By the same author
INDIVIDUALS

Introduction to LOGICAL THEORY

by

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variations in conditions we shall think it necessary to take account of. We have to remember, too, the great complexity of the ways in which background beliefs may be related to foreground problems; how many general beliefs may bear upon the probability of a single event; how important are the analogies between one case and another, as well as the strictly deductive relations between beliefs; how relatively crude many of our ways of classifying phenomena are. When we bear in mind these things, it will not seem surprising either that no precise rules of general application can be formulated for the assessment of evidence or that no precise vocabulary is available for the description of its degrees. There are techniques of limited application (e.g., for collecting and interpreting statistics), but there is no general technique. We say, of a man who is good at weighing evidence in the ordinary affairs of life, that he has good judgment. The use of this very general, non-specialist, word is revealing. The man who is good at weighing evidence has not mastered the instructions for using some particularly intricate scales. His experience must be wide; but he is not, except incidentally, a specialist.

We must remember, moreover, that our assessment of evidence is an activity undertaken not primarily for its own sake, but for the sake of practical decision and action. Our use of words for grading evidence will in part reflect the degree of caution demanded by the action proposed. Evidence which the general public finds conclusive may not satisfy the judge.

II. THE 'JUSTIFICATION' OF INDUCTION

7. We have seen something, then, of the nature of inductive reasoning; of how one statement or set of statements may support another statement, S, which they do not entail, with varying degrees of strength, ranging from being conclusive evidence for S to being only slender evidence for it; from making S as certain as the supporting statements, to giving it some slight probability. We have seen, too, how the question of degree of support is complicated by consideration of relative frequencies and numerical chances.

There is, however, a residual philosophical question which enters so largely into discussion of the subject that it must be

discussed. It can be raised, roughly, in the following forms. What reason have we to place reliance on inductive procedures? Why should we suppose that the accumulation of instances of *A*s which are *B*s, however various the conditions in which they are observed, gives any good reason for expecting the next *A* we encounter to be a *B*? It is our habit to form expectations in this way; but can the habit be rationally justified? When this doubt has entered our minds it may be difficult to free ourselves from it. For the doubt has its source in a confusion; and some attempts to resolve the doubt preserve the confusion; and other attempts to show that the doubt is senseless seem altogether too facile. The root-confusion is easily described; but simply to describe it seems an inadequate remedy against it. So the doubt must be examined again and again, in the light of different attempts to remove it.

If someone asked what grounds there were for supposing that deductive reasoning was valid, we might answer that there were in fact no grounds for supposing that deductive reasoning was always valid; sometimes people made valid inferences, and sometimes they were guilty of logical fallacies. If he said that we had misunderstood his question, and that what he wanted to know was what grounds there were for regarding deduction *in general* as a valid method of argument, we should have to answer that his question was without sense, for to say that an argument, or a form or method of argument, was valid or invalid would *imply* that it was deductive; the concepts of validity and invalidity had application only to individual deductive arguments or forms of deductive argument. Similarly, if a man asked what grounds there were for thinking it reasonable to hold beliefs arrived at inductively, one might at first answer that there were good and bad inductive arguments, that sometimes it was reasonable to hold a belief arrived at inductively and sometimes it was not. If he, too, said that his question had been misunderstood, that he wanted to know whether induction in general was a reasonable method of inference, then we might well think his question senseless in the same way as the question whether deduction is in general valid; for to call a particular belief reasonable or unreasonable is to apply inductive standards, just as to call a particular argument valid or invalid is to apply deductive standards. The parallel is not wholly convincing;

for words like 'reasonable' and 'rational' have not so precise and technical a sense as the word 'valid'. Yet it is sufficiently powerful to make us wonder how the second question could be raised at all, to wonder why, in contrast with the corresponding question about deduction, it should have seemed to constitute a genuine problem.

