inverse-probability arguments. But in practice such arguments are seldom applicable, since we do not generally know the antecedent probabilities of the various alternative possible proportions.

(4) *Probability of Inductive Science.* The question discussed in this Part may be put as follows. Is there any sense of 'justification' in which ampliative induction *needs* justification? If so, *can* it be justified in that sense? And, if so, *how* can it be justified? The discussion is inevitably somewhat complicated. For, in the first place, we have to deal with (1) *primary*, and (2) *secondary* inductions, *i.e.*, those which directly induce laws or probability-rules from observations and those which establish explanatory theories on the basis of such laws. Then, within the discussion of primary induction, we have to consider the establishment of (1.1) *laws*, and (1.2) *probability-rules.* Moreover, a law may be either (1.11) of the purely *qualitative*

form All S is P, or (1.12) of the *functional* form $Y = f(X)$. Lastly, the results of an inductive argument, whether primary or secondary, are not just rationally acceptable or unacceptable. According to circumstances they may be *more or less* rationally acceptable.

The ground has already been cleared to the following extent. We know that it is absurd to think that ampliative induction can be justified in the sense that its conclusions can be *deduced demonstratively* from its premisses. We also know that it is absurd to think that it can be justified in the sense that its conclusions can be shown to have a *high probability* (as understood by the theory of chances) in relation to its premisses. Some persons have concluded from this that the question: 'Is ampliative induction justifiable, and if so, how?' is a meaningless question which would cease to be asked if these negative facts were pointed out and appreciated. Mr. Kneale does not accept this conclusion. According to him, induction is a 'policy' which one might or might not adopt in certain situations in which we are all very often placed. The question is whether we can show, apart from all reference to the truth or the probability (in the technical sense) of inductive conclusions, that inductive policy is the one which a sensible person, aware of his own needs, resources, and limitations, 'could not fail to choose'. I think that the phrase in inverted commas is highly ambiguous, and I am not perfectly sure what Mr. Kneale means by it. The meaning may become clearer to the reader when he has seen the application.

What then *is* the policy of primary induction (*a*) in regard to laws of the form All S is P, (*b*) in regard to laws of the form $Y = f(X)$, and (*c*) in regard to probability-rules?

(*a*) Let us use the symbol 'S_0' to denote *observed* instances of S, and similarly *mutatis mutandis* for 'P_0' and 'Q_0'. Suppose that the empirical facts can be stated as follows. All S_0 is P. All S_0 is Q. Some P_0 is neither S nor Q. Some Q_0 is neither S nor P. The only laws which are compatible with these observarions are All S is P, All SQ is P, All S is Q, and All SP is Q. The most timid policy would be to formulate no laws at all. Still playing for safety, one might formulate the laws All SQ is P and All SP is Q. The boldest policy consistent with the observations would be to accept tentatively the laws All S is P and All S is Q. The other aspect of the policy would be to look out for instances of S which were not P and instances of S which were not Q. But, unless and until such instances were found, it would be contrary to the policy to be content with the more restricted laws All SQ is P and All SP is Q. The policy here may be summed up as follows. (i) Act in all relevant circumstances on the assumption that combinations of characteristics of which you have found no instances in spite of seeking for them are incompatible. But (ii) continue to look for instances of such conjunctions, and be prepared to admit extensions of the range of what you have hitherto taken to be possible so far and only so far as fresh observations compel you to do so.

(*b*) The inductive policy in the case of functional laws is as follows. Act on the assumption that the law connecting the values of Y with the associated values of X is the 'simplest' consistent with the observations made up to date, but be on the look-out for new pairs of associated values which this curve fails to fit. Here one curve is 'simpler' than another if it requires fewer independent parameters to determine it completely; in this sense a straight line is simpler than a circle, a circle than a parabola, and a parabola than an ellipse or an hyperbola.

(*c*) In the case of probability-rules the inductive policy is as follows. If the relative frequency of instances of α which are β among all the instances of α which have been observed is p, act on the assumption that the value of $P(\alpha, \beta)$ is p. What we are trying to do in such cases, on Mr. Kneale's interpretation of $P(\alpha, \beta)$, is to make the best guess that we can, on the basis of the available statistical evidence, as to the ratio of the range of possibilities under $\alpha\beta$, left open by all the principles of necessitation and exclusion, to the range of possibilities under α, left open by those principles. It should be noted that to act on this policy is equivalent to assuming that value of $P(\alpha, \beta)$ which gives the maximum probability to the actual frequency of β's found in the finite class of n observed instances of α, *i.e.*, which maximises the value of $P({}^{\alpha}\sigma_n, {}^{\beta}\rho_p)$.

