INDUCTION AND THE CALCULUS OF PROBABILITIES

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It is no longer thought that factual reasoning can be turned into a kind of valid syllogism, with the principle of the Uniformity of Nature as a major premiss. There are, however, those who hold that if we are allowed to make suitable assumptions about the constitution of nature, we can prove that some generalizations are at least highly probable. Others attempt to achieve the same result by relying on the *a priori* calculus of chances, or, what comes to much the same thing, on the logical theory of probability. I wish to examine these claims, beginning with those that appeal to the calculus of chances.

The key principle for any theory of this type is the Law of Large Numbers. This is the law that if in a large sample a given property occurs with the frequency $\frac{m}{n}$, there is a high probability that it occurs with approximately the same frequency in the parent population. It can, in fact, be mathematically demonstrated that this probability approaches certainty as the size of the sample increases. We must, however, be careful not to misunderstand the use of the word 'probability' here. What it refers to is the fact that if we take all possible selections from the parent population which yield a sample of the given size, those which roughly match the parent population with respect to the incidence of any given property must greatly outnumber those which do not. In other words, deviant samples are untypical, and become more untypical, the larger the samples.

Now there is one set of cases in which the argument from inverse probability, that is, the inference from the composition of the sample to the composition of the parent population, can be applied with complete safety. These are the cases in which the size of the sample is almost as great as the size of the population from which it is drawn. Suppose, for example, that we know what is the total number of births of children within a certain area over a given period, but that our statistics with regard to the sex distribution are not quite complete. At the same time, let us suppose that the percentage of cases for which our information is lacking is relatively small. Then whatever the proportion $\frac{m}{n}$ which is the average proportion of male births in our sample, we can be sure that the proportion in the total population does not differ from it very much, just because there are not enough unexamined cases left to make any great difference. By supposing all the unexamined cases male or all of them female we can calculate the limits.

But this conclusion, though secure, is of no interest to us. It tells us no more than we know already. The father who wants to know whether the child which his wife is carrying is more likely to be a boy or a girl learns nothing at all to his purpose from the information that the accumulated statistics already entail that the proportion of male births in the population to which his child is assigned is over 50 %. All he learns is that the figures have now reached a stage where the sex of his child is not going to make any appreciable difference to the final percentage. Not only can he deduce nothing whatsoever about his own child, since statements of frequency never in any event say anything about the individual case, but he can deduce nothing about the frequency in the sub-class of unexamined cases to which his child belongs. What is known as the law of averages applies only *ex post facto*. If we know both the final percentage and the recorded percentage, we can calculate what the percentage among the unexamined cases must be to make the sum come out right.

This example is one in which the size of the parent population is at least roughly known. But in the normal and indeed the only interesting type of case such knowledge is not available to us. Consider the stock example of the black ravens. We have observed a large number of black ravens and none that are not black and we wish to infer from this that the proportion of all ravens which are black is at least very near 100 %. But since we do not know what the total number of past, present and future ravens is, we do not know what fraction of the population is represented by our sample. Nevertheless the Law of Large Numbers assures us that the more numerous the sample in which we find all our ravens black and none not black the more unlikely it is that the proportion of black ones in the total population differs markedly from 100 %. Thus Sir Roy Harrod calculates, in his *Foundations of Inductive Logic,* that in a sample of 540, drawn from as large a population as you please, in which 95 % of the individuals have a property q, the probability that all the members of the sample will have q is less than one in a billion. In other words, the existence of such a sample makes it overwhelmingly probable that the incidence of q in the total population is higher than 95 %.

But what does this talk of probability come to here ? Just that if the incidence of q in the total population is as low as 95 %, our sample is very untypical. And why should it not be ? To return to the ravens, we can conclude from the evidence we have that if the proportion of black ravens in the total population is markedly different from 100 %, the instances which we have not examined must include a sufficient number of non-black ravens to redress the balance. And where has that got us ?

All the same, it will be argued, if there are all that many non-black ravens it is very unlikely that we should come across only the black ones. To estimate the force of this argument, let us consider it in the more general form in which it is advanced by Professor Donald Williams in his book *The Ground of Induction.* His presentation does not differ essentially from Harrod's but he does not limit himself to the cases in which the incidence in the sample of the character under investigation is 100 %.