Suppose that a man is brought up to regard formal logic as the study of the science and art of reasoning. He observes that all inductive processes are, by deductive standards, invalid; the premises never entail the conclusions. Now inductive processes are notoriously important in the formation of beliefs and expectations about everything which lies beyond the observation of available witnesses. But an *invalid* argument is an *unsound* argument; an *unsound* argument is one in which *no good reason* is produced for accepting the conclusion. So if inductive processes are invalid, if all the arguments we should produce, if challenged, in support of our beliefs about what lies beyond the observation of available witnesses are unsound, then we have no good reason for any of these beliefs. This conclusion is repugnant. So there arises the demand for a justification, not of this or that particular belief which goes beyond what is entailed by our evidence, but a justification of induction in general. And when the demand arises in this way it is, in effect, the demand that induction shall be shown to be really a kind of deduction; for nothing less will satisfy the doubter when this is the route to his doubts.

Tracing this, the most common route to the general doubt about the reasonableness of induction, shows how the doubt seems to escape the absurdity of a demand that induction in general shall be justified by inductive standards. The demand is that induction should be shown to be a rational process; and this turns out to be the demand that one kind of reasoning should be shown to be another and different kind. Put thus crudely, the demand seems to escape one absurdity only to fall into another. Of course, inductive arguments are not deductively valid; if they were, they would be deductive arguments. Inductive reasoning must be assessed, for soundness, by inductive standards. Nevertheless, fantastic as the wish for induction to be deduction may seem, it is only in terms of it that we can understand some of the attempts that have been made to justify induction.

8. The first kind of attempt I shall consider might be called the search for the supreme premise of inductions. In its primitive form it is quite a crude attempt; and I shall make it cruder by caricature. We have already seen that for a particular inductive step, such as 'The kettle has been on the fire for ten minutes, so it will be boiling by now', we can substitute a deductive argument by introducing a generalization (e.g., 'A kettle always boils within ten minutes of being put on the fire') as an additional premise. This manœuvre shifted the emphasis of the problem of inductive support on to the question of how we established such generalizations as these, which rested on grounds by which they were not entailed. But suppose the manœuvre could be repeated. Suppose we could find one supremely general proposition, which taken in conjunction with the evidence for any accepted generalization of science or daily life (or at least of science) would entail that generalization. Then, so long as the status of the supreme generalization could be satisfactorily explained, we could regard all sound inductions to unqualified general conclusions as, at bottom, valid deductions. The justification would be found, for at least these cases. The most obvious difficulty in this suggestion is that of formulating the supreme general proposition in such a way that it shall be precise enough to yield the desired entailments, and yet not obviously false or arbitrary. Consider, for example, the formula: 'For all f, g, wherever n cases of $f.g$, and no cases of $f.\sim g$, are observed, then all cases of f are cases of g.' To turn it into a sentence, we have only to replace 'n' by some number. But what number? If we take the value of 'n' to be 1 or 20 or 500, the resulting statement is obviously false. Moreover, the choice of any number would seem quite arbitrary; there is no privileged number of favourable instances which we take as decisive in establishing a generalization. If, on the other hand, we phrase the proposition vaguely enough to escape these objections—if, for example, we phrase it as 'Nature is uniform'—then it becomes too vague to provide the desired entailments. It should be noticed that the impossibility of framing a general proposition of the kind required is really a special case of the impossibility of framing precise rules for the assessment of evidence. If we could frame a rule which would tell us precisely when we had *conclusive* evidence for a generaliza-

tion, then it would yield just the proposition required as the supreme premise.

Even if these difficulties could be met, the question of the status of the supreme premise would remain. How, if a non-necessary proposition, could it be established? The appeal to experience, to inductive support, is clearly barred on pain of circularity. If, on the other hand, it were a necessary truth and possessed, in conjunction with the evidence for a generalization, the required logical power to entail the generalization (e.g., if the latter were the conclusion of a hypothetical syllogism, of which the hypothetical premise was the necessary truth in question), then the evidence would entail the generalization independently, and the problem would not arise: a conclusion unbearably paradoxical. In practice, the extreme vagueness with which candidates for the role of supreme premise are expressed prevents their acquiring such logical power, and at the same time renders it very difficult to classify them as analytic or synthetic: under pressure they may tend to tautology; and, when the pressure is removed, assume an expansively synthetic air.

