

II.—THE RELATION BETWEEN INDUCTION AND PROBABILITY—(Part II.).

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1.

IN the first part of this paper, in *MIND*, No. 108, I tried to show that the statement of inductive arguments in terms of probability is a necessary but not a sufficient condition of their validity. We saw that the laws of probability and the ordinary assumptions about equiprobability will not suffice to justify a strong belief in any law or even in a prediction for a few steps ahead. Some additional proposition about nature and not merely about probability seemed to be needed if induction were to be anything more than a guessing game in which we have so far had surprising luck. In this second part I propose to try and find what propositions are needed and what kind of evidence there is for them.

2.

The usual view of the logic books seems to be that inductive arguments are really syllogisms with propositions summing up the relevant observations as minors, and a common major consisting of some universal proposition about nature. If this were true it ought to be easy enough to find the missing major, and the singular obscurity in which it is enshrouded would be quite inexplicable. It is reverently referred to by inductive logicians as the Uniformity of Nature; but, as it is either never stated at all or stated in such terms that it could not possibly do what is required of it, it appears to be the inductive equivalent of Mrs. Gamp's mysterious friend, and might be more appropriately termed Major Harris.

It is in fact easy to prove that this whole way of looking at inductive arguments is mistaken. On this view they are all syllogisms with a common major. Now their minors are propositions summing up the relevant observations. If the observations have been carefully made the minors are practically certain. Hence, if this theory were true, the conclusions

of all inductive arguments in which the observations were equally carefully made would be equally probable. For what could vary their probabilities? Not the major, which is common to all of them. Not the minors, which, by hypothesis, are equally certain. Not the mode of reasoning, which is syllogistic in each case. But the result is preposterous, and is enough to refute the theory which leads to it.

Though we have thus cleared the ground of a false view its falsity leaves us with a much harder task than we should have had if it were true. For it is now by no means obvious in what direction to look for the missing premise about nature. Two courses seem open to us. (i) We might consider just where induction breaks down if it does not assume any premise about nature. We might then try to think of one or more propositions which would suffice to remove the difficulty. Lastly we might try to pare these down to their irreducible minimum and see whether they be self-evident or have any good evidence for or against them. (ii) But it will evidently be wise to use another method as a clue. We regard some inductive conclusions as fairly trustworthy and others as much less so. It will be wise to consider what assumptions or knowledge we have at the back of our minds when we make inductions. These may be betrayed by comparing the cases where we are satisfied with the induction with those where we are not. We can then state these assumptions explicitly; see whether they do suffice to make some inductions fairly probable; and consider the evidence for or against these assumptions. It seems reasonable to hope that the first method will suggest to us the *kind* of propositions about nature that are wanted, and that the second will suggest the actual propositions which people use when they make inductions. And we may hope that the latter will be instances of the former.

3.

Induction by simple enumeration has so far been wrecked on two different reefs. (1) The number of S's examined could only bear a vanishingly small proportion to all the S's in the world, even if any one S were as likely to have fallen under our notice as any other. The result was that the number of antecedently equiprobable hypotheses about the proportion of S's which are P is enormous, and therefore the antecedent probability of the only pair which would be laws, *viz.*, All S is P and No S is P—is vanishingly small. (2) It is certain that not every S is equally likely to have fallen into the class

of observed S's; for those which begin to exist after the experiment is concluded or exist in places remote from all the experimenters could not possibly have fallen into this class. It is pretty clear what *kind* of proposition is needed to diminish the first difficulty. We want some proposition which favours *laws* (i.e., universal propositions) as against propositions of the form $n\%$ of the S's in nature are P's; so that all S is P or no S is P shall be antecedently much more probable than the innumerable possible alternatives. And I have no doubt that this is what people must have had in mind when they spoke of the Uniformity of Nature and told us that it was a necessary premise of all inductions. But they hardly noticed how extremely difficult it is to state any such proposition in a form in which it is not flagrantly false. The variety of nature is just as marked as its uniformity; and, on the face of it, far more certain, since variety can be directly observed, whilst uniformity, strictly speaking, cannot. It is all very fine to adopt a haughty attitude towards particular propositions and to call them trivial; the fact remains that many such propositions are true, and that it is excessively difficult to state any principle which will favour laws as against particular propositions and not fly in the face of the facts. I can indeed state a principle of uniformity which will be compatible with any amount of variety, but I am far from sure whether it is either true or useful. The principle would be this:—

$$\phi a . \psi a . \supset : (\exists \chi) : \chi \neq \psi . \chi a : \phi x . \chi x . \supset_x . \psi x .$$

This means that if any individual a has the property ϕ and the property ψ [e.g., is a swan and is white] then there is some property χ other than whiteness [e.g., that of being European] which is possessed by a , and such that everything that is both ϕ and χ [e.g., is a European swan] is also ψ [e.g., is white]. The condition $\chi \neq \psi$ is added to avoid triviality, since if χ might be ψ a χ fulfilling the conditions always exists for $\phi x . \psi x$ analytically implies ψx . Of course χ might be identical with ϕ .

I am inclined to think that this is what those logicians like Prof. Bosanquet who say that all particular propositions are imperfectly apprehended universals have in mind. I am the more inclined to this view because this principle does make all laws simply convertible in a certain sense, and this is another characteristic opinion of the same school of logicians. Suppose that in the above formula we substitute everywhere ψ for ϕ and ϕ for ψ . We get

$$\psi a . \phi a . \supset : (\exists \chi) : \chi \neq \phi . \chi a : \psi x . \chi x . \supset_x . \phi x .$$

Of course the χ will not in general be the same in the two cases ; but it does at least follow from the principle that there is always an universal proposition with ψ as subject and ϕ as predicate as well as one with ϕ as subject and ψ as predicate. And I can hardly suppose that these logicians intend to maintain much more than this.

Another principle, which many people seem to believe, can be deduced from the above. Many people would say that, if you find that some swans are white and that some are not, this is never the whole truth about the matter ; all the white swans must have something common, and peculiar to them which 'accounts for' their whiteness.

A little simple logical manipulation leads to the proposition :

$$\phi a . \phi b . \psi a . \neg \psi b \supset : (\exists \chi, \theta) : \\ \chi a . \theta b . \chi \neq \psi . \theta \neq \bar{\psi} : \phi x . \chi x . \supset x . \neg \theta x .$$

e.g., If a and b are swans and a is white and b is not then there is another property χ possessed by a and a property θ possessed by b such that no swan with the property χ has the property θ .

4.

Now the proposed principle, which we will call *Unax* for short, must be admitted to have certain merits. If *Unax* were true the problem of induction would be shifted and lightened. Without it we do not know whether there is any law connecting S with P ; we are therefore liable to go wrong in two ways : (*a*) by thinking that there is a law and that we have discovered it when really there is no law at all, or (*b*) by thinking that the law is All S is P when really it is of some more complex form such as All SQ is P . If *Unax* be granted the first source of error vanishes. The second, which corresponds to the second difficulty in induction by simple enumeration, remains. But it could certainly be reduced by examining S 's under as various conditions as possible. We could never end by being sure that the law took the simple form All S is P , but we might conclude with fair confidence that, *if* it be All SQ is P , the factor Q is pretty abstract and accompanies S under extremely variable conditions, so that for most practical purposes, it is negligible.

Unax also has the merit that it could never be refuted by experience. Whenever you seem to have a conjunction of attributes ϕ and ψ which is not an instance of a general law of the form $\phi x . \chi x . \supset x . \psi x$ you can always say that this is because the property χ is too minute or obscure to be detected

by our present means of observation. No one could refute this possibility; and, if you believed it, it would furnish a motive for further and more accurate investigations.

This, however, is about all that can be said in favour of Unax. There remains much to be said against it. In fact Unax may be a first approximation to the principle for which we are looking; but it seems quite certain that, as it stands, it is in some ways far too general and in others not general enough, and that it is neither ultimate nor plausible. By developing these criticisms we may find out in what direction to look for more light.

