of a phenomenon which has appeared as part of a more complex phenomenon, may be *some* reason for expecting it to be associated on another occasion with part of the same complex. Yet if different wholes were subject to different laws *quâ* wholes and not simply on account of and in proportion to the differences of their parts, knowledge of a part could not lead, it would seem, even to presumptive or probable knowledge as to its association with other parts. Given, on the other hand, a number of legally atomic units and the laws connecting them, it would be possible to deduce their effects *pro tanto* without an exhaustive knowledge of all the coexisting circumstances.

We do habitually assume, I think, that the size of the atomic unit is for mental events an individual consciousness, and for material events an object small in relation to our perceptions. These considerations do not show us a way by which we can justify Induction. But they help to elucidate the kind of assumptions which we do actually make, and may serve as an introduction to what follows.

CHAPTER XXII

THE JUSTIFICATION OF THESE METHODS

1. The general line of thought to be followed in this chapter may be indicated, briefly, at the outset.

A system of facts or propositions, as we ordinarily conceive it, may comprise an indefinite number of members. But the ultimate constituents or indefinables of the system, which all the members of it are about, are less in number than these members themselves. Further, there are certain laws of necessary connection between the members, by which it is meant (I do not stop to consider whether *more* than this is meant) that the truth or falsity of every member can be inferred from a knowledge of the laws of necessary connection together with a knowledge of the truth or falsity of some (but not all) of the members.

The ultimate constituents together with the laws of necessary connection make up what I shall term the *independent variety* of the system. The more numerous the ultimate constituents and the necessary laws, the greater is the system's independent variety. It is not necessary for my present purpose, which is merely to bring before the reader's mind the sort of conception which is in mine, that I should attempt a complete definition of what I mean by a system.

Now it is characteristic of a system, as distinguished from a collection of heterogeneous and independent facts or propositions, that the number of its premisses, or, in other words, the amount of independent variety in it, should be less than the number of its members. But it is not an obviously essential characteristic of a system that its premisses or its independent variety should be actually finite. We must distinguish, therefore, between systems which may be termed finite and infinite respectively, the terms *finite* and *infinite* referring not to the number of members in the system but to the amount of independent variety in it.

The purpose of the discussion, which occupies the greater part of this chapter, is to maintain that, if the premisses of our argument permit us to assume that the facts or propositions, with which the argument is concerned, belong to a *finite* system, then probable knowledge can be validly obtained by means of an inductive argument. I now proceed to approach the question from a slightly different standpoint, the controlling idea, however, being that which is outlined above.

2. What is our actual course of procedure in an inductive argument? We have before us, let us suppose, a set of n instances which have r known qualities, $a_1a_2 \ldots a_r$ in common, these r qualities constituting the known positive analogy. From these qualities three (say) are picked out, namely, a_1, a_2, a_3, and we inquire with what probability *all* objects having these three qualities have also certain other qualities which we have picked out, namely, a_{r-1}, a_r. We wish to determine, that is to say, whether the qualities a_{r-1}, a_r are *bound up* with the qualities a_1, a_2, a_3. In thus approaching this question we seem to suppose that the qualities of an object are bound together in a limited number of *groups*, a sub-class of each group being an infallible symptom of the coexistence of certain other members of it also.

Three possibilities are open, any of which would prove destructive to our generalisation. It may be the case (1) that a_{r-1} or a_r is independent of all the other qualities of the instances—they may not overlap, that is to say, with any other groups; or (2) that $a_1a_2a_3$ do not belong to the same groups as $a_{r-1}a_r$; or (3) that $a_1a_2a_3$, while they belong to the same group as $a_{r-1}a_r$, are not sufficient to specify this group uniquely—they belong, that is to say, to other groups also which do not include a_{r-1} and a_r. The precautions we take are directed towards reducing the likelihood, so far as we can, of each of these possibilities. We distrust the generalisation if the terms typified by $a_{r-1}a_r$ are numerous and comprehensive, because this increases the likelihood that some at least of them fall under heading (1), and also because it increases the likelihood of (3). We trust it if the terms typified by $a_1a_2a_3$ are numerous and comprehensive, because this decreases the likelihood both of (2) and of (3). If

we find a new instance which agrees with the former instances in $a_1a_2a_3a_{r-1}a_r$ but not in a_4, we welcome it, because this disposes of the possibility that it is a_4, alone or in combination, that is bound up with $a_{r-1}a_r$. We desire to increase our knowledge of the properties, lest there be some positive analogy which is escaping us, and when our knowledge is incomplete we multiply instances, which we do not know to increase the negative analogy for certain, in the hope that they may do so.

If we sum up the various methods of Analogy, we find, I think, that they are all capable of arising out of an underlying assumption, that if we find two sets of qualities in coexistence there is a finite probability that they belong to the same group, and a finite probability also that the first set specifies this group uniquely. Starting from this assumption, the object of the methods is to increase the finite probability and make it large. Whether or not anything of this sort is explicitly present to our minds when we reason scientifically, it seems clear to me that we do act exactly as we should act, if this were the assumption from which we set out.

In most cases, of course, the field is greatly simplified from the first by the use of our pre-existing knowledge. Of the properties before us we generally have good reason, derived from prior analogies, for supposing some to belong to the same group and others to belong to different groups. But this does not affect the theoretical problem confronting us.