"Any sample α", says Williams, "which we may have drawn from a population is itself one of large class W, of possible samples, to wit, the multitude of groups or sets, each of the same size as α, which are included in the population, and among these the overwhelming majority have the property (P) of matching the population. Hence, by the proportional syllogism, it is overwhelmingly probable that the actual sample α is one of those which match, and hence it is probable that the population matches it, that is, has approximately the same composition which we may now discern in the sample".

The argument of the proportional syllogism is that if $\frac{m}{n}$ of A's are B, and this is an A, there is a probability $\frac{m}{n}$ that this is a B. It is an argument that has to be handled cautiously, since it can easily lead to contradiction. By assigning an individual to different classes of reference we can derive incompatible values for the probability of its having a given property. The contradiction can be avoided only if the ascription of probability to the individual instance is taken merely as a restatement of the fact that the corresponding frequency obtains in a particular class of cases under which the instance falls.

Let us suppose then that the value of the fraction $\frac{m}{n}$ in William's example is $\frac{95}{100}$. Then what his argument comes to is that if we ascribe the property of being a B to each A throughout the whole range of A's, we shall be right 95 times out of 100. In this way, he is able to calculate that of all the possible samples of 2500 members apiece which are included in any given population, the proportion which match the population within 1 % must be at least .6826, while the proportion which match within 2 % cannot be less than .9545, and so forth. This is calculated with respect to a 50 % distribution which yields the least favourable results. For other percentages the proportion of matches would be higher, and we have seen that the Law of Large Numbers ensures that, for any percentage, an indefinite increase in the size of the sample makes the proportion tend to unity.

There is nothing wrong with these calculations in themselves. But in order to apply them, we need more than the assurance that the majority of possible samples match the population. We have to take it to be probable that the samples which we actually obtain belong to this majority. For this purpose, it has to be assumed that any one sample is antecedently as likely to be selected as any other. Williams sees that this assumption is needed but maintains that it is an innocent truism. It is an innocent truism in the sense that if probability is measured by the ratio which selections of a certain type bear to the total number of possible selections, each selection counts for one and only one. An angel who made every possible selection would necessarily find that most of his samples were typical.

But the truth is that we are not in this position. The Briareus model, according to which the population is sorted into bags and we simultaneously dip a hand into each one, does not fit the facts. So far from its being the case that we are as likely to make any one selection as any other, there are a vast number of selections, indeed no doubt the vast majority, that it is impossible for us to make. Our samples are drawn from a tiny corner of the universe during a very short period of time. And even this tiny section of the whole four-dimensional continuum is not one that we can search at all thoroughly.

For these reasons, we need to make two rather large empirical assumptions:

1) That the composition of our selections, the states of affairs which we find out and note down, reflects the composition of all the selections which are available to us, that is, the states of affairs which we could observe if we took enough trouble: in short, that the volumes of the book of nature which have already been published have been correctly read and transcribed.

2) That the distribution of a given property within a given population is approximately even in space and time, in the sense that the ratio of its incidence in a sufficiently large segment of space-time approximately reflects the ratio of its incidence throughout.

The first of these assumptions may be fairly readily conceded. It is indeed the purpose of scientific method to see that it holds. The reason why instances are not only multiplied but also varied, the reason why it is important to try out hypotheses under different conditions, is to make sure, so far as possible, that our selections are not biassed by the circumstances in which they are made.

It is the second assumption that gives trouble. For plainly it postulates the uniformity of nature, not trivially but in quite a strong form. Indeed, it goes beyond what would ordinarily be believed. We should not consider it a matter for surprise that many things were very different in other parts of the universe, or even that our earth and the behaviour of things upon it should change very radically in the course of the next million years. Certainly it would not at all astonish us to learn that birds otherwise resembling ravens had a different pigmentation in some remote part of space or time, however strong a local sample we had been able to build up.

But, it may be said, it does not really matter to us what goes on in the outer galaxies or what is going to happen on earth in a million years time. We are seriously interested only in our fairly immediate environment and in the fairly immediate future. After all, the main purpose of inductive logic is to assess the reliability of hypotheses on which we want to act. So that the fact that our selections may be biassed with respect to the Universe as a whole is not really of any great importance.

I think that this point is well taken, but it does not save the argument. For now we are faced with a further difficulty. If we confine ourselves to the next hundred years or so, and to our habitual environment, then so long as the rate of growth of the population in which we are interested does not enormously increase, we can be certain, and this without making any further assumptions, that in many cases the percentages up to the end of that time will not be very different from what they are up to now. This will be true in all those cases in which we have built up such a backlog of instances that they are bound to swamp the new instances, however deviant these are. But, as I said before, this conclusion is of no value to us. For we are interested in the maintenance of a percentage only in so far as it affects the new instances. We do not want to be assured that the final result will be the same even if the further instances are deviant. If we make the time short enough, we know this anyway. We want to be assured that the new instances will not be deviant. But for this we do need our non trivial assumption of uniformity.