(i) Unax, as stated, makes no difference between ϕ and ψ ; they may be any properties or combinations of properties. Now when ϕ is a property like being a swan or a crow and ψ is a property like whiteness or blackness the principle seems plausible enough. But suppose that ϕ were a property such as being spherical. I hardly imagine that the statement that, if anything is spherical and white, then it possesses some other property χ , such that all spherical objects with the property χ are white, would seem plausible. It therefore looks as if ϕ and ψ must not be properties which are wholly unrestricted, and that in fact ϕ must be a property of a very special sort, if the statement is to seem plausible. This is reinforced by the following consideration. We have seen that, if we take Unax without any special hypothesis about ϕ and ψ , two laws correspond to every conjunction of attributes. Now many people would hold that if a swan is white there must be some property χ possessed by this swan such that all swans with this property are white. But how many people would hold that if a white object is a swan there is some property χ , other than that of being a swan, which is possessed by this white object and is such that all white objects with the property χ are swans? Yet this, as we have seen, equally follows from Unax, if ϕ and ψ are supposed to be subject to no special hypothesis in it.

(ii) For Unax a *single* conjunction of attributes is enough to make it certain that this conjunction is an instance of *some* general law. Nor is it easy to see how this could be otherwise, for the influence of *number* of instances seems to have been exerted in the only way in which it can be relevant, *viz.*, through the laws of probability, before ever Unax was invoked. I hardly see how any principle about nature which is to do the work required of it can refer to the number of observed instances. If it is about nature it is about what exists whether we observe or not, whilst the number of instances observed is at least partly dependent on our own actions.

Yet many people who would agree that a good number of observed conjunctions of ϕ and ψ make it certain that ϕ and ψ are connected by a law would hesitate to say that a single such conjunction makes it even highly probable. It is important to be quite clear as to the precise nature of the difficulty here. (a) Nobody supposes that, with Unax or without, a single instance of ϕ conjoined with ψ makes the particular law that ϕ is always accompanied by ψ probable. But (b) Unax does say that a single instance makes it absolutely certain that there is *some* general law connecting ϕ with ψ . Now most people would be inclined to hold (c) that a fair number of instances of conjunction are needed to make even this probable, though a fair number will make it practically certain. Now their view is not supported at all by the probability-theory of induction *without* Unax; whilst, if they accept Unax as offered, their view is unintelligibly timid. Hence it must be supposed that they accept some principle about nature which is less sweeping than Unax; yet it is very difficult to see what principle about *nature* there could be which makes *number of observed conjunctions* relevant at just this point.

5.

I am inclined to think that both these difficulties (i) and (ii) are to be met by the same modification. When do inductions by simple enumeration seem to be highly plausible and when not? They seem plausible when we are dealing with substances which are believed to belong to what Mill would call a Natural Kind. We believe pretty strongly in the results of such inductions when they deal with the properties of such things as crows or swans or pieces of silver. But no one attaches much weight to inductions about the colour of billiard balls or counters in a bag. If Unax is to be rendered plausible it must be subject to the restricting hypothesis that ϕ is a property or set of properties defining a kind. If this be granted we see why common sense will not allow the reversibility which Unax permits when ϕ and ψ are unrestricted. Unax now takes the form:—

$$\phi\epsilon K. \phi a. \psi a. \supset : (\exists \chi) : \chi \neq \psi. \chi a : \phi x. \chi x. \supset_2. \psi x.$$

This we will call *Unaxk*. Now Unaxk says nothing about ψ defining a kind; hence, on substituting ψ for ϕ and ϕ for ψ , we get nothing startling, but merely a proposition with an hypothesis $\psi\epsilon K$ which is in general false.

We can also see now why common sense wants a number of observed instances before it will consent to be sure that there is *some* law connecting ϕ with ψ . It wants these instances in order to persuade it of the truth of the hypothesis that ϕ defines a kind.

It can only feel sure of this when it has met with a fair number of instances of ϕ and found that they have a great number of properties beside ϕ common and peculiar to them.

Finally (iii) we can now admit that Unax is not ultimate, and can see why. Unax is only plausible in the modified form of Unaxk. Unaxk refers essentially to kinds, and we have not as yet analysed what is meant by kinds and what is involved in the assumption that there are kinds in nature. Any further progress in solving our problem will therefore depend on a careful discussion of this subject. We must therefore bid Unaxk a long farewell for the present and turn our attention to the assumption that there are natural kinds.

6.

Even without entering at all deeply into the question of kinds we can see in a general way how the assumption of kinds affects the problem of induction about the properties of substances. Such inductions seem most plausible when the subject is a well-marked class like swans or crows and the predicate some fairly general and simple property like blackness or whiteness. Now the mere fact that ordinary language has taken the trouble to invent a general name like *swan* or *crow* tells us a good deal about nature. It implies that a large number of objects have been met with which have combined pretty constantly a large number of properties varying only within fairly narrow limits. It is true that you may *define* a crow or a swan or a man by a few properties. But this very fact is symptomatic. Whatever may be the dictionary meaning of 'man' we always mean by it something with a great many more properties than animality and rationality or two-leggedness and featherlessness. Anything that had these properties but differed widely in other respects from the men that we had met would only with great hesitation be called a man. Hence the fact that we are content with the dictionary definition is due to the fact that so far in our experience the properties mentioned therein have been associated with a whole bunch of other properties, and that all these have been exemplified together with but slight variations in a great number of instances. Thus when we ask ourselves the question: Are all S's also P? and suggest the possibility that

some may not be P we imply that P is only one of a large number of attributes, and we imply that a slight variation in P is consistent with the bulk of the remaining attributes being unchanged. For with any large change, we should cease to go on calling the object *an S*, and thus, even if this object turned out not to be a P, this would not be relevant to the question whether all S's are P; for this object would not be counted as an S.

So the actual state of affairs in any induction about substances to which we should be inclined to attach much weight is this: (a) A large number of individuals have been observed all of which had a large number of attributes in common and only differed by small variations of these attributes within narrow and characteristic limits. Scarcely any individuals have been observed which agreed with the former in a great many respects, but otherwise differed profoundly from them. And if such have been observed and have been numerous they count as a different kind and have a different name, so that no question arises of treating them along with the former individuals in making our induction. (b) The attribute P has been found to be present in all these individuals. This attribute is not of such importance that a change in it alone would prevent an object otherwise agreeing with other S's from being called by the name S. (c) If there be other individuals which agree so far with those already observed as to be appropriately called by the same general name S as they, how probable is it that they will also agree in having the attribute P?

The superior plausibility of inductions about kinds is thus partly a matter of words; but, like most matters of words, it rests ultimately on a matter of fact. The purely verbal point is that, unless the unobserved objects resemble the observed S's in the vast majority of their attributes they will not be called S's, and the question whether they be P or not will be irrelevant to the question whether all S's are P. The factual basis of all this is that a large number of very similar individuals have been observed; if they had not been numerous and had not exemplified an outstanding bunch of attributes men would not have troubled to give them the special name S. Thus, in any actual induction, the evidence is never merely the *number* of examined instances, but also the predominant agreement of all these instances with each other and the presupposition that the doubtful and unexamined cases must predominantly agree with the examined ones in order to count as relevant instances for or against the suggested law.

We might put the argument in the following way. The objections to induction by simple enumeration about the properties of substances are unfair to that process in the only case where anyone attaches much weight to it. They are unfair for two reasons: (a) They do not state the problem properly; and (b) they do not consider the whole of the evidence. Let us consider these points.