3. What kind of ground could justify us in assuming the existence of these finite probabilities which we seem to require? If we are to obtain them, not directly, but by means of argument, we must somehow base them upon a finite number of exhaustive alternatives.

The following line of argument seems to me to represent, on the whole, the kind of assumption which is obscurely present to our minds. We suppose, I think, that the almost innumerable apparent properties of any given object all arise out of a finite number of generator properties, which we may call $\phi_1\phi_2\phi_3 \ldots$ Some arise out of ϕ_1 alone, some out of ϕ_1 in conjunction with ϕ_2, and so on. The properties which arise out of ϕ_1 alone form one group; those which arise out of $\phi_1\phi_2$ in conjunction form another group, and so on. Since the number of generator properties is finite, the number of groups also is finite. If a set of apparent properties arise (say) out of three generator properties $\phi_1\phi_2\phi_3$, then this set of properties may be said to specify the group $\phi_1\phi_2\phi_3$. Since the total number of apparent properties is assumed to be greater than that of the generator properties, and since the number of groups is finite, it follows that, if two sets of apparent properties are taken, there is, in the absence of evidence to the contrary, a finite probability that the second set will belong to the group specified by the first set.

There is, however, the possibility of a plurality of generators. The first set of apparent properties may specify more than one group,—there is more than one group of generators, that is to say, which are competent to produce it; and some only of these groups may contain the second set of properties. Let us, for the moment, rule out this possibility.

When we argue from an analogy, and the instances have two groups of characters in common, namely ϕ and f, either f belongs to the group ϕ or it arises out of generators partly distinct from those out of which ϕ arises. For the reason already explained there is a finite probability that f and ϕ belong to the same group. If this is the case, *i.e.* if the generalisation $g(\phi f)$ is valid, then f will certainly be true of all other cases in which ϕ is true; if this is not the case, then f will not always be true when ϕ is true. We have, therefore, the preliminary conditions necessary for the application of pure induction. If x_r, etc., are the instances,

$$g/h = p_0, \text{ where } p_0 \text{ is finite,}$$
$$x_r/gh = 1, \text{ etc.,}$$

and $$x_r/x_1x_2 \ldots x_{r-1}\bar{g}h = 1 - \epsilon, \text{ where } \epsilon \text{ is finite.}$$

And hence, by the argument of Chapter XX., the probability of a generalisation, based on such evidence as this, is capable, under suitable conditions, of tending towards certainty as a limit, when the number of instances is increased.

If ϕ is complex and includes a number of characters which are not always found together, it must include a number of separate generator properties and specify a large group; hence the initial probability that f belongs to this group is relatively large. If, on the other hand, f is complex, there will be, for the same reasons *mutatis mutandis*, a relatively smaller initial probability than otherwise that f belongs to any other given group.

When the argument is mainly by analogy, we endeavour to obtain evidence which makes the initial probability p_0 relatively high; when the analogy is weak and the argument depends for its strength upon pure induction, p_0 is small and p_m, which is based upon numerous instances, depends for its magnitude upon their number. But an argument from induction must always involve some element of analogy, and, on the other hand, few arguments from analogy can afford to ignore altogether the strengthening influence of pure induction.

4. Let us consider the manner in which the methods of analogy increase the initial likelihood that two characters belong to the same group. The numerous characters of an object which are known to us may be represented by $a_1a_2 \ldots a_n$. We select two sets of these, a_r and a_s, and seek to determine whether a_s always belongs to the group specified by a_r. Our previous knowledge will enable us, in general, to rule out many of the object's characters as being irrelevant to the groups specified by a_r and a_s, although this will not be possible in the most fundamental inquiries. We may also know that certain characters are always associated with a_r or with a_s. But there will be left a residuum of whose connection with a_r or a_s we are ignorant. These characters, whose relevance is in doubt, may be represented by $a_{r+1} \ldots a_{s-1}$. If the analogy is perfect, these characters are eliminated altogether. Otherwise, the argument is weakened in proportion to the comprehensiveness of these doubtful characters. For it may be the case that some of $a_{r+1} \ldots a_{s-1}$ are necessary as well as a_r, in order to specify all the generators which are required to produce a_s.

5. We may possibly be justified in neglecting certain of the characters $a_{r+1} \ldots a_{s-1}$ by *direct* judgments of irrelevance. There are certain properties of objects which we rule out from the beginning as wholly or largely independent and irrelevant to all, or to some, other properties. The principal judgments of this kind, and those alone about which we seem to feel much confidence, are concerned with absolute position in time and space, this class of judgments of irrelevance being summed up, I have suggested, in the Principle of the Uniformity of Nature. We judge that *mere* position in time and space cannot possibly affect, as a determining cause, any other characters; and this belief appears so strong and certain, although it is hard to see

how it can be based on experience, that the judgment by which we arrive at it seems perhaps to be direct. A further type of instance in which some philosophers seem to have trusted direct judgments of relevance in these matters arises out of the relation between mind and matter. They have believed that no mental event can possibly be a *necessary* condition for the occurrence of a material event.

The Principle of the Uniformity of Nature, as I interpret it, supplies the answer, if it is correct, to the criticism that the instances, on which generalisations are based, are all alike in being past, and that any generalisation, which is applicable to the future, must be based, for this reason, upon imperfect analogy. We judge directly that the resemblance between instances, which consists in their being past, is in itself irrelevant, and does not supply a valid ground for impugning a generalisation.

