MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY

I.—ON THE RELATION BETWEEN INDUCTION AND PROBABILITY—(Part I.).

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IN the present paper I propose to try to prove three points, which, if they can be established, are of great importance to the logic of inductive inference. They are (1) that *unless* inductive conclusions be expressed in terms of probability all inductive inference involves a formal fallacy; (2) that the degree of belief which we actually attach to the conclusions of well-established inductions cannot be justified by any known principle of probability, unless some further premise about the physical world be assumed; and (3) that it is extremely difficult to state this premise so that it shall be at once plausible and non-tautologous. I believe that the first two points can be rigorously established without entering in detail into the difficult problem of what it is that probability-fractions actually measure. The third point is more doubtful, and I do not profess to have reached at present any satisfactory view about it.

1.

All inductions, however advanced and complicated they may be, ultimately rest on induction by simple enumeration or on the use of the hypothetical method. We shall see at a later stage the precise connexion between these two methods. In the meanwhile it is sufficient for us to notice that, whilst the inductions of all advanced sciences make great use of deduction, they can never be reduced without residue to that process. In working out the consequences of a scientific hypothesis many natural laws are assumed as already established and much purely deductive reasoning is used. But the evidence for the assumed laws will itself be ultimately inductive, and the use which is made of our deduced conclusions to establish the hypotheses by their agreement with observable facts involves an inductive argument.

Now both induction by simple enumeration and the hypothetical method involve, on the face of them, formal fallacies. The type of argument in the first kind of induction is: All observed S's have been P, therefore all S's whatever will be P. Now the observed S's are not known to be all the S's (indeed they are generally believed not to be all the S's). Hence we are arguing from a premise about *some* S's to a conclusion about *all* S's, and are clearly committing an illicit process of S.

Most inductive logicians of course recognise this fact, but most of them seem to suppose that the fallacy can be avoided by the introduction of an additional premise which they call the Uniformity of Nature or the Law of Causation. They admit that there is a difficulty in stating this principle satisfactorily and in deciding on the nature of the evidence for it, but they seem to feel no doubt that *if* it could be satisfactorily stated and established the apparent formal fallacy in induction by simple enumeration would vanish. It is easy, however, to show that this is a complete mistake. Whatever the supposed principle may be, and however it may be established, it cannot be stronger than an universal proposition. But if an universal proposition be added to our premise, All observed S's are P, the latter premise still remains particular as regards S. And from a universal and a particular premise no universal conclusion can be drawn.

It follows then that no additional premise, whether about logic or about nature, can save induction by simple enumeration from a formal fallacy, so long as the conclusion is in the form all S's are P. If the validity of the process is to be saved at all it can only be saved by modifying the conclusion. It remains of course perfectly possible that some additional premise about nature is *necessary* to justify induction; but it is certain that no such premise is *sufficient* to justify it.

The hypothetical method equally involves, on the face of it, a formal fallacy. The general form of the argument here is: If h be true then $c_1, c_2 \ldots c_n$ must be true. But $c_1, c_2 \ldots c_n$ are all found by observation to be true, hence h is true. This argument of course commits the formal fallacy of asserting the consequent in a hypothetical syllogism. The only additional premise which could validate such an argument would be the proposition: h is the only possible hypothesis which implies $c_1, c_2 \ldots c_n$. But this proposition is never known to be true and is generally known to be false.

The conclusions of inductive argument must therefore be modified, and the most reasonable modification to make is to state them in terms of probability. The advantages of such a course are (*a*) that this accords with what we actually believe when we reflect. We always admit that the opposite of an inductive conclusion remains possible; even when we *say* that such conclusions are certain we only mean that they are so probable that for all practical purposes we can act as if they were certain. That this differs from genuine certainty may be seen if we reflect on the difference in our attitude towards the true propositions, All grass is green and $2 \times 2 = 4$. In ordinary language both would be called 'certain,' but our attitudes towards the two are quite different. No one would care to assert that there might not be in some part of space or time something answering to our definition of grass but having a blue instead of a green colour.

(*b*) With the suggested modification of our conclusion the logical difficulty vanishes. Suppose the conclusion becomes: It is highly probable on the observed data that all S's are P. There is then no illicit process. We argue from a *certain proposition* about *some* S's to the *probability* of a proposition about *all* S's. This is perfectly legitimate. The subject of our conclusion is no longer All S's, but is the proposition All S's are P. The predicate is no longer P, but is the complex predicate 'highly probable with respect to the observed data'.