The policy in all three cases falls under the following general maxim. In any case where you have to act, either practically or theoretically, on partial knowledge, act as if you knew that the boundaries of possibility lie as nearly as may be to the actual associations and dissociations and proportions which you have observed and critically tested up to date.

Why, and in what sense, is this policy 'reasonable' or 'justifiable'? We are often in a position where our practical or theoretical interests oblige us to treat an object, of which we *know* only that it is or will be an instance of α, as if it were or would be β or as if it were or would be non-β. The only way in which we can do this is by assuming the truth of a relevant law or probability-rule on the basis of our observations up to date. If all the observed instances of α have been β, it is for various reasons more profitable to assume the law that All α's are β than to assume any less sweeping law, such as All $\alpha\gamma$'s are β, or to assume merely that a certain percentage of α's are β. The advantages are the following. If the supposition should be false, it is likely to be sooner refuted by counter-instances than any of the less sweeping suppositions compatible with the at present known facts. If, on the other hand, it should be true, it will be more powerful as a premiss for inference than any of these less sweeping assumptions. To this it may be added that, if one were to postulate anything but the strongest law consistent with the known facts, it is difficult to see where one could reasonably draw a line, since any set of observed instances of S which were all P would have innumerable properties in common beside S and P.

The justification is very similar in the case of functional laws. Suppose, *e.g.*, that you have observed n pairs of associated values of Y and X, and have found that they all fall on a certain straight line $y = a_0 + a_1 x$. The law connecting Y with X must be represented either by this straight line or by one of the innumerable curves of higher order which cut it in at least those n points but diverge from it elsewhere. If the linear hypothesis should be false, a single unfavourable further observation will suffice definitely to refute it; but, however the $n+1$th observation may turn out, it will be consistent with innumerable more complicated laws, between which one would have no reasonable ground for choosing.

I doubt whether I fully understand Mr. Kneale's argument to justify the procedure of assigning to $P(\alpha, \beta)$ the value p, when one has examined n instances of α and found that they contain a proportion p of β's. It certainly starts from the proposition (which is easily proved) that to assign any *other* value than p to $P(\alpha, \beta)$ would entail a *lower* value for the probability that a set of n instances of α would contain the observed proportion p of β's. The argument then seems to run as follows. By definition, the latter probability is the ratio of the range of possibilities under the property of being an n-fold set of α's containing a proportion p of β's to the range of alternatives under the property of being an n-fold set of α's containing *any* proportion of β's from 0 to 1. Now, it is alleged, the extent of the former range is *independent* of the value of $P(\alpha, \beta)$, whilst the extent of the latter range is *dependent* on the value of $P(\alpha, \beta)$. It follows that the value of $P(\alpha, \beta)$ which makes this ratio a maximum is the value which makes its denominator a *minimum*. Therefore, to assign as the value of $P(\alpha, \beta)$ the observed frequency p, with which instances of β have occurred in the n-fold set of α's examined, is equivalent to assuming that the range of possibilities under the property of being an n-fold set of α's containing *any* proportion of β's is as *narrow* as is consistent with the observations.

The step in this argument which I do not understand is the statement that the range of alternatives under the property of being an n-fold set of α's containing a proportion p of β's is *independent* of the value of $P(\alpha, \beta)$, whilst the range of alternatives under the property of being an n-fold set of α's containing *any* proportion of β's from 0 to 1 is *dependent* on the value of $P(\alpha, \beta)$. Let us take, *e.g.*, a finite class of N α's, and suppose it contains exactly Nq β's. Then the value of $P(\alpha, \beta)$ is q. Now the number of possible n-fold sub-classes containing a proportion p of β's would seem to be

$$^{Nq}C_{np}\ {}^{N(1-q)}C_{n(1-p)},$$

i.e., to be *dependent* on q, the value of $P(\alpha, \beta)$. And the number of possible n-fold sub-classes of *any* possible constitution in respect of β would seem to be $^{N}C_{n}$, *i.e.*, to be *independent* of q. This is the exact opposite of Mr. Kneale's statement. I suppose that there must be a simple misunderstanding somewhere, but I cannot make out where it lies.

The last topic to be discussed under this head is the varying degrees of irrationality which are involved in departing from the inductive policy under various circumstances. Here Mr. Kneale distinguishes two defects in a hypothesis, which he calls 'Extravagance' and 'Negligence'. The former applies both to assumptions of law and assumptions of probability-rules. The latter applies only to the case of laws. I will take them in turn.