(a) It is unfair to put the problem in the following form: 'All the observed S's are P. There are innumerable unobserved S's. What is the probability on your observation that all these are P?' For what is the evidence that there are innumerable unobserved S's? Surely it is of just the same kind as the alleged evidence that the unobserved S's are P. You have observed a large number of S's; they were all P. If the observation of a large number of observed S's be a good ground for thinking that there are innumerable unobserved S's it would seem to be an equally good ground for thinking that there are innumerable SP's; for all the observed S's were in fact SP's. I do not at present wish to assert that we have good evidence for *either* conclusion; but it is obviously unfair to talk as if we were certain of the former and to make this a ground for feeling doubtful about the latter. It does seem likely that anything that is evidence for the one will be in its degree evidence for the other. We might put the matter thus. Either your evidence makes it highly probable that there are unexamined S's or not. If so, it is difficult to see what evidence could make it highly probable that there are unexamined S's and leave it highly improbable that they are SP's, when all the examined S's were SP's. If, on the other hand, there is no strong reason to believe that there are many unexamined S's, there is no strong reason for putting the probability that all S's are P very low, for there is no good reason to think that m is very small as compared with n in the fraction $\frac{m+1}{n+1}$. (It must be understood that at present I am only using general arguments, which must be taken as *illustrating* the way in which the assumption of kinds might affect the theory of induction, and not as *proving* anything conclusively. We shall have to consider the whole question in much greater detail when we have learnt more about kinds.)

(b) To consider only the number of the observed S's is to neglect part of the evidence. We have also to remember that to be called 'an S' at all an unobserved object has to resemble in most of its properties those objects which were observed and were P. Hence an argument by simple

enumeration is always also an argument by analogy, and, *ex hypothesi*, the analogy is very strong or the unobserved case does not count as an instance for or against the law about S's.

7.

We see then that any actual induction about the properties of substances involves at least two presuppositions beside the numerical and other data of the argument, *viz.* (a) that we are dealing with *substances* and (b) that there are *natural kinds* of substances. Anything that is involved in these two assumptions may therefore fairly be regarded as part of the actual premises or principles of such inductions. We must therefore see what these two assumptions really do amount to, and afterwards what evidence there is for them. We shall find that, as regards evidence, (a) and (b) are entangled with each other and with induction by simple enumeration in a highly complicated way. But we must begin by treating them separately.

(a) *The Assumption of Substances.*—When we call a swan a substance we imply that it is something that persists at least for a time; is distinguishable from other swans and from other things which coexist with it; and that, in spite of changes, we can in theory at least identify it as it is at one moment with itself as it was at other moments. A persistent, changeable, and yet identifiable substance is always *at least* a series of states having certain relations to each other and certain properties common to them all. It may be something more than this, but I do not think that it need be so. By a state of a thing I mean a momentary particular which is one of the whole series of related particulars constituting the thing. A state is thus a 'substance' in the logical sense of being a particular and not a universal, though not in the physical sense which involves persistence and identity through change. When I call these states 'momentary' I do not wish to tie myself down either to the view that they have no duration or to the other view that each lasts for a very short time, characteristic perhaps of the series in which they occur. For our present purpose the difference is not of much importance. When I say that θ is a state of the substance Θ I therefore mean that θ is a particular which is momentary in a loose sense and is one of a series of momentary particulars $\theta_1, \theta_2 \dots$ which have the sort of common properties and mutual relations which entitle such a series to be called a substance. (This view is

to be distinguished from the assertion that 'things are *classes* of their states'; it says that things are *complexes* of their states and complexes of a very special kind. To illustrate by an analogy: My face is a complex in which my features are elements; it is not the class of my features.)

To say that Θ persists up to the time t means that there are θ 's fulfilling those conditions up to that time. To say that it then ceases to exist means that after then there are no θ 's which have the right amount in common or the right kind of relations with those of the series $\theta_1, \theta_2 \dots$ which existed before t and were the states of Θ . To say that Θ persists but changes at t means that there are θ 's which exist after t and have enough similarity to and continuity with those which exist before t to be counted as states of the same thing Θ , but that the last to be observed of the latter and the first to be observed of the former differ from each other in some 'first-order property'. By a 'first-order property' I mean a singular proposition ascribing a 'lowest quality' to a definite particular state, or asserting a 'lowest relation' between two or more definite states. I use the phrases 'lowest quality' and 'lowest relation' by analogy to the phrase *infima species*. I should not call colour, or even red, a 'lowest quality', but only a perfectly definite shade of red with definite intensity and saturation. In fact a lowest quality is universal in that it can have a plurality of instances; but these instances must be particulars. Similar explanations apply to the phrase 'lowest relation'.

The next point to notice is that all properties of *things* are at least 'second-order properties'. By a 'second-order property' I mean the assertion that a propositional function whose particular values are first-order properties gives true propositions for all, some, or certain values of the variable. Now it is evident that a great many properties of things are assertions about their characteristic ways of behaving. They thus assert how the first-order properties of one state will differ from those of an earlier state under given circumstances. Evidently such assertions are at least second-order properties. But this is equally true about what are called 'permanent properties' of things, though the fact is here less obvious. When you say that this penny retains its mass through all physical and chemical changes you are saying that for all values of θ , such that θ belongs to the series of states Θ constituting this penny, the function ' θ has the mass m ' gives a true proposition. The permanence of an attribute is thus only a rather special and peculiar mode of behaviour, and the persistent properties of substances are of at least the

second order just as much as assertions about their characteristic ways of changing.

8.

Doubtless permanence in this sense is the earliest and most striking feature which is chosen as a criterion to judge whether a state belongs to a series constituting a thing. Many series do continue in our experience for long periods with scarcely any serious variation in their first-order properties from one state to another. But even such series, which uneducated common sense regards without hesitation as constituting persistent things, have long gaps as far as our experience is concerned. While our attention is otherwise occupied those series may continue, but we certainly have no direct evidence that they do. How does common sense fill in such gaps? Suppose we are aware of a series of very similar states which we regard as the thing Θ_1 ; suppose that there is then a gap in our experience and that we then meet with no more states of this kind for a time. Lastly suppose that we again meet with a series which we can regard as a thing Θ_2 , and that the states of Θ_2 are as similar to those of Θ_1 as those of Θ_1 are to each other. Under what circumstances do we regard Θ_1 and Θ_2 as the same thing? (a) We may find that whenever we choose to adjust our bodies as they were adjusted when we perceived Θ_1 we are aware of a state θ as like those of Θ_1 as the latter are to each other. Under these circumstances we should say that Θ_1 persisted and was the same as Θ_2 . (b) On the contrary we may of course find that a change of bodily adjustment is needed in order to perceive Θ_2 , and that we can only become aware of a θ whenever we choose, provided we suitably alter the adjustment of our bodies. In such cases we tend most strongly to identify Θ_2 with Θ_1 and to hold that Θ_1 has really persisted through the gap in our experience, provided that we find that in order to become aware of θ 's intermediate between the end of Θ_1 and the beginning of Θ_2 an intermediate amount of adjustment is needed between that which was required to be aware of the last θ in Θ_1 and that required to be aware of the first θ in Θ_2 . The point here then is that you can perceive a θ of the right sort at any point in the gap if you will make the right bodily adjustments, and that the right bodily adjustments for success at various points in the gap from a continuous series between those which are successful at the beginning and those which are successful at the end.

We thus see that an important criterion for the persistence of a thing Θ is the belief that *whenever* we choose to perform certain actions we shall observe a particular θ which is so connected with the θ 's that actually are observed as to count as a state of the same thing. Now what evidence can I have for this belief in the case of some definite thing Θ which has ceased to be under my observation for a certain ten minutes? Clearly I cannot know by direct observation of Θ that if I do the right things in the ten minutes' interval I shall perceive a θ which can be taken as a state of *it*. For, by hypothesis, I do not do the right things, and do not become aware of any such states within this interval; this is implied by saying that Θ ceases to be under my observation during that ten minutes. My only evidence (apart from the testimony of others, which is often lacking) is the behaviour of *other* things of the *same kind* as Θ on other occasions. Suppose, *e.g.*, that I observed a certain state θ_1 at the beginning of the ten minutes, and that at the end of it I began to observe a certain state θ_2 . By hypothesis I have observed no intermediate states of this particular Θ . But I may have observed other Θ 's at other times. I may have observed one of them for two minutes after it reached a state like θ_1 , another for five minutes, another for seven, and so on. I may even have observed a Θ for a complete ten minutes after it attained a state like θ_1 and I may have found that it then reached a state like θ_2 . Thus my evidence for supposing that at a given moment in an interval during which Θ was not under observation I should have observed a certain state θ_m if I had done certain things is that I or others actually have observed a state like θ_m at a corresponding period in the history of some other Θ which was under observation.)