From this it appears that the attempt to justify induction on the basis of the calculus of chances fails unless it is supported by a principle of fair sampling. But then we are back with the problem not only of justifying such a principle, and it is hard to see in what terms it could be justified, but even of finding a satisfactory way of formulating it.

Perhaps, however, the need for assuming a principle of this kind can be avoided if we are willing, as Harrod and Williams are not, to introduce initial probabilities. This is the line taken by Carnap in his development of induction. I do not propose to give a general account of Carnap's system, but only to refer to certain essential features of it which are especially relevant to the present argument.

Carnap's method is to construct a simplified model which can, he hopes, eventually be applied to the actual world. For the materials of his model, he takes an artificial language, with an infinite number of individual constants, a finite number of primitive predicates, and the usual truth-functional connectives and quantifiers.

It is stipulated that the primitive predicates should be logically independent of one another. This has the rather startling consequence that there can be only one such predicate in the language which falls under any one determinable; for instance, only one colour predicate, since if there were more than one the condition of independence would be violated. In the same way, quantitative predicates are excluded. These are serious restrictions if Carnap's artificial language is to be a satisfactory model for any actual language. I shall not here enter into the question how far they can be overcome.

Now it is plain that we can get a complete description of the universe corresponding to Carnap's universe of discourse by stating with respect to each individual in it whether or not it has each of the primitive predicates. If the number of the individuals is infinite the description cannot be written out, but with respect to any finite sub-section of the universe it can, though the process might be very laborious. Let us take as an example a very small universe with three individuals, *a, b, c,* and two predicates, say the predicates 'red' and 'round'. Then the statements *a* is red and not round, *b* is red and round, *c* is not red and not round, describe a complete state of this universe. The statements *a* is red and not round, *b* is round and not red, *c* is red and not round describe an alternative state. Carnap calls any such assignment of the primitive predicates or their negations to the individuals of his system a State-Description of the system.

Two such state-descriptions may differ, not in the distribution of their primitive properties but only with respect to the individuals to which their properties are ascribed. Thus, in our simplified example, the state-descriptions "*a* is round and not red, *b* is round and red, *c* is red and not round" and "*a* is red and not round, *b* is round and not red, *c* is round and red" satisfy this condition. When two or more state-descriptions stand in this relation to one another they are said to be Isomorphic. A disjunction of state-descriptions which are mutually isomorphic is called a Structure-Description.

Carnap lays down the convention that in the case of any language representing a universe of discourse in which there are a finite number of individuals, every state-description receives from purely tautological evidence a degree of confirmation greater than 0. Since tautologies are construed as saying nothing about any matter of fact, this means that by convention every state-description is assigned some initial degree of confirmation. Furthermore, since every sentence in such a language can be written out as a disjunction of state-descriptions, it follows that an initial degree of confirmation is assigned to every statement which the language has the resources to express. The initial degree of confirmation of any molecular sentence is the sum of the initial degrees of confirmation of the state-descriptions which it disjoins. In the case of a logically true statement which disjoins all the state-descriptions this sum reaches a maximum of 1.

Given these conventions, it can be proved that in the case of any language L_n, where the number n of individual constants is finite, the degree of confirmation c of a hypothesis h on evidence e is always equal to the initial confirmation of h and e divided by the initial confirmation of e. In the case where the number of individual constants is infinite, the degree of confirmation is defined as the limit to which the values of $c(h,e)$ converge, if they do converge, as n increases.

It follows that all that we need to do in order to be able to calculate the degree of confirmation which any given body of evidence lends to any given hypothesis is to assign initial degrees of confirmation to the state-descriptions. As Carnap sees it, this is purely a matter for decision. Our aim is to arrive at assessments of probability which are intuitively acceptable.