But these judgments of irrelevance are not free from difficulty, and we must be suspicious of using them. When I say that position is irrelevant, I do not mean to deny that a generalisation, the premiss of which specifies position, may be true, and that the same generalisation without this limitation might be false. But this is because the generalisation is incompletely stated; it happens that objects so specified have the required characters, and hence their position supplies a sufficient criterion. Position may be relevant as a sufficient condition but never as a *necessary* condition, and the inclusion of it can only affect the truth of a generalisation when we have left out some other essential condition. A generalisation which is true of one instance must be true of another which *only* differs from the former by reason of its position in time or space.

6. Excluding, therefore, the possibility of a plurality of generators, we can justify the method of perfect analogy, and other inductive methods in so far as they can be made to approximate to this, by means of the assumption that the objects in the field, over which our generalisations extend, do not have an infinite number of independent qualities; that, in other words, their characteristics, however numerous, cohere together in groups of invariable connection, which are finite in number. This does not limit the number of entities which are only *numerically* distinct. In the language used at the beginning of this chapter, the use of inductive methods can be

justified if they are applied to what we have reason to suppose a finite system.[1]

7. Let us now take account of a possible plurality of generators. I mean by this the possibility that a given character can arise in more than one way, can belong to more than one distinct group, and can arise out of more than one generator. ϕ might, for instance, be sometimes due to a generator a_1, and a_1 might invariably produce f. But we could not generalise from ϕ to f, if ϕ might be due in other cases to a different generator a_2 which would *not* be competent to produce f.

If we were dealing with inductive correlation, where we do not claim universality for our conclusions, it would be sufficient for us to assume that the number of distinct generators, to which a given property ϕ can be due, is always finite. To obtain validity for universal generalisations it seems necessary to make the more comprehensive and less plausible assumption that a finite probability always exists that there is *not*, in any given case, a plurality of causes. With this assumption we have a valid argument from pure induction on the same lines, nearly, as before.

8. We have thus two distinct difficulties to deal with, and we require for the solution of each a separate assumption. The point may be illustrated by an example in which only one of the difficulties is present. There are few arguments from analogy of which we are better assured than the existence of other people. We feel indeed so well assured of their existence that it has been thought sometimes that our knowledge of them must be in some way direct. But analogy does not seem to me unequal to the proof. We have numerous experiences in our own person of acts which are associated with states of consciousness, and we infer that similar acts in others are likely to be associated with similar states of consciousness. But this argument from analogy is superior in one respect to nearly all other empirical arguments, and this superiority may possibly explain the great confidence which we feel in it. We do seem in this case to have direct knowledge, such as we have in no other case, that our states of consciousness are, sometimes at least, causally connected with some of our acts. We do not, as in other cases,

[1] Mr. C. D. Broad, in two articles "On the Relation between Induction and Probability" (*Mind*, 1918 and 1920), has been following a similar line of thought.

merely observe invariable sequence or coexistence between consciousness and act; and we do believe it to be vastly improbable in the case of some at least of our own physical acts that they could have occurred without a mental act to support them. Thus, we seem to have a special assurance of a kind not usually available for believing that there is *sometimes* a necessary connection between the conclusion and the condition of the generalisation; we doubt it only from the possibility of a plurality of causes.

The objection to this argument on the ground that the analogy is always imperfect, in that all the observed connections of consciousness and act are alike in being *mine*, seems to me to be invalid on the same ground as that on which I have put on one side objections to future generalisations, which are based on the fact that the instances which support them are all alike in being *past*. If direct judgments of irrelevance are ever permissible, there seems some ground for admitting one here.

9. As a logical foundation for Analogy, therefore, we seem to need some such assumption as that the amount of variety in the universe is limited in such a way that there is no one object so complex that its qualities fall into an infinite number of independent groups (*i.e.* groups which might exist independently as well as in conjunction); or rather that none of the objects about which we generalise are as complex as this; or at least that, though some objects may be infinitely complex, we sometimes have a finite probability that an object about which we seek to generalise is not infinitely complex.

To meet a possible plurality of causes some further assumption is necessary. If we were content with Inductive Correlations and sought to prove merely that there was a probability in favour of *any* instance of the generalisation in question, without inquiring whether there was a probability in favour of *every* instance, it would be sufficient to suppose that, while there may be more than one sufficient cause of a character, there is not an infinite number of distinct causes competent to produce it. And this involves no new assumption; for if the aggregate variety of the system is finite, the possible plurality of causes must also be finite. If, however, our generalisation is to be universal, so that it breaks down if there is a single exception to it, we must obtain, by some means or other, a finite probability that the set of characters,

which condition the generalisation, are *not* the possible effect of more than one distinct set of fundamental properties. I do not know upon what ground we could establish a finite probability to this effect. The necessity for this seemingly arbitrary hypothesis strongly suggests that our conclusions should be in the form of inductive correlations, rather than of universal generalisations. Perhaps our generalisations should always run: 'It is probable that any given ϕ is f,' rather than, 'It is probable that all ϕ are f.' Certainly, what we commonly seem to hold with conviction is the belief that the sun will rise *to-morrow*, rather than the belief that the sun will *always* rise so long as the conditions explicitly known to us are fulfilled. This will be matter for further discussion in Part V., when Inductive Correlation is specifically dealt with.