In theories of the kind which I have here caricatured the ideal of deduction is not usually so blatantly manifest as I have made it. One finds the 'Law of the Uniformity of Nature' presented less as the suppressed premise of crypto-deductive inferences than as, say, the 'presupposition of the validity of inductive reasoning'. I shall have more to say about this in my last section.

9. I shall next consider a more sophisticated kind of attempt to justify induction: more sophisticated both in its interpretation of this aim and in the method adopted to achieve it. The aim envisaged is that of proving that the probability of a generalization, whether universal or proportional, increases with the number of instances for which it is found to hold. This clearly is a realistic aim: for the proposition to be proved does state, as we have already seen, a fundamental feature of our criteria for assessing the strength of evidence. The method of proof proposed is mathematical. Use is to be made of the arithmetical calculation of chances. This, however, seems less realistic: for we have already seen that the prospect of analysing the notion of support in these terms seems poor.

I state the argument as simply as possible; but, even so, it will be necessary to introduce and explain some new terms. Suppose we had a collection of objects of different kinds, some with some characteristics and some with others. Suppose, for example, we had a bag containing 100 balls, of which 70 were white and 30 black. Let us call such a collection of objects a *population*; and let us call the way it is made up (e.g., in the case imagined, of 70 white and 30 black balls) the *constitution* of the population. From such a population it would be possible to take *samples* of various sizes. For example, we might take from our bag a sample of 30 balls. Suppose each ball in the bag had an individual number. Then the collection of balls numbered 10 to 39 inclusive would be one sample of the given size; the collection of balls numbered 11 to 40 inclusive would be another and different sample of the same size; the collection of balls numbered 2, 4, 6, 8 . . . 58, 60 would be another such sample; and so on. Each possible collection of 30 balls is a different sample of the same size. Some different samples of the same size will have the same constitutions as one another; others will have different constitutions. Thus there will be only one sample made up of 30 black balls. There will be many different samples which share the constitution: 20 white and 10 black. It would be a simple matter of mathematics to work out the number of possible samples of the given size which had any one possible constitution. Let us say that a sample *matches* the population if, allowing for the difference between them in size, the constitution of the sample corresponds, within certain limits, to that of the population. For example, we might say that any possible sample consisting of, say, 21 white and 9 black balls matched the constitution (70 white and 30 black) of the population, whereas a sample consisting of 20 white and 10 black balls did not. Now it is a proposition of pure mathematics that, given any population, the proportion of possible samples, all of the same size, which match the population, increases with the size of the sample.

We have seen that conclusions about the ratio of a subset of equally possible chances to the whole set of those chances may be expressed by the use of the word 'probability'. Thus of the 52 possible samples of one card from a population constituted like an orthodox pack, 16 are court-cards or aces. This fact we allow ourselves to express (under the conditions, inductively established, of equipossibility of draws) by saying that the probability of drawing a court-card or an ace was $\frac{4}{13}$. If we express the proposition referred to at the end of the last paragraph by means of this use of 'probability' we shall obtain the result: The probability of a sample matching a given population increases with the size of the sample. It is tempting to try to derive from this result a general justification of the inductive procedure: which will not, indeed, show that any given inductive conclusion is entailed by the evidence for it, taken in conjunction with some universal premise, but will show that the multiplication of favourable instances of a generalization entails a proportionate increase in its probability. For, since *matching* is a symmetrical relation, it might seem a simple deductive step to move from

> I. The probability of a sample matching a given population increases with the size of the sample

to

> II. The probability of a population matching a given sample increases with the size of the sample.