We thus see that the logical relations between substances, natural kinds, and induction are extremely complex. (i) Obviously the assumption of kinds of substances involves the assumption of substances. But (ii) we should have very little evidence for the persistence of a given substance if it were not for the fact that other substances of the *same kind* are observable when it ceases to be under observation. (iii) Inductions about the properties of substances are not plausible unless those substances are supposed to belong to a natural kind. Yet (iv) the evidence for the persistence of an unobserved substance from that of others of the same kind is itself inductive. (I do not of course suggest for a moment that people actually reach the belief that their table continues to exist when everyone goes out of the room by inductive arguments from the behaviour of observed tables. They do

not *reach* such beliefs by argument at all, any more than they argue to the existence of physical objects from their sense-data or to that of other minds from the behaviour of other bodies. But, if their belief in the persistence of a given substance were challenged, the only grounds that they could offer would be inductive arguments from other substances of the same kind which had remained under observation.

It will now be wise to discuss the assumption of kinds, since we see that it is closely connected with the persistence of substances and it is part of the definition of a substance to be a more or less persistent series of states.

9.

(b) *Assumption of Kinds*.—If we consider all the momentary states of all the material things which we have met, we find that, though infinitely various, they ring the changes on a comparatively few variables. States differ from each other in colour, sound, taste, smell, temperature, shape, size, etc. But they agree in being determined by one or more of these variables and by some special values of them. Let us call the various sensible qualities—colour, sound, temperature, ‘feel,’ smell, taste, etc.—*primary variables*. The above list is practically exhaustive as far as human beings are concerned. I have excluded shape and size from the list for reasons which will appear in a moment. Each of these primary variables has a comparatively small number of *dimensions*, as I will call them. *E.g.*, the dimensions of sound are pitch, loudness, and quality. Dimensions are specifications of a primary variable, having the following properties: (i) In any definite instance one value of each dimension must be specified; (ii) *A priori* and apart from any special causal laws which may be found to hold in this particular world any value of one dimension may coexist with any value of any other dimension of the same primary variable. Lastly each dimension of each primary variable is susceptible of a range of possible values which is sensibly continuous.

The position of spatial properties is unique and peculiar. We cannot treat shape and size as themselves dimensions, for they cut across the primary variables; *e.g.*, a patch of colour and a patch of temperature both have shape and size. On the other hand we cannot treat shape and size as primary variables. For it is of the essence of primary variables to be antecedently independent of each other. There is, *e.g.*, no synthetic, *a priori* proposition asserting that colour must be accompanied by temperature or temperature by ‘feel’ (in the

sense of hardness or softness), even though some such propositions should be found to be true in the actual world. Now there are *a priori* connexions between spatial attributes and primary variables. All instances of colour and temperature and 'feel' at least have some shape and size. And all instances of shape and size are also instances of *some* primary variable, *e.g.*, colour or temperature or 'feel'. We may say then that as regards any given primary variable extension behaves like a dimension, *i.e.*, it must be specified to determine any particular instance. But, unlike a genuine dimension, it is not tied down to any one primary variable. Finally extension in itself of course has dimensions in the strict sense.

Now any momentary state is completely specified when we are given (a) the primary variables, (b) the values of each dimension of each variable, and (c) the extension of the determinate value of each primary variable. The sum total of all antecedently possible combinations of values of this kind would give all the antecedently possible sorts of states at a moment. Any one of these sorts of states might, so far as we can see, have any number of instances. The only antecedent restriction on the number is that two precisely similar states will not count as distinct if they completely overlap each other in space. Now antecedently there seems no reason why any one of the possible sorts of states should be represented in nature by more instances than any other. We might therefore have reasonably expected to find at any moment the whole multiply-continuous series of possible sorts of states about equally represented in the existent world. But our actual experience of the world has been utterly and flagrantly contrary to this expectation. What we have found is not a regular distribution of all the states at a moment among all the possible sorts of states, but a "bunching together" of instances in the neighbourhood of certain sorts of states. Intermediate possible sorts are scarcely represented in nature, so far as our experience has gone, at all.

Suppose, *e.g.*, that there are N primary variables. Then of course there are NC_r possible r -fold combinations of them, and the total number of combinations of all orders will be $2^N - 1$. Now let us confine our attention to any one of the NC_r r -fold combinations of primary variables. Each of the r variables will have a finite number of dimensions, and between them they will possess a number of dimensions which may be represented by pr , where p is a positive integer in general greater than 1. Imagine now a pr -dimensional space formed with one dimension of one of the r variables for

each of its axes. Then, setting aside the characteristics of shape and size which, as we have seen, are also needed completely to specify a possible sort of state, we may say that each point in this space represents a possible sort of state defined by this particular selection of r out of the N primary variables. Now suppose that a fluid were distributed throughout this space in such a way that its density at any point represents the number of instances in the world of the sort of state represented by the point. Let us further suppose that the density of the fluid at a point were represented by the blackness of a dot made at that point. Then antecedently to experience we might expect this space to be uniformly shaded. But in actual fact, so far as our experience has gone, we have found a quite different arrangement. We should find a number of blobs in the space surrounding certain points. These blobs would be very dark near their centres and would shade off very quickly in all directions as we moved away from these centres. In the regions between the blobs there would be practically no dots at all, and such as there were would be extremely faint. And lastly the whole set of blobs would be confined within a region defined by moderate values of the variables.

10.

This sort of distribution corresponds to what is meant by natural kinds. A natural kind is a region containing a blob. To drop metaphors, a natural kind of state is a sort which has a predominantly large number of instances in nature and such that the number of instances of neighbouring sorts of states falls away quickly in every direction. The sort which has the maximum number of instances (and in our spatial picture is the mean point and the blackest of a blob) is the *type* of the kind in question. Any particular instance of it, or of its adjacent sorts counts as a state of the kind. A kind of substance, is, to a first approximation, a series of states all of a kind, and possessed of the sort of continuity and relations which make them one substance. (I say to a first approximation, because, as we shall see later, characteristic modes of change are as typical of kinds of substances as constancy of kind throughout a series of states.)

The net result then is that, even to a superficial observer, the distribution of states at a given moment is about as far removed as it could be from what is antecedently most probable, and that this mode of distribution shows no sign of becoming more uniform when we take all the moments of human experience together.

Now either this habit of heaping instances round a comparatively few possible states is typical of nature as a whole or it is not. If it is not we have to explain as best we can why it has been characteristic of nature so far as it has come under the notice of human beings. Supposing, for the sake of argument, that nature as a whole really distributes its instances uniformly among possible sorts we shall have to go on to assume that the position of the human race is in some way wildly abnormal so that the parts of nature which have fallen under its observation have been utterly non-typical of the whole. What would this assumption amount to?

It might mean either that the human race had been confined to a section of the universe in which the distribution of instances is excessively unlike their distribution over nature as a whole, and that this exaggeration in our part of the universe is corrected by complementary exaggerations in other parts. Or it might mean that, even within the part that has fallen under our observation, the distribution of instances is really pretty uniform, but that limitations in our perceptive powers or in our interests have prevented us from noticing all but the instances of a few possible sorts. In the end both alternatives depend on supposed limitations of our powers of perception. The second explicitly does so. The first, on further consideration, is easily seen to do likewise. The only importance of space and time for the inductive problem is that they impose limitations on what we can directly observe, and hence at the same time provide the motives and limit the data for inductive arguments. I cannot directly observe what is very remote in space or what happened before I was born, nor can I now directly observe anything that is going to happen later unless I chance to be a prophet.

11.