10. There is a vagueness, it may be noticed, in the number of instances, which would be required on the above assumptions to establish a given numerical degree of probability, which corresponds to the vagueness in the degree of probability which we do actually attach to inductive conclusions. We assume that the necessary number of instances is finite, but we do not know what the number is. We know that the probability of a well-established induction is great, but, when we are asked to name its degree, we cannot. Common sense tells us that some inductive arguments are stronger than others, and that some are very strong. But how much stronger or how strong we cannot express. The probability of an induction is only numerically definite when we are able to make definite assumptions about the number of independent equiprobable influences at work. Otherwise, it is non-numerical, though bearing relations of greater and less to numerical probabilities according to the approximate limits within which our assumption as to the possible number of these causes lies.

11. Up to this point I have supposed, for the sake of simplicity, that it is necessary to make our assumptions as to the limitation of independent variety in an absolute form, to assume, that is to say, the finiteness of the system, to which the argument is applied, *for certain*. But we need not in fact go so far as this.

If our conclusion is C and our empirical evidence is E, then, in order to justify inductive methods, our premisses must include, in addition to E, a general hypothesis H such that C/H, the *à priori* probability of our conclusion, has a finite value. The effect of E is to increase the probability of C above its initial *à priori* value, C/HE being greater than C/H. But the method of strengthening C/H by the addition of evidence E is valid quite apart from the particular content of H. If, therefore, we have another general hypothesis H' and other evidence E', such that H/H' has a finite value, we can, without being guilty of a circular argument, use evidence E' by the same method as before to strengthen the probability H/H'. If we call H, namely, the absolute assertion of the finiteness of the system under consideration, the *inductive hypothesis*, and the process of strengthening C/H by the addition E the *inductive method*, it is not circular to use the inductive method to strengthen the inductive hypothesis itself, relative to some more primitive and less far-reaching assumption. If, therefore, we have any reason (H') for attributing *à priori* a finite probability to the Inductive Hypothesis (H), then the actual conformity of experience *à posteriori* with expectations based on the assumption of H can be utilised by the inductive method to attribute an enhanced value to the probability of H. To this extent, therefore, we can support the Inductive Hypothesis by experience. In dealing with any particular question we can take the Inductive Hypothesis, not at its *à priori* value, but at the value to which experience in general has raised it. What we require *à priori*, therefore, is not the certainty of the Inductive Hypothesis, but a finite probability in its favour.[1]

Our assumption, in its most limited form, then, amounts to this, that we have a finite *à priori* probability in favour of the Inductive Hypothesis as to there being some limitation of independent variety (to express shortly what I have already explained in detail) in the objects of our generalisation. Our experience might have been such as to diminish this probability *à posteriori*. It has, in fact, been such as to increase it. It is because there has been so much repetition and uniformity in our experience that we place great confidence in it. To this extent the popular opinion that Induction depends upon experience for its validity is justified and does not involve a circular argument.

[1] I have implicitly assumed in the above argument that if H' supports H, it strengthens an argument which H would strengthen. This is not *necessarily* the case for the reasons given on pp. 68 and 147. In these passages the necessary conditions for the above are elucidated. I am, therefore, assuming that in the case now in question these conditions actually are fulfilled.

12. I think that this assumption is adequate to its purpose and would justify our ordinary methods of procedure in inductive argument. It was suggested in the previous chapter that our theory of Analogy ought to be as applicable to mathematical as to material generalisations, if it is to justify common sense. The above assumptions of the limitation of independent variety sufficiently satisfy this condition. There is nothing in these assumptions which gives them a peculiar reference to material objects. We believe, in fact, that all the properties of numbers can be derived from a *limited* number of laws, and that the same set of laws governs all numbers. To apply empirical methods to such things as numbers renders it necessary, it is true, to make an assumption about the nature of numbers. But it is the same kind of assumption as we have to make about material objects, and has just about as much, or as little, plausibility. There is no new difficulty.

The assumption, also, that the system of Nature is finite is in accordance with the analysis of the underlying assumption of scientists, given at the close of the previous chapter. The hypothesis of atomic uniformity, as I have called it, while not formally equivalent to the hypothesis of the limitation of independent variety, amounts to very much the same thing. If the fundamental laws of connection changed altogether with variations, for instance, in the shape or size of bodies, or if the laws governing the behaviour of a complex had no relation whatever to the laws governing the behaviour of its parts when belonging to other complexes, there could hardly be a limitation of independent variety in the sense in which this has been defined. And, on the other hand, a limitation of independent variety seems necessarily to carry with it some degree of atomic uniformity. The underlying conception as to the character of the System of Nature is in each case the same.

13. We have now reached the last and most difficult stage of the discussion. The logical part of our inquiry is complete, and it has left us, as it is its business to leave us, with a question of epistemology. Such is the premiss or assumption which our logical processes need to work upon. What right have we to make it? It is no sufficient answer in philosophy to plead that the assumption is after all a very little one.