(*c*) If inductions with their unmodified conclusions were valid forms of inference we should be faced by a strange paradox which furnishes an additional proof that inductive conclusions must be modified. It is often certain that all the observed S's are P. Now what follows from a certain premise by a valid process of reasoning can be asserted by itself as true. Yet we know quite well that, if the conclusion of an inductive argument be All S's are P, the very next observation that we make may prove this conclusion to be false. Hence we have the paradox that, if induction be valid and the conclusion be All S is P, a *certain* premise and a *valid* argument may lead to a *false* conclusion. This paradox is removed if we modify our conclusion to the form: It is highly probable on the observed data that all S is P. Probability and truth-value are both attributes of propositions.

I omit here further subtleties as to whether they do not more properly belong to propositional forms, or, as Russell calls them, functions.) But they are very different attributes. (i.) A proposition is true or false in itself and without regard to its relations to other propositions; a proposition only has probability with respect to others, and it has different probabilities with respect to different sets of data. (ii.) A proposition which is very probable with respect to certain data may be in fact false, and conversely. This is precisely what we mean by 'a strange coincidence'. It follows from these facts that if I have observed n S's and they were all P it may be highly probable relative to these data that all S's are P, and yet it may be false that all S is P. If I observe an $n + 1$th S and it proves not to be P, I know that it is false that all S is P; but this does not alter the truth of the proposition that, relative to my first n observations, it *is* highly probable that all S is P. For the probability of a proposition may be high with respect to one set of data and may be zero with respect to another set which includes the former. Our original inductive conclusion does not cease to be *true*, it only ceases to be practically important.

For all these reasons I hold that we have established the point that inductive *conclusions* must be modified if induction is to be saved and that no additional *premises* will suffice to save it. And I think it almost certain that the direction in which the modification must be made is the one which I have indicated above. Leibniz said in a famous passage that Spinoza would be right if it were not for the monads; we may say that Hume would be right if it were not for the laws of probability. And just as it is doubtful whether Leibniz was right even with the monads, so there remains a grave doubt whether induction can be logically justified even with the laws of probability.

2.

If we accept the view that inductive conclusions are in terms of probability, it is clear that a necessary premise or principle of all inductive argument will be some proposition or propositions concerning probability. Since probability, like truth, implication, etc., is an attribute of propositions, the laws of probability are laws of logic, not of nature, just like the principle of the syllogism or the law of contradiction. That is, they are principles which hold in all possible worlds, and do not depend on the special structure of the world that actually exists. It remains possible however that they are only capable of fruitful application to real problems if the actual world fulfils certain conditions which need not be fulfilled in all possible worlds. *E.g.* $2 \times 2 = 4$ holds in all possible worlds, but it would be very difficult to make any practical use of this proposition in physics if all objects in the actual world were like drops of water and ran together into a single drop when mixed.

To see what the principles of probability required by induction are, and to consider whether they suffice to justify the actual strength of our beliefs in universal propositions about matters of fact, I propose to consider induction by simple enumeration and the hypothetical method in turn.

A. *Induction by Simple Enumeration.*—The way in which I propose to treat this problem is as follows. I shall first consider the logical principles employed and the factual assumptions made when we draw counters out of a bag, and, finding that all which we have drawn are white, argue to the probability of the proposition that all in the bag are white. I shall then discuss as carefully as I can the analogies and differences between this artificial case and the attempt to establish laws of nature by induction by simple enumeration. We shall then be able to see whether an alleged law of nature can logically acquire a high degree of probability by this method, and, if not, what additional assumptions are needed.

We will divide the factors of the problem into three parts, (*a*) Facts given, (*b*) Principles of probability accepted as self-evident, (*c*) Factual assumptions made.

(*a*) The facts given are:—

(i) That the bag contains n counters indistinguishable to touch.

(ii) That we have no information at the outset of the experiment what proportion of the counters are white; there may be $0, 1, 2, \ldots n$ whites. (We know of course on *à priori* grounds that any one proportion, so long as it subsists, excludes any other, and that, at any given moment, one of these $n + 1$ possible proportions must subsist.)