Now the lack of uniformity in the distribution of instances *within* the region to which I have been confined by spatio-temporal limitations certainly cannot be explained wholly by limitations of my interests and powers of perception. No doubt if the values of primary variables be above or below certain limits I cannot observe them. No doubt, too, there may be many variables that cannot fall under my observation because I lack the needful sense-organs. But this will not account for my failing to observe instances of sorts which fall between the sorts of which I do observe instances. The fact that I occasionally do observe instances of these sorts (*viz.*, 'monsters' in an extended sense of the word) shows

that their rarity in my experience cannot be explained by supposing that they are really present in large numbers but are unobservable to me. Again, while it is true that I often slur over minor differences and treat instances as exactly alike when they are only rather similar, it is certainly not true that my interest is only excited by similarity and not by difference. The success of Messrs. Barnum and Bailey shows that it is not mere lack of interest for intermediate sorts that makes us ignore them. If, *e.g.*, pig-faced ladies were not *really* rare within the range of our physically possible experience it would be unintelligible why the few who do turn up should be so much more interesting than ladies of the more usual kind. Thus I think we are forced to conclude that that part of nature which falls within the spatio-temporal limits of possible observation really departs very far from a uniform distribution of instances among possible sorts; and that the appearance of departure from uniformity cannot be explained by limitations of our interests or powers of observation.

12.

The second alternative, that the part of the world that has fallen under human observation really does depart widely from uniform distribution but that this is averaged out by the much wider part that has never been observed, is much harder to treat properly. It evidently assumes that there is an unobservable part of nature and that the sole reason why it is unobservable is because we cannot perceive what is very distant in space or part time or what is future in time. This assumption itself has doubtless many implications, but for the moment we will take it as it stands. We may then represent the whole course of nature as contained in a four-dimensional space with three spatial and one temporal axis. We may regard a human observer as a point surrounded by a four-dimensional solid. This solid represents the spatio-temporal limits of his possible perceptions. The human race within historical times will be represented by a big four-dimensional solid composed of such solids. Of course the solids will not exclude each other wholly; the centres of one or more will often lie within those of another. Thus the solid will be rather like a mass of bubbles made by blowing through a pipe into soapy water. The limits of this solid will be those of possible human observations within the period for which human history has lasted. Now either (*a*) we may neglect the fact that the human race arose from definite causes in a

definite part of the universe, or (b) we may take it into consideration. Let us first neglect it.

Then antecedently we can regard this solid representing possible human experience as shot at random into the space representing the whole course of the universe, *i.e.*, we have no ground antecedently for thinking that it is more likely to fall in one part of the course of nature than in any other part of the same shape and duration. The actual content of human experience will be represented by the content of the part of the whole four-dimensional space into which the four-dimensional solid happens to fall. Now if the heaping of individuals about kinds be a peculiarity of a small section of the universe, whilst elsewhere the distribution is nearly uniform, it is highly unlikely that human observers will have happened to fall just into this part of the universe. The larger we suppose the universe to be compared with the part of it which has this peculiarity the less likely it is antecedently that the solid representing the limits of human experience should have fallen totally inside this peculiar region. Really we have three four-dimensional volumes to compare: (a) that representing the whole course of nature, (b) that of the solid representing the spatio-temporal limits of historical human observation, and (c) that of the supposed exceptional region within which a discontinuous distribution of individuals about a few natural kinds is supposed to hold. Unless (c) be very small compared with (a) we cannot be very far wrong in extending the characteristics of what we have observed to the whole universe. On the other hand if (c) be very small compared with (a) it is very unlikely that (b) when thrown at random into (a) should fall wholly inside (c). And it is obviously more and more unlikely the nearer (b) approaches in volume to (c). Now it is only if the general course of nature changes soon after the spatio-temporal limits of our present experience are surpassed that the inductive extension of the general characteristics of what we have observed will soon lead us wrong. That is, such an inductive extension will be *practically* harmless *unless* (b) nearly approaches in volume to (c); and we have just seen that *if* (b) nearly approaches (c) the fact that (b) has wholly fallen inside (c) is an extraordinary coincidence which renders the existence of the supposed exceptional region (c) highly improbable.

13.

But it will no doubt be objected at once that all this talk about the human race being 'shot at random' into the

universe like a sack of coals into a cellar is the merest nonsense. It actually did arise at a certain moment in certain parts of space where the right conditions were fulfilled and has gone on ever since. Hence its range of experience cannot be compared to a *movable* solid which might have fallen anywhere in the universe. Now these statements may very well be true—I suppose that we all believe that they are true—but are they relevant? What is a person who makes them assuming? He is assuming that he can write a hypothetical history of the origin of human observers. Now this means that he supposes himself to know (a) that certain conditions held before human observation began, and (b) that these conditions, operating according to certain laws, were necessary (if not sufficient) for the production and continuance of life and mind as we know them. He thus claims a knowledge of what existed outside the range of human observation and of the laws that it obeys. His only ground for this must be the belief that he is justified in extending the characteristics of the part of the world that has fallen under human observation to parts of it which, by hypothesis, cannot have done so.

The logical position therefore seems to be this. Either we know that the general characteristics of nature which we have observed (confinement of instances to kinds, regularities of behaviour, etc.), are equally characteristic of the parts of nature which we have not observed or not. If so, then it is doubtless nonsense to talk of the human race and its observations being as likely to fall in one part of the total course of nature as in another, and our previous argument will be useless. But then it will also be needless. For anyone who supposes himself to have this knowledge supposes himself to know that the part of nature that has fallen under observation is not peculiar in its general (and even in some of its more special) characteristics. If, on the other hand, we entertain a doubt whether the general characteristics of the observed part of nature hold of the unobserved parts we *ipso facto* leave open the possibility that these unobserved parts are subject to no special laws and do not confine instances to kinds. Now relative to that possibility it is not nonsense to talk of the actual position of the human race in the course of nature as a whole as a random position. And what we have argued is that the hypothesis that we are in a singular region of nature tends to undermine itself because it is highly improbable that the whole course of human experience should fall (as it has done) into what on the hypothesis itself is a small exceptional region of the universe.

It must be noticed that this argument only applies at all strongly to the *general* characteristics observable in the part of the universe that has fallen under observation. It would be very extraordinary that, if only a small part of the course of nature confined its instances to kinds and its changes to regular rules whilst the rest of it did nothing of the sort, human experience should have happened to fall wholly within that small region. But it would not be at all extraordinary if in other parts of nature certain kinds which are predominant with us are not represented and conversely. In fact it is obvious that our experience makes it much more probable that the *general* characteristic of confinement to kinds extends widely beyond its limits than that the more *special* characteristic of favouring such and such kinds is widely extended. For the more special proposition implies the more general and not conversely; so that whatever is in favour of the former is in favour of the latter, but there may be evidence for the latter which has no special relevance to the former.

14.

Extension of Theory of Kinds.—So far we have argued that, even to a superficial observer, nature appears not to distribute its instances equally among possible sorts, and that it is reasonable to regard this general characteristic as probably extending much beyond the limits of human experience. But, to a superficial observer, confinement to kinds, though a striking characteristic of the observed part of nature, is by no means an universal rule within this part. In the first place there are occasional ‘monsters’. Then again the contemporary states of various substances which would be counted as of one kind are never exactly alike. *E.g.*, the swans or crows that exist at any moment all differ more or less in their first-order properties. Again, if instead of thus taking a cross-section at a given moment, we consider the series of states constituting a given substance, they differ from each other in many first-order properties. And a point may be reached at which either the series stops altogether and the substance is said to have ceased; or else the first-order properties may change so radically whilst certain conditions of spatio-temporal continuity are still fulfilled that the substance is said to have ‘changed into’ one of another *kind*. There can be no doubt, I think, that the face of nature does present these aspects to all of us whilst we are still ‘trailing clouds of glory behind us,’ and that it continues to do so to many until the end of our lives.

Now at this stage there enters a characteristic habit of the human mind which has constantly operated with highly useful effects in the history of science. We draw a distinction between the superficial appearances of things and their more detailed and latent character. A contemplation even of the superficial aspects has strongly suggested to us some general rule, but there are a certain number of apparent exceptions. We then tend to proceed on the assumption that this general rule really is true without exception when the latent parts of nature are taken into consideration, and that the apparent exceptions can be explained compatibly with this view. Then we make more careful investigations with this idea as our guide, and we find that in a great number of cases the more accurately analysed and observed facts support the assumption. If this be so we tend finally to take the rule as a principle and to assume that any small residuum of obstinate facts which apparently refuse to come under it only *appear* exceptional because we have so far failed to find the right way of analysing or observing them.