I do not believe that any conclusive or perfectly satisfactory answer to this question can be given, so long as our knowledge of the subject of epistemology is in so disordered and undeveloped a condition as it is in at present. No proper answer has yet been given to the inquiry—of what sorts of things are we *capable* of direct knowledge? The logician, therefore, is in a weak position, when he leaves his own subject and attempts to solve a particular instance of this general problem. He needs guidance as to what *kind* of reason we could have for such an assumption as the use of inductive argument appears to require.

On the one hand, the assumption may be absolutely *à priori* in the sense that it would be equally applicable to all possible objects. On the other hand, it may be seen to be applicable to some classes of objects only. In this case it can only arise out of some degree of particular knowledge as to the nature of the objects in question, and is to this extent dependent on experience. But if it is experience which in this sense enables us to know the assumption as true of certain amongst the objects of experience, it must enable us to know it in some manner which we may term direct and not as the result of an inference.

Now an assumption, that *all* systems of fact are finite (in the sense in which I have defined this term), cannot, it seems perfectly plain, be regarded as having absolute, universal validity in the sense that such an assumption is self-evidently applicable to every kind of object and to all possible experiences. It is not, therefore, in quite the same position as a self-evident *logical* axiom, and does not appeal to the mind in the same way. The most which can be maintained is that this assumption is true of *some* systems of fact, and, further, that there are some objects about which, as soon as we understand their nature, the mind is able to apprehend directly that the assumption in question *is* true.

In Chapter II. § 7, I wrote: "By some mental process of which it is difficult to give an account, we are able to pass from direct acquaintance with things to a knowledge of propositions about the things of which we have sensations or understand the meaning." Knowledge, so obtained, I termed direct knowledge. From a sensation of yellow and from an understanding of the meaning of 'yellow' and of 'colour,' we could, I suggested, have direct knowledge of the fact or proposition 'yellow is a colour;' we might also know that colour cannot exist without extension, or that two colours cannot be perceived at the same time in the same place. Other philosophers might use terms differently and express themselves otherwise; but the substance of what I was there trying to say is not very disputable. But when we come to the question as to what kinds of propositions we can come to know in this manner, we enter upon an unexplored field where no certain opinion is discoverable.

In the case of logical terms, it seems to be generally agreed that if we understand their meaning we can know directly propositions about them which go far beyond a mere expression of this meaning;—propositions of the kind which some philosophers have termed *synthetic*. In the case of non-logical or empirical entities, it seems sometimes to be assumed that our direct knowledge must be confined to what may be regarded as an expression or description of the meaning or sensation apprehended by us. If this view is correct the Inductive Hypothesis is not the kind of thing about which we can have direct knowledge as a result of our acquaintance with objects.

I suggest, however, that this view is incorrect, and that we are capable of direct knowledge about empirical entities which goes beyond a mere expression of our understanding or sensation of them. It may be useful to give the reader two examples, more familiar than the Inductive Hypothesis, where, as it appears to me, such knowledge is commonly assumed. The first is that of the causal irrelevance of mere position in time and space, commonly called the Uniformity of Nature. We do believe, and yet have no adequate inductive reason whatever for believing, that mere position in time and space cannot make any difference. This belief arises directly, I think, out of our acquaintance with the objects of experience and our understanding of the concepts of 'time' and 'space.' The second is that of the Law of Causation. We believe that every object in time has a 'necessary' connection [1] with some set of objects at a previous time. This belief also, I think, arises in the same way. It is to be noticed that neither of these beliefs clearly arises, in spite of the directness which may be claimed for them, out of any one single experience. In a way analogous to these, the validity of assuming the Inductive Hypothesis, as applied to a particular class of objects, appears to me to be justified.

Our justification for using inductive methods in an argument

[1] I do not propose to define the meaning of this.

about numbers arises out of our perceiving directly, when we understand the meaning of a number, that they are of the required character.[1] And when we perceive the nature of our phenomenal experiences, we have a direct assurance that in their case also the assumption is legitimate. We are capable, that is to say, of direct synthetic knowledge about the nature of the objects of our experience. On the other hand, there may be some kinds of objects, about which we have no such assurance and to which inductive methods are not reasonably applicable. It may be the case that some metaphysical questions are of this character and that those philosophers have been right who have refused to apply empirical methods to them.

14. I do not pretend that I have given any perfectly adequate reason for accepting the theory I have expounded, or any such theory. The Inductive Hypothesis stands in a peculiar position in that it seems to be neither a self-evident logical axiom nor an object of direct acquaintance; and yet it is just as difficult, as though the inductive hypothesis were either of these, to remove from the organon of thought the inductive method which can only be based on it or on something like it.

As long as the theory of knowledge is so imperfectly understood as now, and leaves us so uncertain about the grounds of many of our firmest convictions, it would be absurd to confess to a special scepticism about this one. I do not think that the foregoing argument has disclosed a reason for such scepticism. We need not lay aside the belief that this conviction gets its invincible certainty from some valid principle darkly present to our minds, even though it still eludes the peering eyes of philosophy.

[1] Since numbers are logical entities, it may be thought less unorthodox to make such an assumption in their case.