II might seem to provide a guarantee that the greater the number of cases for which a generalization is observed to hold, the greater is its probability; since in increasing the number of cases we increase the size of the sample from whatever population forms the subject of our generalization. Thus pure mathematics might seem to provide the sought-for proof that the evidence for a generalization really does get stronger, the more favourable instances of it we find.

The argument is ingenious enough to be worthy of respect; but it fails of its purpose, and misrepresents the inductive situation. Our situation is not in the least like that of a man drawing a sample from a given, i.e., fixed and limited, population from which the drawing of any mathematically possible sample is equiprobable with that of any other. Our only datum is the sample. No limit is fixed beforehand to the diversity, and the possibilities of change, of the 'population' from which it is drawn: or, better, to the multiplicity and variousness of different populations, each with different constitutions, any one of which might replace the present one before we make the next

draw. Nor is there any *a priori* guarantee that different mathematically possible samples are equally likely to be drawn. If we have or can obtain any assurance on these points, then it is assurance derived inductively from our data, and cannot therefore be assumed at the outset of an argument designed to justify induction. So II, regarded as a justification of induction founded on purely mathematical considerations, is a fraud. The important shift of 'given' from qualifying 'population' in I to qualifying 'sample' in II is illegitimate. Moreover, 'probability', which means one thing in II (interpreted as giving the required guarantee) means something quite different in I (interpreted as a proposition of pure mathematics). In I probability is simply the measure of the ratio of one set of mathematically possible chances to another; in II it is the measure of the inductive acceptability of a generalization. As a mathematical proposition, I is certainly independent of the soundness of inductive procedures; and as a statement of one of the criteria we use in assessing the strength of evidence of a generalization, II is as certainly independent of mathematics.

It has not escaped the notice of those who have advocated a mathematical justification of induction, that certain assumptions are required to make the argument even seem to fulfil its purpose. Inductive reasoning would be of little use if it did not sometimes enable us to assign at least fairly high probabilities to certain conclusions. Now suppose, in conformity with the mathematical model, we represented the fact that the evidence for a proposition was conclusive by assigning to it the probability figure of 1; and the fact that the evidence for and against a proposition was evenly balanced by assigning to it the probability figure $\frac{1}{2}$; and so on. It is a familiar mathematical truth that, between any two fractions, say $\frac{1}{6}$ and $\frac{1}{5}$, there is an infinite number of intermediate quantities; that $\frac{1}{6}$ can be indefinitely increased without reaching equality to $\frac{1}{5}$. Even if we could regard II as mathematically established, therefore, it fails to give us what we require; for it fails to provide a guarantee that the probability of an inductive conclusion ever attains a degree at which it begins to be of use. It was accordingly necessary to buttress the purely mathematical argument by large, vague assumptions, comparable with the principles designed for the role of supreme premise in the first type of attempt. These assumptions, like those principles, could never actually be used to give a deductive turn to inductive arguments; for they could not be formulated with precision. They were the shadows of precise unknown truths, which, if one did know them, would suffice, along with the data for our accepted generalizations, to enable the probability of the latter to be assigned, after calculation, a precise numerical fraction of a tolerable size. So this theory represents our inductions as the vague sublunary shadows of deductive calculations which we cannot make.

10. Let us turn from attempts to justify induction to attempts to show that the demand for a justification is mistaken. We have seen already that what lies behind such a demand is often the absurd wish that induction should be shown to be some kind of deduction—and this wish is clearly traceable in the two attempts at justification which we have examined. What other sense could we give to the demand? Sometimes it is expressed in the form of a request for proof that induction is a *reasonable* or *rational* procedure, that we have *good grounds* for placing reliance upon it. Consider the uses of the phrases 'good grounds', 'justification', 'reasonable', &c. Often we say such things as 'He has *every justification* for believing that *p*'; 'I have *very good reasons* for believing it'; 'There are *good grounds* for the view that *q*'; 'There is *good evidence* that *r*'. We often talk, in such ways as these, of justification, good grounds or reasons or evidence for certain beliefs. Suppose such a belief were one expressible in the form 'Every case of *f* is a case of *g*'. And suppose someone were asked what he meant by saying that he had good grounds or reasons for holding it. I think it would be felt to be a satisfactory answer if he replied: 'Well, in all my wide and varied experience I've come across innumerable cases of *f* and never a case of *f* which wasn't a case of *g*.' In saying this, he is clearly claiming to have *inductive* support, *inductive* evidence, of a certain kind, for his belief; and he is also giving a perfectly proper answer to the question, what he meant by saying that he had ample justification, good grounds, good reasons for his belief. It is an analytic proposition that it is reasonable to have a degree of belief in a statement which is proportional to the strength of the evidence in its favour; and it is an analytic proposition, though not a