I imagine that this is what M. Poincaré had in mind when he talked of laws being raised to the rank of 'principles' and then being 'true by convention' and 'beyond the attacks of experience'. It is important for us to consider the logical position of this habit. (i) In the first place we suppose that the law is strongly suggested to us by superficial observation. Now the law that all things are instances of kinds is quite as strongly suggested to us by observation as (say) the law that bodies continue to move uniformly in straight lines except for the action of other bodies. (ii) Our everyday experience has given us every reason to draw a distinction between things as they appear at first sight and things as they appear on closer inspection. Since things exhibit fresh details to us the more closely we observe them it is perfectly reasonable to suppose that they contain parts and details that we cannot observe at all. And, since the details that closer observation reveals are often found to be more important than those which were observable on a more superficial view, it is not unreasonable to think that the details which cannot be directly observed at all may be more important than any that can be observed. (iii) We have plenty of experience both of substances coalescing and of their separating; we know that the coalescence of two substances of the same kind generally gives a substance of that kind; that the coalescence of two of a different kind often gives one with different characteristic properties from either; and that sometimes when a substance splits up it does so into several of the same kind as itself and

sometimes into substances of different kinds. Now all these facts, which are common enough when we examine the world at all carefully, help to make the theory of kinds, which is so strongly suggested but not wholly confirmed by superficial experience, more and more definite and rigid.

The notion of compounds and mixtures which differ markedly in their superficial properties from their components is suggested by experience of actually mixing and separating substances. Once suggested and recognised as a fact in the region of nature with which we have dealt, it enables us to hold that those things which are not on the face of them instances of kinds may yet be mixtures or compounds of things which are genuine instances of kinds. Thus one exception to a rigid theory of kinds (*viz.*, the existence of things of intermediate sorts) is removed by following out a suggestion which is (a) made plausible by our experience so far as it has gone, and (b) which that experience in its gradual development suggests to be extensible beyond the limit reached at any given moment by actual observation. But we cannot stop here, for we are still left with the fact that contemporary instances of the same kind that have actually fallen under our observation are not exactly alike, and that the successive states of what we regard as a single substance of a kind may differ seriously from each other. It is in connexion with these problems, I am inclined to think, that the notion of causation and of conditions becomes prominent.

15.

Kinds, Substances, and Causation.—We here meet again that irritating interweaving of various fundamental notions which we have already had occasion to notice and which makes it so difficult to treat the subject in any satisfactory logical order. Causal laws refer to the states of substances and special causal laws to the behaviour of special kinds of substances. But on the other hand, as we shall see, the definition of a kind of substances partly depend on the causal laws which substances of the kind are supposed to obey. And the identity of a substance of a kind may itself be defined by the fact that the states possesses certain properties which figure in some special way in a causal law. Let me illustrate before going further. Silver is a kind of substance, and the superficial marks of the kind are certain physical properties like colour, hardness, specific gravity, etc. Yet the vast majority of the silver in the world at any

moment is not represented by states with any of these properties ; since most of it exists in chemical compounds of various sorts. A chemist in stating what he meant by silver would hardly trouble to mention these first-order properties. What he would do would be to mention how silver reacts under various conditions with various other substances. And he would count the characteristic properties of the various compounds of silver as much more distinctly characteristic of silver than the superficial properties of the metal itself. Thus when he talks of the characteristic properties of the kind of substance called *silver* he scorns to give us a mere enumeration of first-order properties, because he knows that these are constantly changing and that if he confined himself to them it would hardly be plausible to count silver as a kind at all. Instead he gives us second or higher-order properties, *i.e.*, statements of the characteristic mode of variation of the first-order properties under given conditions. Thus the characteristic marks of a kind involve conditions and causation. On the other hand all these higher-order properties themselves involve a reference to kinds of substances. They include statements as to what silver does in presence of chlorine, in presence of sulphur, and so on. Yet again these other kinds are themselves mainly recognised and defined by what substances belonging to them do in presence of other kinds of substances. If it is part of the 'definition' of silver that it is the kind of substance which gives a white insoluble compound with chlorine, it is equally part of the 'definition' of chlorine that it is the kind of substance that gives a white insoluble compound with silver. Lastly, when the chemist states all these second-order properties of silver he does not profess to be announcing merely analytical propositions; they cannot therefore be part of the meaning of *silver*, which must therefore be assumed to be known before the propositions are asserted: How are all these tangles and apparent circles to be straightened out?

I take it that the solution is somewhat as follows. The notion of silver as a kind of substance was first suggested by bits of metallic silver seen and touched under certain 'normal' conditions of illumination, etc. These first-order properties continued much the same through long series of states which had the sort of continuity with each other that constitutes them states of one thing. They were taken as the original definition of silver. But silver, defined in this way, is continually ceasing to exist as circumstances change. It is found however that when a 'silver series' stops and

is replaced (say) by a 'silver chloride series' certain regularities of mass hold between the two series, and under suitable conditions the 'silver chloride series' can be stopped and replaced once more by a 'silver series' in the old sense of silver. The mass of each state of this second silver series is the same as that of the first silver series. This identity of mass and of other first-order properties, the spatio-temporal continuity of the two silver series by the intermediation of the silver chloride series, and the regularity with which the silver series passes into a silver chloride series under one set of conditions and conversely under another, enable us to identify the first silver and the second. And these facts are summed up in the statement that the silver continued to exist throughout the silver chloride series in spite of appearances to the contrary. Now regularities of precisely the same kind hold for sulphur, chlorine, etc., defined originally by certain superficial first-order properties which persist under 'normal conditions'.

16.

We thus arrive at a distinction of kinds into kinds of the first, of the second, and (as we shall see in a moment) of higher orders. Kinds of the second order (chemical compounds) are true kinds in the sense in which we have all along been using the word. But the instances of them begin and cease in the course of history. This always happens, so far as our experience goes, by the coming together or separation of instances of kinds of the first order (chemical elements). Instances of kinds of the first order are taken to be persistent and not to have begun or ceased in the course of human experience. And this view is held in spite of the fact that such instances are constantly disappearing and apparently coming to an end; for, after all, chemical elements are much less common and less stable than chemical compounds. The explanation of this apparent paradox is however quite simple after what has been said above. The kinds which are so noticeable even on the most superficial view of the world are mostly of the second or third order. Swans, crows, etc., are kinds of the third order; for they consist of instances of certain kinds of the second order in certain characteristic proportions, arrangements, and extensions, about which they vary within narrow limits. The main reason why these are the kinds that strike us is their comparative stability. By this I mean that each instance of such kinds consists of a series of states with first-order

properties which vary very little even though conditions change a good deal. This is of course less true of kinds of the third order than of many of the second, for crows and swans die and decay, but many chemical compounds are intensely stable towards quite enormous changes in conditions. We can see then why it is kinds of the higher orders which first attract our attention and suggest to us the notion that confinement of instances to kinds is a general characteristic of nature, and that if we look more carefully we shall find that it is a rigidly general rule in spite of superficial appearances to the contrary. But, when we do investigate more closely, we find that these kinds which first struck our attention are not as a rule the most important kinds in nature. *E.g.*, silver chloride, as defined by its common physical properties, is an extremely stable kind; *i.e.*, these properties persist through long series of states under highly variable conditions. Compared with it silver, as defined by its common physical properties, is an unstable kind, for it is constantly tarnishing, dissolving, reacting, and so on. But under certain conditions a silver chloride series does wholly change its first-order properties and is succeeded by a silver and a chlorine series. Now we have no ground for saying that the silver chloride really persists after the change; for, if it does, does it do so in the silver series or in the chlorine series? It seems arbitrary to choose either. Again the mass of the silver chloride is now divided between the two series, and no silver chloride can be got from any one of them till either the other itself or an equal mass of some different sample of it is added to the first. We thus can attach a definite meaning to the statement that bits of silver and masses of chlorine persist in spite of appearances to the contrary; but, when we define persistence in this way, we have to deny that a bit of silver chloride persists when a silver chloride series ceases to show its defining first-order properties. Thus we reach the notion of first-order kinds and see that they are more important though less obvious superficially than those of higher orders.