(iii) That at the end of the experiment m counters have been drawn out in succession, none being replaced, and that these have all been found to be white.

(*b*) The principles of probability accepted as *à priori* truths are:—

(i) If p and q be two mutually exclusive propositions and x/h means 'the probability of x given h,' then

$$p \vee q/h = p/h + q/h.$$

(ii) If p and q be any two propositions, then

$$p.q/h = p/h \times q/p.h = q/h \times p/q.h.$$

(iii) If we know that several mutually exclusive alternatives are possible and do not know of any reason why one rather than another should be actual, the probability of any one alternative, relative to this precise state of knowledge and ignorance, is equal to that of any other of them, relative to the same data.

(iv) The present proposition is to be regarded rather as a convention for measuring probability than as a substantial proposition. It is: If p and q be coexhaustive and coexclusive propositions, then

$$p/h + q/h = 1.$$

(*c*) The assumptions which we make about matters of fact are:—

(i) That in drawing out a counter our hand is as likely to come in contact with any one as with any other of all those present in the bag at the moment.

(ii) That no process going on in nature during the experiment alters the total number or the proportion of the white counters, and that the constitution of the contents only changes during the experiment by the successive removal of counters by the experimenter.

It is clear that the propositions (*c*) are assumptions about the course of nature and have no *à priori* guarantee. This is perfectly obvious about *c* (ii), and it is evident that a factual assumption is an essential part of *c* (i) even if the *à priori* factor *b* (iii) should also somewhere be involved in it.

On these assumptions it can be proved that the probability that the *next* to be drawn will be white is $\frac{m+1}{m+2}$, and that the probability that *all* the n are white is $\frac{m+1}{n+1}$. I do not propose to go into the details of the argument, which involves the summation of two series. What I wish to point out is that all the nine propositions mentioned above are used in the proof and that no others are needed except the ordinary laws of logic and algebra. It is easy to see in a general way how the assumptions (*c*) enter. Suppose there were a kind of pocket in the bag and that non-whites happened to be accumulated there. Then *c* (i) would be false, and it is clear that a large number of whites might be drawn at first and give a misleadingly high expectation of all being white even though there were quite a large proportion of non-whites in the bag. Suppose again that *c* (ii) were false and that the proportion of whites might change between one draw and the next.

Putting the course of the argument very roughly indeed we may say that at the beginning we start with $n + 1$ equally likely hypotheses as to the constitution of the bag's contents. As we go on drawing whites and no non-whites we learn more of this constitution, certain of these hypotheses are ruled out altogether, the others have their probabilities strengthened in various degrees. But this is only true if we really do learn more about the constitution of the contents by our successive drawings; if, between these, the constitution changes from any cause, we have learnt nothing and the argument breaks down.

We can now consider how far the attempt to establish laws of nature by simple enumeration is parallel to the artificial example just dealt with. For clearness it is best to distinguish here between laws about the qualities of classes of substances [such as the law that All crows are black] and laws about the connexion of events [such as All rises of temperature are followed by expansion]. I do not suggest that this distinction is of great philosophic importance or is ultimately tenable, but it will help us for the present.

There is obviously a very close analogy between investigating the colours of crows and the colours of the counters in a bag. To the counters in the bag correspond all the crows in the universe, past, present, and future. To the pulling out and observing the colour of a counter corresponds the noticing of a certain number of crows. At this point however, the analogy fails in several ways, and all these failures tend to reduce the probability of the suggested law. (i.) The same crow might be observed several times over and counted as a different one. Thus m in the fraction $\frac{m+1}{n+1}$ might be counted to be larger than it really is and the probability thus over-estimated. (ii.) We have no guarantee whatever that crows may not change their colours in the course of their lives. (This possibility was of course also present in the artificial case of counters, and our only ground for rejecting it is in previous inductions.) (iii.) It is quite certain that we are not equally likely to meet with any crow. Even if we grant that any past crow is equally likely to have been met with and its colour reported to us, we know that the assumption of equiprobability is false as to future crows. For we clearly cannot have observed any of the crows that begin to exist after the moment when we make the last observation which we take into account when we make our induction. And the assumption of equiprobability is most precarious even as regards past and present crows. Neither by direct observation nor by the reports of others can I know about crows in any but a restricted region of space. Thus the blackness of the observed crows may not be an attribute of all crows but may be true only of crows in a certain area. Outside this it may fail, as whiteness has been found to fail in the case of Australian swans. Our situation then is like that which would arise with the bag of counters if (*a*) there were a rigid partition in it past which we could not get our hands (distinction of past and future cases), and (*b*) if the bag were much bigger than the extreme stretch of our arm and we could only enter it through one comparatively small opening (restricted area of observation in space). We may sum up this objection by saying that the argument which leads to the probability $\frac{m+1}{n+1}$ assumes that a 'fair selection' has been observed, and that in the case of the crows we know that a 'fair selection' cannot have been observed owing to the fact that I cannot *now* observe *future* instances, and that I cannot directly observe even contemporary instances in all parts of space.