proposition of mathematics, that, other things being equal,[1] the evidence for a generalization is strong in proportion as the number of favourable instances, and the variety of circumstances in which they have been found, is great. So to ask whether it is reasonable to place reliance on inductive procedures is like asking whether it is reasonable to proportion the degree of one's convictions to the strength of the evidence. Doing this is what 'being reasonable' *means* in such a context.

As for the other form in which the doubt may be expressed, viz., 'Is induction a justified, or justifiable, procedure?', it emerges in a still less favourable light. No sense has been given to it, though it is easy to see why it seems to have a sense. For it is generally proper to inquire *of a particular belief*, whether its adoption is justified; and, in asking this, we are asking whether there is good, bad, or any, evidence for it. In applying or withholding the epithets 'justified', 'well founded', &c., in the case of specific beliefs, we are appealing to, and applying, inductive standards. But to what standards are we appealing when we ask whether the application of inductive standards is justified or well grounded? If we cannot answer, then no sense has been given to the question. Compare it with the question: Is the law legal? It makes perfectly good sense to inquire of a particular action, of an administrative regulation, or even, in the case of some states, of a particular enactment of the legislature, whether or not it is legal. The question is answered by an appeal to a legal system, by the application of a set of legal (or constitutional) rules or standards. But it makes no sense to inquire in general whether the law of the land, the legal system as a whole, is or is not legal. For to what legal standards are we appealing?

The only way in which a sense might be given to the question, whether induction is in general a justified or justifiable procedure, is a trival one which we have already noticed. We might interpret it to mean 'Are all conclusions, arrived at inductively, justified?', i.e., 'Do people always have adequate evidence for the conclusions they draw?' The answer to this question is easy, but uninteresting: it is that sometimes people have adequate evidence, and sometimes they do not.

[1] This phrase embodies the large abstractions referred to in Sections 5 and 6.

11. It seems, however, that this way of showing the request for a general justification of induction to be absurd is sometimes insufficient to allay the worry that produces it. And to point out that 'forming rational opinions about the unobserved on the evidence available' and 'assessing the evidence by inductive standards' are phrases which describe the same thing, is more apt to produce irritation than relief. The point is felt to be 'merely a verbal' one; and though the point of this protest is itself hard to see, it is clear that something more is required. So the question must be pursued further. First, I want to point out that there is something a little odd about talking of 'the inductive method', or even 'the inductive policy', as if it were just one possible method among others of arguing from the observed to the unobserved, from the available evidence to the facts in question. If one asked a meteorologist what method or methods he used to forecast the weather, one would be surprised if he answered: 'Oh, just the inductive method.' If one asked a doctor by what means he diagnosed a certain disease, the answer 'By induction' would be felt as an impatient evasion, a joke, or a rebuke. The answer one hopes for is an account of the tests made, the signs taken account of, the rules and recipes and general laws applied. When such a specific method of prediction or diagnosis is in question, one can ask whether the method is justified in practice; and here again one is asking whether its employment is inductively justified, whether it commonly gives correct results. This question would normally seem an admissible one. One might be tempted to conclude that, while there are many different specific methods of prediction, diagnosis, &c., appropriate to different subjects of inquiry, all such methods could properly be called 'inductive' in the sense that their employment rested on inductive support; and that, hence, the phrase 'non-inductive method of finding out about what lies deductively beyond the evidence' was a description without meaning, a phrase to which no sense had been given; so that there could be no question of justifying our selection of one method, called 'the inductive', of doing this.