As we have seen, if we follow the inductive policy we are in effect ascribing to $P(\alpha, \beta)$ that value which maximizes the probability that an n-fold set of α's would have the proportion of β's which it has in fact been found to have. Mr. Kneale defines the 'extravagance' of any departure from the inductive policy as the ratio of the diminution of this probability, entailed by that departure, to the maximal value, which it has if the policy is followed exactly. It is easy to show that, with this definition, the extravagance of a given departure from that value of $P(\alpha, \beta)$ which the inductive policy would dictate increases with the size of the sample observed. The formula covers the two extreme cases of 100 per cent. and 0 per cent. observed frequencies of β among α's, where the inductive policy would be to postulate a *law*.

'Negligence', in the technical sense, consists in assuming only a probability-rule where the observations are consistent with a law; or in assuming a law with a more restricted subject or a less determinate predicate when the observations are compatible with a law with a less restricted subject or a more determinate predicate.

So much for Mr. Kneale's views on the 'justification' of primary induction; it remains to consider the 'justification' of secondary induction.

A theory is put forward to *explain* laws and probability-rules which have been or may be established by primary induction. A successful theory introduces *simplification* in two different, though connected, senses. In the first place, it must, of course, entail all the primary generalizations which it is put forward to explain, and others too which can be tested. Now it seems clear that the question whether a generalization, which is entailed by a theory, was established by primary induction *before* or *after* the putting forward of that theory cannot be of any logical relevance to the support which it gives to the theory. If a newly drawn consequence is to support the theory, it must be verified by primary induction before it can do so; and, when once this has been done, it is in the same position as the already verified generalizations which the theory was originally put forward to explain. Mr. Kneale concludes that a theory is not worth serious consideration unless it entails an unlimited number of testable consequences. If this be granted, the first sense in which a successful theory simplifies is that it restricts the realm of possibility more than is done by any finite number of empirical generalizations entailed by it.

The second sense in which a successful theory simplifies is that it reduces the number of independent concepts, and thus reduces the number of independent propositions, which we have to accept. An example is the unification of electricity, magnetism, light, etc., by Maxwell's Theory.

If the acceptability of a theory is to rest on its having been formulated and tested in accordance with a policy indispensable to pursuing an end which we seek, we must ask what that end is. Now theories certainly have the following two uses. A theory suggests subjects which it may be profitable to investigate by primary induction, and thus has an important directive use. Again, when it is shown that a number of primary generalizations are all consequences of a theory, the special evidence for each is reinforced by the evidence for all the rest. But, Mr. Kneale holds, these two valuable services which theories render are not the ultimate motive for theorizing by scientists. Men desire explanation for its own sake, and this desire is the main motive with pure scientists. The satisfaction derived from a good theory is in certain ways analogous to aesthetic satisfaction. But there are important differences. Scientific theorizing is not *free* construction, like musical composition. The scientist wants his theories to be *true*, and the minimum condition is that they shall be consistent with all known empirical facts. Moreover, he has the ideal of a single *all-embracing* theory, under which all possible empirical generalizations can be subsumed, and to which there is no alternative. Why men should have this ideal we do not know, but it is a fact that great scientists do have it. Secondary induction is justified in so far as it is the only policy by which we can set about realizing this ideal. We have no guarantee that it is realizable, and, if we happened to have realized it, we could never *know* that we had done so. But, if there *is* a single system of natural necessity, then the procedure of secondary induction is the only policy by which we can hope to approximate our beliefs to it.

(II) CERTAIN CHARACTERISTIC DOCTRINES OF MR. KNEALE. As we have seen, Mr. Kneale holds the following unfashionable views. (i) That laws of nature are principles of necessity, of the same nature as the proposition: A surface cannot be at the same time red and green all over; though, unlike that proposition, they are incapable of being revealed by intuitive induction and known *a priori*. (ii) That such propositions are not merely linguistic. It will be convenient to consider his views on these two points in the opposite order to that in which I have stated them.

(1) *Principles of Modality are not merely linguistic.* Principles are truths about the possibility or impossibility of certain characteristics being combined in facts of a certain structure. They are more fundamental than facts, in the sense that it depends on them what are possible facts and what are not. On the other hand, we could not formulate any principle unless we were acquainted with, and had formulated, some facts. For, in the first place, we could not be aware of any characteristic unless we were acquainted with

facts in which it is a component. And, secondly, unless we had formulated some facts, we should have no means of symbolizing the structure of various kinds of possible fact. Mr. Kneale holds that all knowledge of singular negative facts, *e.g.*, the fact that the paper on which I am writing is not blue, involves knowing principles as well as facts. I must know, *e.g.*, the fact that this paper is white. But I must also know that it is possible for paper to be blue, and that being white all over is incompatible with being blue all over. This seems to me to be obviously true.