At this stage the extremely peculiar character of the part of nature that has fallen within human experience becomes still more marked. For we find that every bit of matter that we come across can be regarded as either an instance of a kind of some order or as a mixture of instances of various kinds, and that the number of distinct first-order kinds is ridiculously small. We admit of course that there may be first-order kinds that we have never met with, and that what we take to be a first-order kind, may prove to be of a higher

order. But we do seem to have hit on the general ground-plan of the material world, however inadequate may be our knowledge of the details. And that ground-plan, suggested to us even by a superficial observation of nature, has shown itself to be capable of statement in a more and more rigid and exacting form as we have investigated nature more and more carefully.

17.

We have now seen that many of the most interesting properties of kinds of substances are not assertions about the *persistence* of the first-order properties of states of a series, but assertions about the ways in which such properties vary from state to state of a series with varying conditions. However Irish it may sound, it is true to say that the most important properties of first-order kinds are properties of second-order kinds. This of course simply means that, *e.g.*, the most important properties of silver are not the superficial physical properties of metallic silver, but are statements of the conditions under which metallic silver turns into such and such compounds and the conditions under which such and such compounds again give metallic silver. Now the identification of 'such and such' a compound of silver (*e.g.*, silver chloride) can only be made by mentioning enough of its properties to characterise it unambiguously. Thus it is true that most statements about first-order kinds are statements about the properties of the second-order kinds into and out of which they pass under given conditions.

Again, it is probably true that we should not have troubled much about conditions if it had not been for the changes in first-order properties that occur along a series of states regarded as constituting a thing. If first-order properties had all been highly persistent with varying conditions we should probably not have noticed that they depend on conditions at all. But, as it is, the variations in many series of states having thinghood force the notion of conditions on our attention, and then we come to see that even persistence of first-order properties depends on conditions and is only relative. Change the conditions enough and the most persistent first-order properties will begin to vary.

Now I am inclined to think that the notion of causation and conditions is best regarded as an attempt to reconstruct at a higher level the crude notion of things which has broken down on reflexion and minuter observation. I think that we shall see this clearly if we consider what is commonly

believed in practice about causal laws and the Law of Causation. In the first place it is always changes that are felt to need explanation, *i.e.*, if the series of states constituting a thing varies from state to state in first-order properties we are not inclined to accept this as an ultimate fact. Parallel with this, but less often explicitly noticed, is another fact. We find instances of the same kind coexisting at different places in space. Though we count them of the same kind the contemporary states of several of them will not as a rule be exactly alike. All crows are instances of a kind, but at every moment there are small differences between one crow and another. This is felt to demand some explanation. The cause of demands such as this should now be fairly obvious. Our original criterion of the persistence of a given thing was identity of first-order properties throughout a series of states possessed of spatio-temporal continuity with each other. In so far as the first-order properties vary throughout such a series the series departs from the standard of a persistent thing. Hence the need of an explanation for *changes* and the absence of need for an explanation of *persistence* is the need to reconcile a contradiction. We are determined (a) to go on talking of *this* thing and saying that *it* persists; indeed this is implied by calling the change a change in *it*. But (b) our original criterion of identity uses persistence of first-order properties. The need for explanation of change is the need for a less simple-minded criterion of one thing and of the persistence of a thing, which shall be compatible with both change and identity.

Again our ideal kind, suggested to us but never wholly exemplified in the world as we have found it, would have a large number of *exactly* similar instances. Actually we find large numbers of *very* similar but partly different states coexisting in various parts of space. Our demand for explanation is the demand to be allowed in some way to keep our notion of kinds as possessing *exactly* similar instances and yet to admit that the contemporary instances very rarely are exactly alike.

18.

These two closely connected demands are, I think, to be regarded as being in the strictest sense postulates and not axioms. They set us a problem, but there is no guarantee *à priori* that it will be soluble. What I mean is that it is not in the least self-evident that the universe *must* respond to our demand for permanent substances and for ideal kinds in some new sense of permanence and of kind, when it has failed to

answer completely to our original criterion. The actual fact seems to be this. The world as it presents itself to superficial observation fulfils to a highly surprising extent the condition of consisting of permanent substances of a few marked kinds. It fulfils this still better when we investigate more closely. But it does not fulfil it altogether. The position is that it fulfils it so well as to raise the expectation that a modification of the definition of permanence and of kinds, which shall be in the spirit of the original definitions, can be found, and that with this definition the universe will *strictly* consist of permanent substances belonging to a few ideal kinds. I am prepared to believe, if anyone can produce satisfactory evidence, that this expectation, in a crude form at least, is innate. This is of no logical importance, however; the really important point is that it is not *a priori*, that it is perfectly conceivable that the universe might not answer to these demands and that no such amended definitions that might be suggested would help us.

Now it will be found that the Law of Causation, as actually used, is such that if it be true the world does consist of permanent substances of a few ideal kinds, in a perfectly reasonable sense of *permanence* and *kind* which is only an extension of our original senses of these words. The Law of Causation says that every event has a cause. It refers to definite particular events and to each one ascribes another definite event or set of them as *its* cause. What then is meant by *a cause*? Evidently it has something to do with causal laws, but the precise connexion is not at first obvious. Causal laws, even in their crudest form, connect, not definite particular events, but classes of abstract events. For they imply the possibility of recurrence under varying conditions and at different times and places. Even the crudest sort of causal law is doubly abstract; it takes the form: Whenever an event of the sort η happens to a substance of the sort a an event of the sort η^1 follows after a certain lapse of time t in a substance of the sort a^1 . Of course as a particular case η^1 and η may be the same kind of event, a and a^1 may be the same kind of substance, and the two events may happen in the same substance. Again, of course, the antecedent in a causal law may be several abstract events in substances of several kinds; and these events may not be contemporary with each other. The same is true of the consequent. But in any case the important point for us to notice is (a) that the antecedent and the consequent in any causal law are doubly abstract and (b) that the Law of Causation, on the contrary, is an assertion about definite events in definite

substances. To use a phrase employed by Mr. Russell in *Principles of Mathematics* the Law of Causation deals with 'the causation of particulars by particulars'; and we have to reconcile this with the fact that no causal law deals with particulars at all.

The way to reconcile the two facts is as follows. We assume that any definite particular event can be unambiguously *described* by mentioning a finite number of abstract characteristics. These together tie us down to one definite substance or set of substances and to one definite event or set of definite events in these substances. Each of the characteristics used in the description is abstract, and, taken by itself, can recur at other times and places and in other substances. Each can therefore be taken (say) as the consequent in some causal law, and the antecedent of each in that causal law will, of course, again be abstract. The further assumption is that these abstract antecedents when taken together will once more suffice to tie us down to a single definite event or to a set of definite events in a single definite substance or set of definite substances. This event or set of events is then *the* cause of the definite event or set of events with which we started.

Thus the Law of Causation, in asserting that every event has a cause, makes the following three assumptions. (i) Every definite event can be unambiguously described by mentioning a finite number of its abstract characteristics. (ii) Either each of these characteristics taken separately, or selections out of them which together exhaust them, are consequents in causal laws. (iii) The antecedents in these causal laws are a set of abstract characteristics which, when taken together, unambiguously describe a definite event or set of events.

19.

We have now seen what the Law of Causation asserts; we can now see how it enables us to extend our definitions of *kind* and of *permanent substance*. The individual instances of a kind (even of a first-order kind) do constantly change their first-order properties, and thus at any moment two instances may be in very different states. But all these changes are subject to laws; these are characteristic of the kind, and they do not change. The permanence of first-order properties and their exact similarity among all instances, which first suggested kinds and permanent things, breaks down; but it is replaced by permanence of laws, *i.e.*, of second

and higher-order properties. Contemporary states do not now cease to be states of substances of the same kind merely because they differ in their first-order properties; for these differences in first-order properties are compatible with, and indeed are the consequence of, identity of higher-order properties combined with the varying external conditions which are implied by differences of place.