However, someone might object: 'Surely it is possible, though it might be foolish, to use methods utterly different from accredited scientific ones. Suppose a man, whenever he wanted to form an opinion about what lay beyond his observation or the

observation of available witnesses, simply shut his eyes, asked himself the appropriate question, and accepted the first answer that came into his head. Wouldn't this be a non-inductive method?' Well, let us suppose this. The man is asked: 'Do you usually get the right answer by your method?' He might answer: 'You've mentioned one of its drawbacks; I never do get the right answer; but it's an extremely easy method.' One might then be inclined to think that it was not a method of finding things out at all. But suppose he answered: Yes, it's usually (always) the right answer. Then we might be willing to call it a method of finding out, though a strange one. But, then, by the very fact of its success, it would be an inductively supported method. For each application of the method would be an application of the general rule, 'The first answer that comes into my head is generally (always) the right one'; and for the truth of this generalization there would be the inductive evidence of a long run of favourable instances with no unfavourable ones (if it were 'always'), or of a sustained high proportion of successes to trials (if it were 'generally').

So every successful method or recipe for finding out about the unobserved must be one which has inductive support; for to say that a recipe is successful is to say that it has been repeatedly applied with success; and repeated successful application of a recipe constitutes just what we mean by inductive evidence in its favour. Pointing out this fact must not be confused with saying that 'the inductive method' is justified by its success, justified because it works. This is a mistake, and an important one. I am not seeking to 'justify the inductive method', for no meaning has been given to this phrase. *A fortiori*, I am not saying that induction is justified by its success in finding out about the unobserved. I am saying, rather, that any successsful method of finding out about the unobserved is necessarily justified by induction. This is an analytic proposition. The phrase 'successful method of finding things out which has no inductive support' is self-contradictory. Having, or acquiring, inductive support is a necessary condition of the success of a method.

Why point this out at all? First, it may have a certain therapeutic force, a power to reassure. Second, it may counteract the tendency to think of 'the inductive method' as something on a par with specific methods of diagnosis or prediction and therefore, like them, standing in need of (inductive) justification.

12. There is one further confusion, perhaps the most powerful of all in producing the doubts, questions, and spurious solutions discussed in this Part. We may approach it by considering the claim that induction is justified by its success in practice. The phrase ' success of induction ' is by no means clear and perhaps embodies the confusion of induction with some specific method of prediction, &c., appropriate to some particular line of inquiry. But, whatever the phrase may mean, the claim has an obviously circular look. Presumably the suggestion is that we should argue from the past ' successes of induction ' to the continuance of those successes in the future; from the fact that it has worked hitherto to the conclusion that it will continue to work. Since an argument of this kind is plainly inductive, it will not serve as a justification of induction. One cannot establish a principle of argument by an argument which uses that principle. But let us go a little deeper. The argument rests the justification of induction on a matter of fact (its ' past successes '). This is characteristic of nearly all attempts to find a justification. The desired premise of Section 8 was to be some fact about the constitution of the universe which, even if it could not be used as a suppressed premise to give inductive arguments a deductive turn, was at any rate a ' presupposition of the validity of induction '. Even the mathematical argument of Section 9 required buttressing with some large assumption about the make-up of the world. I think the source of this general desire to find out some fact about the constitution of the universe which will ' justify induction ' or ' show it to be a rational policy ' is the confusion, the running together, of two fundamentally different questions: to one of which the answer is a matter of non-linguistic fact, while to the other it is a matter of meanings.