It is easy to prove that when we know that a 'fair selection' has not been observed the probability of a general law must fall below and can never rise above the value $\frac{m+1}{n+1}$ which it reaches if the observed selection be a fair one. Let us suppose that all the S's that might actually have been observed were SQ's; that, *within this class*, the selection observed was a fair one, though not fair for the S's as a whole; and that the number of SQ's is ν. Then, since the number of SQ's examined was m and all were found to be P, the probability that all Sθ's are P is $\frac{m+1}{\nu+1}$. The number of S$\bar{Q}$'s is $n - \nu$; but, by hypothesis, none of these came under examination. Hence we have no information whatever about them, and the probability that any proportion from O to the whole $n - \nu$ inclusive is P is the same, *viz.*, $\frac{1}{n-v+1}$. Now the probability that All S's are P = the probability of the compound proposition: All SQ's are P and All S$\bar{Q}$'s are P. This cannot exceed $\frac{m+1}{\nu+1}\frac{1}{n-\nu+1}$. It is evident that this is less than $\frac{m+1}{n+1}$; for its numerator is the same, whilst its denominator is $n+1+\nu(n-\nu)$, which is greater than $n+1$, since ν is a positive integer less than n.

(iv.) Lastly there is the following fatal difference even if all other difficulties could be overcome. In investigating the counters in the bag we know the total number n. It is finite, and we can make the number m of counters observed approximate fairly closely to it. We do not of course know the total number of crows that have been, are, and will be; but we can be perfectly sure that it must be enormous compared with the number investigated. Hence m is very small compared with n in the investigation of any natural law. Hence $\frac{m+1}{n+1}$, the probability of the law, as determined by induction by simple enumeration, is vanishingly small even under the impossibly favourable conditions of a 'fair selection'. In real life it will be indefinitely smaller than this indefinitely small fraction.

It must be noted, however, that from the same premises from which we deduced the expression $\frac{m+1}{n+1}$ for the probability that *all* S's are P we also deduced the expression $\frac{m+1}{m+2}$ for the probability that the *next* S to be examined will be P. A more general formula which can also be proved from the same premises is that the probability that the next μ to be examined will be P is $\frac{m+1}{m+\mu+1}$. These latter expressions, it will be noted, are independent of n. Hence, if we could get over the difficulties about a 'fair selection' and about possible changes in time and possible repeated examinations of the same S, induction by simple enumeration would play a modest but useful *rôle* even in the investigation of nature. If m were pretty large both in itself and as compared with μ we could predict for the next case and for the next few with tolerable certainty. But this assumes that the 'next case' is one which had as much likelihood as any other of falling under our observations, though it did not actually do so. In the case of persistent entities like counters and crows this condition may perfectly well be fulfilled, for the 'next' simply means the 'next which I happen to observe'. In the case of the counters the one which I shall pull out next was in the bag all through the experiment and was as likely to be taken out as those which actually were taken out. In that of the crows the crow that I shall next observe may have existed when I observed the previous ones, and may have been as likely to fall under my observation as any of those which actually did so. But, as we shall see in a moment, there are special difficulties about events which will not allow us to apply this reasoning to them.

We will now consider the connection of events. Much of what has been said about the investigation of the properties of substances remains true here, but there are the following differences to be noted. Suppose our events are rises in temperature. The class about which we wish to learn is all events of this kind past, present, and future. Now events, unlike substances, cannot change; each is tied to its own position in time and is determined by it. There is no possibility that the *same* rise in temperature should be at one moment followed by an expansion and at another not, as there is a possibility that the same crow may sometimes be black and sometimes white. Rises of temperature at different times are different rises of temperature; it is of course perfectly possible that one may be and another may not be followed by an expansion, but the *same* one cannot occur at two different moments and therefore cannot have different sequents at different times. Hence one difficulty inherent in investigating substances and their properties is ruled out in investigating events and their connexion.