CHAPTER XXIII

SOME HISTORICAL NOTES ON INDUCTION

1. The number of books, which deal with inductive[1] theory, is extraordinarily small. It is usual to associate the subject with the names of Bacon, Hume, and Mill. In spite of the modern tendency to depreciate the first and the last of these, they are the principal names, I think, with which the history of induction ought to be associated. The next place is held by Laplace and Jevons. Amongst contemporary logicians there is an almost complete absence of constructive theory, and they content themselves for the most part with the easy task of criticising Mill, or with the more difficult one of following him.

That the inductive theories of Bacon and of Mill are full of errors and even of absurdities, is, of course, a commonplace of criticism. But when we ignore details, it becomes clear that they were really attempting to disentangle the essential issues. We depreciate them partly, perhaps, as a reaction from the view once held that they helped the progress of scientific discovery. For it is not plausible to suppose that Newton owed anything to Bacon, or Darwin to Mill. But with the logical problem their minds were truly occupied, and in the history of logical theory they should always be important.

It is true, nevertheless, that the advancement of science was the main object which Bacon himself, though not Mill, believed that his philosophy would promote. The *Great Instauration* was intended to promulgate an actual method of discovery entirely different from any which had been previously known.[2] It did

[1] See note at the end of this chapter on "The Use of the Term *Induction*."
[2] He speaks of himself as being "in hac re plane protopirus, et vestigia nullius sequutus"; and in the *Praefatio Generalis* he compares his method to the mariner's compass, until the discovery of which no wide sea could be crossed (see Spedding and Ellis, vol. i. p. 24).

not do this, and against such pretensions Macaulay's well-known essay was not unjustly directed. Mill, however, expressly disclaimed in his preface any other object than to classify and generalise the practices "conformed to by accurate thinkers in their scientific inquiries." Whereas Bacon offered rules and demonstrations, hitherto unknown, with which any man could solve all the problems of science by taking pains, Mill admitted that "in the existing state of the cultivation of the sciences, there would be a very strong presumption against any one who should imagine that he had effected a revolution in the theory of the investigation of truth, or added any fundamentally new process to the practice of it."

2. The theories of both seem to me to have been injured, though in different degrees, by a failure to keep quite distinct the three objects: (1) of helping the scientist, (2) of explaining and analysing his practice, and (3) of justifying it. Bacon was really interested in the second as well as in the first, and was led to some of his methods by reflecting upon what distinguished good arguments from bad in actual investigations. To logicians his methods were as new as he claimed, but they had their origin, nevertheless, in the commonest inferences of science and daily life. But his main preoccupation was with the first, which did injury to his treatment of the third. He himself became aware as the work progressed that, in his anxiety to provide an infallible mode of discovery, he had put forth more than he would ever be able to justify.[1] His own mind grew doubtful, and the most critical parts of the description of the new method were never written. No one who has reflected much upon Induction need find it difficult to understand the progress and development of Bacon's thoughts. To the philosopher who first distinguished some of the complexities of empirical proof in a generalised, and not merely a particular, form, the prospects of systematising these methods must have seemed extraordinarily hopeful. The first investigator could not have anticipated that Induction, in spite of its apparent certainty, would prove so elusive to analysis.

Mill also was led, in a not dissimilar way, to attempt a too

[1] This view is taken in the edition of James Spedding and Leslie Ellis. Their introductions to Bacon's philosophical works seem to me to be very greatly superior to the accounts to be found elsewhere. They make intelligible, what seems, according to other commentaries, fanciful and without sense or reason.

simple treatment, and, in seeking for ease and certainty, to treat far too lightly the problem of justifying what he had claimed. Mill shirks, almost openly, the difficulties; and scarcely attempts to disguise from himself or his readers that he grounds induction upon a circular argument.

3. Some of the most characteristic errors both of Bacon and of Mill arise, I think, out of a misapprehension, which it has been a principal object of this book to correct. Both believed, without hesitation it seems, that induction is capable of establishing a conclusion which is absolutely certain, and that an argument is invalid if the generalisation, which it supports, admits of exceptions in fact. "Absolute certainty," says Leslie Ellis,[1] "is one of the distinguishing characters of the Baconian induction." It was, in this respect, mainly that it improved upon the older induction *per enumerationem simplicem*. "The induction which the logicians speak of," Bacon argues in the *Advancement of Learning*, "is utterly vicious and incompetent. . . . For to conclude upon an enumeration of particulars, without instance contradictory, is no conclusion but a conjecture." The conclusions of the new method, unlike those of the old, are not liable to be upset by further experience. In the attempt to justify these claims and to obtain demonstrative methods, it was necessary to introduce assumptions for which there was no warrant.

Precisely similar claims were made by Mill, although there are passages in which he abates them,[2] for his own rules of procedure. An induction has no validity, according to him as according to Bacon, unless it is absolutely certain. The following passage[3] is significant of the spirit in which the subject was approached by him: "Let us compare a few cases of incorrect inductions with others which are acknowledged to be legitimate. Some, we know, which were believed for centuries to be correct, were nevertheless incorrect. *That all swans are white, cannot have been a good induction, since the conclusion has turned out erroneous.* The experience, however, on which the conclusion rested was genuine." Mill has not justly apprehended the relativity of all inductive arguments to the evidence, nor the element of uncertainty which is present, more

[1] *Op. cit.* vol. i. p. 23.
[2] When he deals with Plurality of Causes, for instance.
[3] Bk. iii. chap. iii. 3 (the italics are mine).

or less, in all the generalisations which they support.[1] Mill's methods would yield certainty, if they were correct, just as Bacon's would. It is the necessity, to which Mill had subjected himself, of obtaining certainty that occasions their want of reality. Bacon and Mill both assume that experiment can shape and analyse the evidence in a manner and to an extent which is not in fact possible. In the aims and expectations with which they attempt to solve the inductive problem, there is on fundamental points an unexpectedly close resemblance beween them.