Consider now the allegation that the sentence 'It is impossible for anything to be at once red and green all over' merely records a linguistic convention that no sentence of the form 'X is at once red and green all over' is to be used. Certainly it is a matter of linguistic convention that 'red' means what it does in English and that 'green' means what it does in English. It is quite possible, *e.g.*, that 'red' should have meant what it does now mean, and that 'green' should have meant what is now meant by 'scarlet' or what is now meant by 'hot'. In that case the sentence 'X is at once red and green all over' would have been permissible. The fact that it *is* not permissible depends on the fact that 'red' and 'green' at present mean two characteristics which are in themselves incompatible spatio-temporally. And it *would have been* permissible only if the meaning of one or of both of these words had been such that they name characteristics which are in themselves spatio-temporally compatible. *Any* language which contains names for the characteristics of which the words 'red' and 'green' are the names in contemporary English will have to use those words in accordance with a rule corresponding to the English rule about the use of 'red' and 'green'. And that is because the rule states a principle concerning the characteristics of which these words are names. This, again, seems to me to be quite obviously true.

Mr. Kneale adds the following argument, which I give for what it may be worth. When one learns how to use a word, *e.g.*, 'red', correctly, an essential part of what one learns is *not* to use it *unless* a certain condition C is fulfilled. In order to act on this knowledge one must be able to recognize cases in which C is *not* fulfilled. But one can never know a negative singular fact without using one's knowledge of a principle of incompatibility. Therefore ability to avoid using a word incorrectly involves knowing principles of modality.

(2) *Laws are Principles of Modality.* Mr. Kneale's view of the nature of laws may be compared with democracy in at least one respect. There are strong *prima facie* objections to it, and the only good arguments for it are the arguments against all the alternatives. Accordingly, we shall be concerned mainly with his criticisms of alternative analyses of law, and with his attempt to answer the *prima facie* objections to his own analysis of it.

The two alternative analyses which are worth serious consideration are the following. (i) It might be alleged that the law: All S is P can be identified with the unrestricted factual proposition: Every instance of S that has been, is, or will be, has been P or is P or will be P, as the case may be. (ii) It has been alleged that laws, though expressed by sentences in the indicative, like 'All S is P', are not really propositions at all. They are prescriptions, which would be less misleadingly expressed by a sentence in the imperative, *e.g.*, 'Whenever you meet with an instance of S and do not know whether it is P or not, act on the assumption that it is P'.

It has been objected to the purely factual analysis of law that, if it were true, no law could conceivably be verified by experience, and that this would entail that all nomic sentences are meaningless. Mr. Kneale does not accept this argument, because he rejects this criterion of significance. He points out that the statement 'There is at least one instance of S which is not P' is certainly capable in principle of being verified, and is therefore significant by this criterion. It would be strange if this significant statement should have no significant contradictory.

Mr. Kneale's own objection is radical. Laws are not facts at all, and therefore not facts of the form alleged. To state a law properly we need a *conditional* sentence, not a mere sentence in the indicative. If it is a *law* that all S is P, then anything that *had* been, or that *might* now be, or that *should* in future be an instance of S *would* have been P, or *would* now be P, or *would* then be P.

Since nomic sentences are not statements of fact, anyone who denies that they are statements of modal principles of necessity, is practically forced to hold that they are not really statements at all but are disguised prescriptions. Now a prescription is either a command or an admonition. If Boyle's Law, *e.g.*, is a command, like 'form fours', one would wish to know, before obeying it, who issues the command, what authority he has for doing so, and what penalties he can and will inflict in case of disobedience. Obviously there is no answer to these legitimate questions in the case of a law of nature. If on the other hand, it is an admonition, like 'Cast not a clout till May be out', it is reasonable to ask what advantages are to be derived or what disadvantages are to be avoided by following the advice. If the person who gives us this advice answers that acting in this way will enable one to make successful predictions, he appears to be enunciating a law of nature in a non-prescriptive sense. If he answers that this is the policy which scientists do pursue, one can raise the following two supplementary questions. 'Do you mean merely to put on record the way in which scientists have in fact behaved up to date, or are you enunciating a *law*, in the non-prescriptive sense, about the behaviour of a certain class of human beings?' And whichever answer is given to this question, one can then ask: 'What is the relevance of your answer to the question why I should follow your advice in this matter?' To put it shortly, is there any reasonable ground for following the advice to act as if

S were P whenever you meet an instance of S except that there is reason to believe that: All S is P is a law of nature in the non-prescriptive sense?