For similar reasons there is no possibility of observing the same event twice, as there is of investigating the same crow twice. In observing events the position is quite parallel to pulling out counters and not putting them back. What is secured artificially in the counter experiment is secured in investigating events by the fact that each event is tied to its moment and ceases to belong to the class of observable events when that moment is past.

So far the inductive observation of events is in a stronger position than that of substances. But here its advantages cease. There is clearly the same impossibility of observing any finite proportion of the whole class, and hence of ascribing any appreciable probability to a general law about its members. There is the same difficulty about observing a 'fair selection' in space. And there is a still more hopeless difficulty about predicting the future even for the next event of the class. For it is perfectly certain that I could not up to now have observed any event which belongs to a moment later than my last observation. Hence the condition of equiprobability breaks down and my observations add nothing to the probability that the next event to be observed will agree with those which I have already observed. With substances, as we saw, it was possible that the next one to be observed had an equal chance of having been observed with any of those which I actually happened to notice. Hence there was a possibility of predicting a few steps ahead if we assume that the substances are not changing their qualities. But this is because substances persist for a time and are not tied to single moments like events.

I conclude then that, neither for substances nor for events, will the principle of probability alone allow us to ascribe a finite probability to general laws by induction by simple enumeration. In the case of substances we can argue a few steps ahead if we can assume a 'fair selection' in space, and can further assume that the substances do not change in the property in question over the time for which we are observing and predicting. For events even this amount of prediction is incapable of logical justification. And the latter fact really invalidates the process for substances. For, if our ground for assuming that the substances will not change their attributes be inductive, it must be an induction about events. The possession of an attribute at each moment of a duration constitutes a class of events, and to argue inductively that there will be no change is to argue from observations on some of the earlier members of this class to the later ones which cannot fall into the class of those which it was equally likely for us to have observed up to the moment at which we stop our observations. It was for this, among other reasons, that I said that the distinction between inductions about substances and inductions about events, though convenient in discussing the subject, was not of ultimate philosophic importance.

Before leaving induction by simple enumeration and passing to the hypothetical method it may be of interest to remark that, in theory, there are two quite different reasons for trying to enlarge the number of our observations as much as possible. (i.) We want to examine as many S's as possible simply in order to increase the proportion of m to n in the fraction $\frac{m+1}{n+1}$. For this purpose it is quite irrelevant whether the observed instances happen under very similar or under very diverse circumstances. It is simply the number that counts. Unfortunately in investigating nature it is of little use to worry ourselves to increase m for this reason, since we know that however much we increase it, it will remain vanishingly small compared with n. (ii.) We want to examine S's under as many different circumstances as possible. This is so as to approximate as nearly as we can to a 'fair selection'. Here it is not the mere number of instances that counts but the number of different circumstances under which the observed instances happen. Unfortunately however well we succeed in this we cannot raise the probability above $\frac{m+1}{n+1}$, we can only ensure that it shall not fall indefinitely below that indefinitely small fraction.

B. *The Hypothetical Method.*—I shall first briefly state the connexion between this and induction by simple enumeration. I shall then consider the logical principles on which the hypothetical method is based and see whether they, without additional assumptions about nature, will suffice to give a finite probability to any suggested law.

Induction by simple enumeration is just a rather special case of the hypothetical method. At the outset of our experiment with the bag we have $n + 1$ equally likely hypotheses as to the constitution of its contents. After the first draw has been made and the counter found to be white one of these hypotheses is definitely refuted (*viz.* that there were no whites present). The others remain possible but no longer equally probable; the probability of each on the new datum can be calculated. After the second draw another one hypothesis is definitely refuted; the remaining $n - 1$ are all possible, but once more their probabilities have been altered in various calculable amounts by the addition of the new datum. The procedure after each draw (assuming that all turn out to be white) is the same; one hypothesis is always refuted; the rest always remain possible, and among these is always the hypothesis that all in the bag are white; and the probabilities of each are increased in various calculable degrees. The special peculiarities of this method are (*a*) that the various hypotheses are known to be mutually exclusive and to exhaust all the possibilities, (*b*) that they deal solely with the question of numbers or ratios, and (*c*) that only two of them, *viz.* the hypothesis that none are white and the hypothesis that all are white are comparable with general laws.