4. Turning from these general criticisms to points of greater detail, we find that the line of thought pursued by Mill was essentially the same as that which had been pursued by Bacon, and, also, that the argument of the preceding chapters is, in spite of some real differences, a development of the same fundamental ideas which underlie, as it seems to me, the theories of Mill and Bacon alike.

We have seen that all empirical arguments require an initial probability derived from analogy, and that this initial probability may be raised towards certainty by means of pure induction or the multiplication of instances. In some arguments we depend mainly upon analogy, and the initial probability obtained by means of it (with the assistance, as a rule, of previous knowledge) is so large that numerous instances are not required. In other arguments pure induction predominates. As science advances and the body of pre-existing knowledge is increased, we depend increasingly upon analogy; and only at the earlier stages of our investigations is it necessary to rely, for the greater part of our support, upon the multiplication of instances. Bacon's great achievement, in the history of logical theory, lay in his being the first logician to recognise the importance of methodical analogy to scientific argument and the dependence upon it of most well-established conclusions. The *Novum Organum* is mainly concerned with explaining methodical ways of increasing what I have termed the Positive and Negative Analogies, and of avoiding false Analogies. The use of exclusions and rejections, to which

[1] This misapprehension may be connected with Mill's complete failure to grasp with any kind of thoroughness the nature and importance of the theory of probability. The treatment of this topic in the *System of Logic* is exceedingly bad. His understanding of the subject was, indeed, markedly inferior to the best thought of his own time.

Bacon attached supreme importance, and which he held to constitute the essential superiority of his method over those which preceded it, entirely consists in the determination of what characters (or natures as he would call them) belong to the positive and negative analogies respectively. The first two tables with which the investigation begins are, first, the table *essentiae et praesentiae*, which contains all known instances in which the given nature is present, and, second, the table *declinationis sive absentiae in proximo*, which contains instances corresponding in each case to those of the first table, but in which, notwithstanding this correspondence, the given nature is absent.[1] The doctrine of prerogative instances is concerned no less plainly with the methodical determination of Analogy. And the doctrine of idols is expounded for the avoidance of *false* analogies, standing, he says, in the same relation to the interpretation of Nature, as the doctrine of fallacies to ordinary logic.[2] Bacon's error lay in supposing that, because these methods were new to logic, they were therefore new to practice. He exaggerated also their precision and their certainty; and he underestimated the importance of pure induction. But there was, at bottom, nothing about his rules impracticable or fantastic, or indeed unusual.

5. Almost the whole of the preceding paragraph is equally applicable to Mill. He agreed with Bacon in depreciating the part played in scientific inquiry by pure induction, and in emphasising the importance of analogy to all systematic investigators. But he saw further than Bacon in allowing for the Plurality of Causes, and in admitting that an element of pure induction was therefore made necessary. "The Plurality of Causes," he says,[3] "is the only reason why mere number of instances is of any importance in inductive inquiry. The tendency of unscientific inquirers is to rely too much on number, without analysing the instances. . . . Most people hold their conclusions with a degree of assurance proportioned to the mere *mass* of the experience on which they appear to rest; not considering that by the addition of instances to instances, all of the same kind, that is, differing from one another only in points already recognised as immaterial, nothing whatever is added to the evidence of

[1] Ellis, vol. i. p. 33.
[2] Ellis, vol. i. p. 89.
[3] Book iv. chap. x. 2.

the conclusion. A single instance eliminating some antecedent which existed in all the other cases, is of more value than the greatest multitude of instances which are reckoned by their number alone." Mill did not see, however, that our knowledge of the instances is seldom complete, and that new instances, which are not known to differ from the former in material respects, may add, nevertheless, to the negative analogy, and that the multiplication of them may, for this reason, strengthen the evidence. It is easy to see that his methods of Agreement and Difference closely resemble Bacon's, and aim, like Bacon's, at the determination of the Positive and Negative Analogies. By allowing for Plurality of Causes Mill advanced beyond Bacon. But he was pursuing the same line of thought which alike led to Bacon's rules and has been developed in the chapters of this book. Like Bacon, however, he exaggerated the precision with which his canons of inquiry could be used in practice.

6. No more need be said respecting method and analysis. But in both writers the exposition of method is closely intermingled with attempts to justify it. There is nothing in Bacon which at all corresponds to Mill's appeals to Causation or to the Uniformity of Nature, and, when they seek for the ground of induction, there is much that is peculiar to each writer. It is my purpose, however, to consider in this place the details common to both, which seem to me to be important and which exemplify the only line of investigation which seems likely to be fruitful; and I shall pursue no further, therefore, their numerous points of difference.

The attempt, which I have made to justify the initial probability which Analogy seems to supply, primarily depends upon a certain limitation of independent variety and upon the derivation of all the properties of any given object from a limited number of primary characters. In the same way I have supposed that the number of primary characters which are capable of producing a given property is also limited. And I have argued that it is not easy to see how a finite probability is to be obtained unless we have in each case some such limitation in the number of the ultimate alternatives.