Finally, we can consider Mr. Kneale's answer to the *prima facie* objection that laws of nature cannot be principles of necessity, because any principle of necessity would be capable of being known *a priori* whilst no law of nature can be so known.

The objection is often put in the form that, if you can imagine an instance of S which is not P, then S cannot necessitate P. I shall state what I believe to be Mr. Kneale's main contentions in my own way and with my own examples.

In the first place, an example from pure mathematics has a certain relevance to the objection. Take the proposition that the square-root of 2 is irrational. This means that there are no two integers *m* and *n*, such that the ratio of *m* to *n* (reduced to its lowest terms) squared is equal to 2. Now this proposition is true and necessary and easily proved. But there is an important sense in which it is perfectly easy to 'imagine what it would be like' if the proposition were false. One can imagine oneself applying to the number 2 the ordinary process for extracting a square-root, and finding that it came to an end after a finite number of steps, as it does, *e.g.*, after two steps if applied to the number 841. This example is useful as a counter-instance to the general principle that a proposition cannot be necessary if one can 'imagine what it would be like' for it to be false. But it would be a mistake to rest any positive analogy on it; for laws are certainly different in kind from propositions about numbers, even if they be of the same kind as propositions which can be established by intuitive induction.

Coming to Mr. Kneale's main contention, I find it easier to give an account of the explicit premisses, the main steps of the argument, and the conclusion, than to indicate the precise connexion between them. Mr. Kneale begins by pointing out that natural laws are concerned with *perceptual* events and things, *e.g.*, flashes of lightning or samples of ammonia, and not with merely *sensible* events and objects, such as the visual sense-datum which is presented to a person when he sees a flash of lightning or the olfactory sense-datum which is presented to him when he smells a whiff of ammonia.

He then considers the relation between sensation and sense perception. He accepts the conclusion that the statement 'X is seeing the perceptual-object O' implies (i) that X is sensing a certain visual sense-datum, and (ii) that this, in some sense, 'belongs' to a certain physical object which can be correctly described as, or named by, 'O'. In considering what meaning to attach to the word 'belongs' in this context he rejects any view which would imply that it is intelligible to suggest that there might be a sense-datum which was not sensed by anyone. After considering and rejecting various alternative theories, Mr. Kneale says that he thinks that the following is 'correct so far as it goes'. Statements involving names and descriptions of perceptual objects and their properties are not *reducible to* statements about actual and possible sensations; but they are an *appropriate device* for referring briefly and compendiously to innumerable propositions about the sensations which would be experienced under innumerable different conditions. It must be noted, however, that an unlimited number of these propositions about sensations would be of the form: 'If a person *had* been in such and such a place at such and such a time and *had* then and there done such and such things, he *would* have had such and such sensations', where no-one in fact was there or did those things at that time. Such propositions about the consequences of unfulfilled conditions seem to involve, either directly or at a later move, propositions to the effect that one kind of sensible experience would *necessitate* a sensible experience of a certain other kind.

Mr. Kneale concludes from all this (what is undoubtedly true) that perceptual-object words, like 'lightning', 'ammonia', 'flexible', 'soluble in water', and so on, obey utterly different rules from words and phrases about individual sense-data and their qualities. He suggests that the opinion that laws of nature would be knowable *a priori* if they were principles of necessity has arisen only because people have either failed to notice that such laws are concerned with perceptual objects and their properties, or have failed to see that propositions about the latter differ fundamentally from propositions about sense-data and their qualities.

Now it is this last vitally important contention which seems to me not to have been adequately developed and illustrated by Mr. Kneale. I think that he ought to have done the following three things. (i) To produce evidence that competent contemporary philosophers who disagree with his views on the nature of laws *do in fact* fail to see the distinction in question. For my part, I very much doubt that they do. (ii) To show us *why* principles of necessary connexion concerning sense-data and their qualities might be expected to be capable of being known *a priori*. And (iii) to indicate *how precisely* the admitted differences between sense-data and their qualities, on the one hand, and perceptual objects and their properties, on the other, make it impossible that any principle of necessary connexion concerning the latter should be known *a priori*. If Mr. Kneale has given an adequate answer to questions (ii) and (iii), I must confess that I do not understand it clearly enough to be able to convey it to the reader.

I greatly hope that Mr. Kneale will enlighten us further on these points. In the meanwhile he may be heartily congratulated and thanked for the bold, original, and extremely well-written contribution which he has made to one of the hardest and weightiest of the problems of philosophy.

C. D. BROAD.