The reasoning of the hypothetical method in its most general form is the following. Let h be the hypothesis; it will consist of one or more propositions. We prove by ordinary deductive reasoning that h implies the propositions $c_1, c_2 \ldots c_n$. Let h/f be the probability of the hypothesis relative to any data that we may have before we start our experiments to verify it. Then we know in general that

$$h.c_1/f = c_1/f \times h/c_1.f = h/f \times c_1/h.f.$$

If h implies c_1 it is clear that c_1/h (and $\therefore c_1/h.f) = 1$.

Hence $$c_1/f \times h/c_1.f = h/f.$$

Whence $$h/c_1.f = \frac{h/f}{c_1/f}$$

Again $$h.c_1.c_2/f = h.c_1c_2/f = c_1c_2/f \times h/c_1c_2f$$
$$= h/f \times c_1c_2/h.f.$$

But $$c_1c_2/hf = c_1/hf \times c_2/c_1hf$$
$$= c_2/c_1hf.$$

And since h implies c_2 it is clear that c_2/h (and $\therefore c_2/c_1hf$) $= 1$.

Hence $$h/c_1c_2f = \frac{h/f}{c_1c_2/f}$$

[1] In general, if h implies $c_1, c_2 \ldots c_n$, we shall have

$$h/c_1c_2 \ldots c_nf = \frac{h/f}{c_1c_2 \ldots c_n/f}$$

We can learn much from a careful study of this formula. We see that the probability of a hypothesis is increased as we verify its consequences because the initial probability is the numerator of a fraction whose denominator is a product which contains more factors (and $\therefore$, since they are proper fractions, grows *smaller*) the more consequences we deduce and verify.

For $c_1c_2 \ldots c_n/f = c_1/f \times c_2/c_1f \times c_3/c_2c_1f \times \ldots c_n/c_{n-1} \ldots c_1f$. Next we see that it is only by increasing the number of verified consequences which are logically independent of each other that we increase the probability of the hypothesis. For if, *e.g.*, c_{r-1} implies c_r the factor $c_r/c_{r-1} \ldots c_1f = 1$, and so does nothing to reduce the denominator and thus increase the probability of the hypothesis. Again, the more

[1] The mathematical theory of the probability of hypotheses is treated by Boole in his *Laws of Thought*. The problem in its most general form (where it is not assumed that h *implies* $c_1, c_2 \ldots c_n$, but only that it modifies their probability) has been worked out, but not I think published, by Mr. W. E. Johnson. I take this opportunity of expressing the very great obligations which I am under to Mr. Johnson, obligations which I know are felt by all those who have had the privilege of attending his lectures on advanced logic or discussing logical problems with him. Mr. Johnson, however, must not be held responsible for the views expressed in the present paper.

unlikely the consequences were on the original data f which we had before we started to verify the hypothesis the more they increase the probability of the hypothesis if they be found to be true. For this means that the factors like c_1/f are very small, hence that the denominator is small, hence that the final value of $h/c_1c_2 \ldots c_n$ is likely to be large. This is the precise amount of truth that there is in the common view that an hypothesis is greatly strengthened by leading to some surprising consequence which is found to be true. The important point is not the psychological surprisingness of the consequence, but is the purely logical fact that *apart from* the hypothesis it was very unlikely to be true, *i.e.* it was neither implied nor rendered probable by anything that we knew when we put the hypothesis forward. Lastly we must notice that the factor h/f, expressing the probability of our hypothesis on the data known before any attempt at verification has been made, is always present in the numerator, *i.e.* as a multiplicative factor. Hence, unless we can be sure that this is not indefinitely small, we cannot be sure that the final probability of the hypothesis will be appreciable.