It was in a manner which bears fundamental resemblances to this that Bacon endeavoured to demonstrate the cogency of his method. He considers, he says, "the simple forms or difference of things which are few in number, and the degrees and co-ordinations whereof make all this variety." And in *Valerius Terminus* he argues "that every particular that worketh any effect is a thing compounded more or less of diverse single natures, more manifest and more obscure, and that it appeareth not to which of the natures the effect is to be ascribed."[1] It is indeed essential to the method of exclusions that the matter to which it is applied should be somehow resolvable into a finite number of elements. But this assumption is not peculiar, I think, to Bacon's method, and is involved, in some form or other, in every argument from Analogy. In making it Bacon was initiating, perhaps obscurely, the modern conception of a finite number of laws of nature out of the combinations of which the almost boundless variety of experience ultimately arises. Bacon's error was double and lay in supposing, first, that these distinct elements lie upon the surface and consist in visible characters, and second, that their natures are, or easily can be, known to us, although the part of the *Instauration*, in which the manner of conceiving simple natures was to be explained, he never wrote. These beliefs falsely simplified the problem as he saw it, and led him to exaggerate the ease, certainty, and fruitfulness of the new method. But the view that it is possible to reduce all the phenomena of the universe to combinations of a limited number of simple elements—which is, according to Ellis,[2] the central point of Bacon's whole system—was a real contribution to philosophy.

7. The assumption that every event can be analysed into a limited number of ultimate elements, is never, so far as I am aware, explicitly avowed by Mill. But he makes it in almost every chapter, and it underlies, throughout, his mode of procedure. His methods and arguments would fail immediately, if we were to suppose that phenomena of infinite complexity, due to an infinite number of independent elements, were in question, or if an infinite plurality of causes had to be allowed for.

In distinguishing, therefore, analogy from pure induction, and in justifying it by the assumption of a *limited* complexity in the problems which we investigate, I am, I think, pursuing, with numerous differences, the line of thought which Bacon first

[1] Quoted by Ellis, vol. i. p. 41.
[2] Vol. i. p. 28.

pursued and which Mill popularised. The method of treatment is dissimilar, but the subject-matter and the underlying beliefs are, in each case, the same.

8. Between Bacon and Mill came Hume. Hume's sceptical criticisms are usually associated with causality; but argument by induction—inference from past particulars to future generalisations—was the real object of his attack. Hume showed, not that inductive methods were false, but that their validity had never been established and that all possible lines of proof seemed equally unpromising. The *full* force of Hume's attack and the nature of the difficulties which it brought to light were never appreciated by Mill, and he makes no adequate attempt to deal with them. Hume's statement of the case against induction has never been improved upon; and the successive attempts of philosophers, led by Kant, to discover a transcendental solution have prevented them from meeting the hostile arguments on their own ground and from finding a solution along lines which might, conceivably, have satisfied Hume himself.

9. It would not be just here to pass by entirely the name of the great Leibniz, who, wiser in correspondence and fragmentary projects than in completed discourses, has left to us sufficient indications that his private reflections on this subject were much in advance of his contemporaries'. He distinguished three degrees of conviction amongst opinions, logical certainty (or, as we should say, propositions known to be formally true), physical certainty which is only logical probability, of which a well-established induction, as that man is a biped, is the type, and physical probability (or, as we should say, an inductive correlation), as for example that the south is a rainy quarter.[1] He condemned generalisations based on mere repetition of instances, which he declared to be without logical value, and he insisted on the importance of *Analogy* as the basis of a valid induction.[2] He regarded a hypothesis as more probable in proportion to its *simplicity* and its *power*, that is to say, to the number of the phenomena it would explain and the fewness of the assumptions it involved. In particular a power of accurate prediction and of explaining phenomena or experiments previously untried is a just ground of secure confidence, of which he cites as a nearly perfect example the key to a cryptogram.[1]

[1] Couturat, *Opuscules et fragments inédits de Leibniz*, p. 232.
[2] Couturat, *La Logique de Leibniz d'après des documents inédits*, pp. 262, 267.

10. Whewell and Jevons furnished logicians with a storehouse of examples derived from the practice of scientists. Jevons, partly anticipated by Laplace, made an important advance when he emphasised the close relation between Induction and Probability. Combining insight and error, he spoilt brilliant suggestions by erratic and atrocious arguments. His application of Inverse Probability to the inductive problem is crude and fallacious, but the idea which underlies it is substantially good. He, too, made explicit the element of Analogy, which Mill, though he constantly employed it, had seldom called by its right name. There are few books, so superficial in argument yet suggesting so much truth, as Jevons's *Principles of Science*.

11. Modern text-books on Logic all contain their chapters on Induction, but contribute little to the subject. Their recognition of Mill's inadequacy renders their exposition, which, in spite of criticisms, is generally along his lines, nerveless and confused. Where Mill is clear and offers a solution, they, confusedly criticising, must withhold one. The best of them, Sigwart and Venn, contain criticism and discussion which is interesting, but constructive theory is lacking. Hitherto Hume has been master, only to be refuted in the manner of Diogenes or Dr. Johnson.

[1] Letter to Conring, 19th March 1678.