There is nothing self-contradictory in supposing that all the uniformities in the course of things that we have hitherto observed and come to count on should cease to operate to-morrow; that all our familiar recipes should let us down, and that we should be unable to frame new ones because such regularities as there were were too complex for us to make out. (We may

assume that even the expectation that all of us, in such circumstances, would perish, were falsified by someone surviving to observe the new chaos in which, roughly speaking, nothing foreseeable happens.) Of course, we do not believe that this will happen. We believe, on the contrary, that our inductively supported expectation-rules, though some of them will have, no doubt, to be dropped or modified, will continue, on the whole, to serve us fairly well; and that we shall generally be able to replace the rules we abandon with others similarly arrived at. We might give a sense to the phrase ' success of induction ' by calling this vague belief the belief that induction will continue to be successful. It is certainly a factual belief, not a necessary truth; a belief, one may say, about the constitution of the universe. We might express it as follows, choosing a phraseology which will serve the better to expose the confusion I wish to expose:

> I. (The universe is such that) induction will continue to be successful.

I is very vague: it amounts to saying that there are, and will continue to be, natural uniformities and regularities which exhibit a humanly manageable degree of simplicity. But, though it is vague, certain definite things can be said about it. (1) It is not a necessary, but a contingent, statement; for chaos is not a self-contradictory concept. (2) We have good inductive reasons for believing it, good inductive evidence for it. We believe that some of our recipes will continue to hold good because they have held good for so long. We believe that we shall be able to frame new and useful ones, because we have been able to do so repeatedly in the past. Of course, it would be absurd to try to use I to ' justify induction ', to show that it is a reasonable policy; because I is a conclusion inductively supported.

Consider now the fundamentally different statement:

> II. Induction is rational (reasonable).

We have already seen that the rationality of induction, unlike its ' successfulness ', is not a fact about the constitution of the world. It is a matter of what we mean by the word ' rational ' in its application to any procedure for forming opinions about what lies outside our observations or that of available witnesses. For to have good reasons for any such opinion is to have good inductive support for it. The chaotic universe just envisaged, therefore, is not one in which induction would cease to be rational; it is simply one in which it would be impossible to form rational expectations to the effect that specific things would happen. It might be said that in such a universe it would at least be rational to refrain from forming specific expectations, to expect nothing but irregularities. Just so. But this is itself a higher-order induction: where irregularity is the rule, expect further irregularities. Learning not to count on things is as much learning an inductive lesson as learning what things to count on.

So it is a contingent, factual matter that it is sometimes possible to form rational opinions concerning what specifically happened or will happen in given circumstances (I); it is a non-contingent, *a priori* matter that the only ways of doing this must be inductive ways (II). What people have done is to run together, to conflate, the question to which I is answer and the quite different question to which II is an answer; producing the muddled and senseless questions: 'Is the universe such that inductive procedures are rational?' or 'What must the universe be like in order for inductive procedures to be rational?' It is the attempt to answer these confused questions which leads to statements like 'The uniformity of nature is a presupposition of the validity of induction'. The statement that nature is uniform might be taken to be a vague way of expressing what we expressed by I; and certainly this fact is a condition of, for it is identical with, the likewise contingent fact that we are, and shall continue to be, able to form rational opinions, of the kind we are most anxious to form, about the unobserved. But neither this fact about the world, nor any other, is a condition of the necessary truth that, if it is possible to form rational opinions of this kind, these will be inductively supported opinions. The discordance of the conflated questions manifests itself in an uncertainty about the status to be accorded to the alleged presupposition of the 'validity' of induction. For it was dimly, and correctly, felt that the reasonableness of inductive procedures was not merely a contingent, but a necessary, matter; so any necessary condition of their reasonableness had

likewise to be a necessary matter. On the other hand, it was uncomfortably clear that chaos is not a self-contradictory concept; that the fact that some phenomena do exhibit a tolerable degree of simplicity and repetitiveness is not guaranteed by logic, but is a contingent affair. So the presupposition of induction had to be both contingent and necessary: which is absurd. And the absurdity is only lightly veiled by the use of the phrase 'synthetic *a priori*' instead of 'contingent necessary'.