Pari passu with this modification of the notion of a kind goes a change in the notion of the permanence of a given thing. In the first place, even though spatio-temporal continuity throughout a series of states be still demanded as a necessary condition of identity, we no longer demand exact similarity of first-order properties. We are content with permanent laws + reversibility. By this I mean that if S be a certain state of a certain substance we do not demand that every state of a series shall be exactly like S in order to count as belonging to the substance; we admit very different states under different conditions; but we do demand that by suitably reversing the conditions any state that has happened in the series can be reproduced. And we assume that when this condition is not fulfilled we are not dealing with an elementary substance, and that all substances which do not fulfil it are compounded of substances which do fulfil it.

I think that we also demand some kind of first-order identity throughout the series, though it may be very slight, and, to superficial observation, very unimportant and obscure. This is why we make so much of all laws of conservation, *e.g.*, the conservation of mass, of energy, of momentum, and so on.

Corresponding to these changes a new notion is introduced side by side with the old notion of things. This is the notion of the causally isolated system. The old single substances of common sense, determined largely by spatial continuity of matter within a limited region still persist, but the notion of the isolated system composed of several such substances separated in space, largely usurps their place. Such a system is one in which all the laws governing the changes of first-order properties throughout the parts refer only to other parts of the same system and to their spatial relations and not to anything outside the system. An isolated system is thus the old single substance in a much modified and purified form. The importance of continuous filling of a boundary has diminished, and the parts are not series of precisely similar states. But, regarding the system as a whole as a substance spread out in space and time, all its variations follow constant rules and none of these rules refer to anything outside itself.

The existence side by side of the new notion of the isolated system and the old criterion of one substance as what fills a certain boundary leads to the distinction between immanent and transeunt causation. The causal laws characteristic of the system are immanent to *it*, as referring to nothing but its parts, but are transeunt to each of its parts, as referring to changes in other parts to account for the changes in any given part.

Complete causal isolation is of course an ideal rather than a fact. What we find is that a system is isolated for certain changes in its parts and for a certain degree of accuracy in accounting for these changes; for other changes and for greater degrees of accuracy different and in general larger systems must be considered. But it is evident that the law of causation would be a useless platitude and that the notions of permanent substance and kind would have broken down beyond hope of salvation if nature were not so constituted that there are systems much smaller than the whole of nature which are for many changes practically isolated.

20.

Let me at length sum up the results of this long, confused, and confusing discussion. All particular inductive arguments depend on probability and only lead to probable conclusions, *whatever* we may assume about nature. But *unless* we assume something about nature they give no finite probability to any law (*a*) because an indefinite number of alternative hypotheses which are not laws are as probable antecedently as the suggested law, and (*b*) because we are not equally likely to have met with any instance of the class under discussion, since it is quite certain that if there be instances remote in space or time they *could* not have fallen into the selection which we observed. What we actually assume is that nature consists of a comparatively few kinds of permanent substances, that their changes are all subject to laws, and that the variety of nature is due to varying combinations of the few elementary substances. These assumptions are neither self-evident nor mutually independent nor are they capable of complete proof or disproof by experience. The actual course of the process by which we reach these assumptions is somewhat as follows. Nature, even as known to us superficially, presents a surprisingly selective appearance. Of sorts of substances which are *a priori* possible and could be perceived if presented only a very small selection is presented, whilst those sorts which we do meet with have

very large numbers of instances. And, to a superficial view even, there are many series of states in nature which have the kind of spatio-temporal continuity which characterises a thing and moreover show practical constancy of first-order properties over long periods of time. Reasons have been given to show that this appearance can hardly be due to limitations of our powers of perception and interest *within* the spatio-temporal field of actual human experience. The view that these characteristics may only be true of a small part of nature into which we happen to have fallen was then discussed. It was argued that, as an objection to the possibility of induction, the argument is unsatisfactory. Either it literally assumes that our connexion with the part of nature with which we are connected is a random one, or that we have arisen here rather than elsewhere because of laws of nature. The latter view *assumes* laws of nature in regions spatio-temporally outside that with which we have come in contact through experience, since the supposed conditions for the origin of human *experiences* cannot themselves have fallen within the region of nature open to direct human experience. If, on the other hand, the view that the human race is as likely to fall into one part of the course of nature as into another be taken literally, we can show that it is highly improbable that the general characteristic of confinement to kinds, which we have noticed, extends but slightly beyond the limits of human experience. We thus seem justified in disregarding the possibility that this characteristic of the experienced world does not extend beyond it, as an argument against induction.

Up to this point, however, we can only say that experience has *suggested* a simple ground-plan of the material world to us, and that it is reasonable to suppose that this plan extends beyond what we have actually experienced. So far we have neither formulated the plan in rigid terms, nor, on the face of it, does nature, even as experienced, completely accord with it. At this stage the distinction between elements and compounds and between the perceptible and imperceptible parts of bodies, a distinction itself suggested by much even in the crudest experience, comes to our help. Pursuing this suggestion we have found it possible to regard nature as built up of a comparatively few natural kinds of the first order, all instances of which are *exactly* alike and *completely* permanent. An analysis of the meaning of kinds and of the permanence of substances has shown us what is the precise 'cash-value' of these statements. It has shown that it is because nature, so far as our experience goes, obeys laws in

its changes, that the criterion of persistence of substances and sameness of kinds, which broke down when we confined ourselves to first-order properties, can be rendered satisfactory by taking into account second and higher-order properties. It follows that it is a fundamental error to take the scientific notion of substance by itself as 'something that any fellow can understand,' and then raise difficulties about the law of causation. The notions of permanent substances, genuine natural kinds, and universal causation are parts of a highly complex and closely interwoven whole and any one of them breaks down hopelessly without the rest.

The upshot of the matter is that whenever we make a particular induction we have this general view about nature at the back of our minds. If we think that we have hold of a substance that is an instance of one of the few fundamental natural kinds, we attach great weight to our induction, otherwise we do not. The logical position is then (a) that those inductions which we regard as highly probable are so relatively to the belief that we really have got hold of the general ground-plan of nature in the region of phenomena under investigation; (b) the evidence for this is never of the nature of a 'knock-down' proof and no numerical probability can be assigned to it. The kind of evidence is that this plan is suggested to us in a rough form by crude experience, and that, as we investigate nature more and more thoroughly, experience itself *suggests* ways in which we can state this plan with greater and greater definiteness and rigour, and, at the same time, nature is found to *accord* with the more rigorous and definite plan far better than it did with the first crude suggestion of a plan. *E.g.*, we believe that we have got very near to the ground-plan of the material world in the theory of chemical elements, in the laws of mechanics, and in Maxwell's equations, and it is relative to these beliefs that particular inductions in chemistry, electricity, etc., are practically certain. The certainty of the most certain inductions is thus relative or hypothetical, and the probability of the hypothesis is not of a kind that can be stated numerically.

21.

I think that the actual history of the natural sciences bears out this view. They flounder about in the dark till some man of genius sees what are the really fundamental factors and the really fundamental structure of the region of phenomena under investigation. In mechanics the keystone is the notion of acceleration; in chemistry it is the theory of

elements and compounds and the conservation of mass; in economics, perhaps, it is the notion of marginal utility. Sciences where no such discovery has yet been made, such, *e.g.*, as psychology and biology are almost at a prescientific level; their inductions carry no great conviction to anyone trained in the more advanced sciences.

At the beginning of the first part of this paper I told the reader that I was extremely doubtful as to the additional principles about nature, which are needed if any law is to be rendered reasonably probable by induction. I have done my best in this second part to indicate the beginnings of an answer to my own question. But I am painfully aware that the article is complex and diffuse without being exhaustive. There is hardly a line in it which I could seriously defend even against myself if I chose to be an hostile critic. But I print it in the knowledge that if I now spend more time I shall only puzzle myself more thoroughly, and in the hope that its very badness may convince the charitable reader at least of the extreme difficulty of the subject.