There is just one thing further to be said about h/f. h may be a complex set of propositions. Suppose we have two alternative hypotheses h_1 and h_2. Suppose $h_1 \equiv p_1p_2 \ldots p_m$ and $h_2 \equiv q_1q_2 \ldots q_n$, and let $n > m$. Then h_2/f is a product of n factors all fractional and h_1/f is a product of m factors all fractional. There will thus be a tendency for the less complex hypothesis to be more probable intrinsically than the more complex one. But this is only a tendency, not a general rule. The product $\frac{1}{2} \cdot \frac{3}{4} \cdot \frac{5}{6} \cdot \frac{7}{8}$ is greater than $\frac{1}{2} \cdot \frac{1}{3} \cdot \frac{5}{16}$, although the latter contains fewer factors than the former. This tendency, however, is the small amount of logical truth in the common notion that a more complicated hypothesis is less likely to be true than a simpler one.

We are now in a position to see whether the hypothetical method in general is any more capable of giving a finite probability to alleged laws of nature, without some additional premise, than its special case the method of induction by simple enumeration. I shall try to prove that, whilst the hypothetical method has many advantages which fully explain why it is the favourite instrument of all advanced sciences, it yet is insufficient, without some further assumption, to establish reasonably probable laws.

The advantages of the method are obvious enough. (i) The hypotheses of induction by simple enumeration are purely numerical and therefore no consequence can be deduced from them except the probability of getting a certain number of favourable cases in a certain number of experiments. When hypotheses are not limited in this way the most varied consequences can be deduced, and, if verified, they increase the probability of the hypothesis. (ii) If the hypothesis be stated in mathematical form remote and obscure consequences can be deduced with all the certainty of mathematical reasoning. We thus have guidance as to what experiments to try, and powerful confirmation if our experiments succeed. The history of the wave theory of light is full of examples of this fact. (iii) If careful experiments refute some of the consequences of an hypothesis we knew of course from formal logic that the hypothesis cannot, as it stands, be true. But if most of the deduced consequences have been verified we may fairly suspect that there cannot be much wrong with the hypothesis. And the very deductions which have failed to be verified may suggest to us the kind and degree of modification that is necessary. (iv) It is true that in induction by simple enumeration we have the advantage of knowing that our alternative hypotheses are exhaustive and exclusive. But in investigating nature this is of little profit since we also know that their number is indefinitely large. Now, it might be said, in the hypothetical method, although we cannot be sure that we have envisaged all possible alternatives, yet the number of possible laws to explain a given type of phenomena cannot be extremely great, hence the intrinsic probability of nóne of them will be excessively small if we regard them as all equally probable before attempted verification.

This last argument seems plausible enough at first sight. Yet it is mistaken, and in exposing the mistake we shall see why it is that the hypothetical method by itself will not give an appreciable probability to any suggested law. Why is it that the intrinsic probability of the law that all S is P is vanishingly small in induction by simple enumeration whilst that of any suggested law in the hypothetical method is not, to all appearance, vanishingly small? One reason is that the alternatives taken as intrinsically equally probable are not *in pari materia* in the two methods. In induction by simple enumeration the alternatives are not various possible *laws*, but various possible *proportions*, only two of which, *viz.* 0 °/₀ and 100 °/₀ of the S's being P, are laws. In the hypothetical method we have so far assumed that the alternative hypotheses are always laws. This naturally reduces the number of possible alternatives and hence increases the intrinsic probability of each as compared with the alternatives of induction by simple enumeration. But this difference renders comparison between the two methods unfair. If in simple enumeration alternatives other than laws are to be accepted as intrinsically as probable as laws there is no reason why the same assumption should not be made in the hypothetical method. And it is surely evident that the objections which apply to induction by simple enumeration as a sufficient means of establishing a law apply equally to the hypothetical method. All the experiments which have been made up to a given moment to verify an hypothesis can throw no light on the truth of this hypothesis as referring to moments after that at which the last experiment was performed. Now it is certain that an indefinite number of hypotheses could be put forward agreeing in their consequences up to a given moment and diverging after it. Exactly similar remarks apply to space; there can clearly be any number of alternative hypotheses which have the same consequences within a given region of space and different consequences outside it, and no experiments performed wholly within this region can give any ground for deciding between them. I think therefore that we may now claim to have proved our second contention that the degree of belief which we actually attach to the conclusions of well-established inductive arguments cannot be justified by any known principle of probability unless some further premise about the existent world be assumed. What this premise is, whether it can be stated clearly enough to admit of logical criticism, and whether in that event it will survive logical criticism, are extremely difficult questions which I reserve for the second part of this paper. What I have said so far I believe to be fairly certain, what I have yet to say I know to be extremely doubtful.

(*To be continued.*)