His own choice, in *The Logical Concept of Probability,* is to assign equal initial measures not to state-descriptions but to structural-descriptions. From this he derives, as the initial degree of confirmation of any state-description *S*, the formula $\frac{1}{tz}$ where *t* is the number of structural descriptions in the language and *z* the number of state-descriptions which are isomorphic with *S*. This results in a system which is heavily weighted on the side of uniformity. The likelihood that a given individual possesses a given combination of properties is very much increased by the evidence that other individuals possess it. In this way, when a universal hypothesis has been found to be satisfied in a large number of instances, a high degree of confirmation accrues to the proposition that the next instance will also satisfy it. On the other hand, it is a somewhat startling feature of Carnap's system that the probability that a universal hypothesis holds in a universe with an infinite number of individuals, no matter what the evidence, is always 0. This has the strange consequence that a kind of ontological argument is valid in the system. Given any non-contradictory combination of the predicates which it contains, it is certain that at least one individual possesses them.

I find this consequence counter-intuitive, but it is not on this ground that I mainly object to the system which generates it. My concern is not so much with Carnap's assessment of the initial degree of confirmation as with the notion itself. What can possibly be meant by saying that a given statement is confirmed by any tautology, that is by zero evidence, to such and such a degree ? The answer is that this is a purely formal assertion. It follows analytically from the syntax of the language. But what has this to do with what is actually likely to happen ? What are we to understand by the assumption that every structural description is equally probable, if this is to be construed not just as the truism that in the enumeration of structural descriptions each one counts as one, but as furnishing a guide to our expectations ? A purely formal concept of probability will not serve us here. We need a material concept for which the theory makes no provision.

How then can we provide for it ? The assumption which leads Carnap to assign equal initial measures to structural descriptions is that it is equally probable that any given individual has any given primitive property. Translated into empirical terms, this would appear to mean that whatever the proportion in which a primitive property is distributed among the total population, we are safe in assuming that it is distributed in roughly the same proportion in any reasonably large sample. But this is just the same principle as we have seen to be required by those who try to found induction on the *a priori* calculus of chances. And just the same difficulties arise.

Oxford A. J. AYER

A. J. AYER

I'm going to follow Professor McKeon's excellent example and not read my paper, but make a few remarks about it, and I'll make my remarks pretty short, as I am warned by Professor Russell that Professor Juhos' paper is rather long.

This is an unambitious paper. It tries to make one small point, and that is a negative one, and it falls under the general heading, I suppose, of problems of justification. It deals with one of the cases that Professor Poznanski brought out yesterday, where justification is justification of a method, of a set of rules, or would-be rules. Because one of the problems in the case of induction is that we don't know the rules, and it is all clear even if there are any. It's also, I think, a very interesting case from the point of view of Professor Perelman, in that this is one of the instances where philosophers have always tried to make justification a case of demonstration, and probably have gone wrong for that reason. I want to make a few historical remarks about that.

The problem was, you will know, really «mis au point» by Hume, and in a most brilliant way, it's a most marvellous piece of philosophical argumentation. He did, however, make one mistake in which he was followed by Kant. And the mistake was continued by John Stuart Mill. Both Hume and Kant appear to have thought that induction could be turned into demonstration if only you had the right major premiss, the premiss of the uniformity of nature. And Hume was sceptical, because he said «such a premiss can't be known, and can't be proved without circularity», and Kant said «oh no, I can prove it». But, of course, both were wrong in as much as this major premiss will do you no good. That is to say, it is impossible to have a syllogism of the form. Nature is uniform, all observed *a* is *b*, therefore all *a* is *b*. And this can be shown very neatly in a way that I haven't seen elsewhere, but it's a very simple argument. Let's take an actual example where we have a true minor premiss. Let's take the old example of the swans. So, the syllogism is going to run now — Nature is uniform, major premiss. Minor premiss: all observed swans are white. Now, this has to be true, so let's make the date of our statement 1700. So, all swans observed before 1700 are white. Therefore all swans are white. Since the conclusion is false, and this is a valid syllogism, one of the premisses must be false. Ex hypothesi the minor premiss is true, therefore the major premiss is false, therefore the discovery of a black swan refutes the uniformity of nature. By this argument, if Hume and Kant had been right, we would have proved, absolutely correctly, that nature is not uniform. Since nobody draws this conclusion, it follows that the principle of the uniformity of nature doesn't figure, and can't figure, as the major premiss in a demonstration, and in fact no such demonstration is possible.

To do philosophers justice, since John Stuart Mill, nobody of any consequence has thought that the demonstration is possible. But people dc still think that you can do this demonstratively if you are a little more cautious; if, instead of saying «all swans are white», you say «probably all swans are white», it is thought that this can still come out as the conclusion of a demonstration. And my paper was really an attempt to explode this, to show that it is a blind alley. What happens next I'm not really so clear about.

The attempted demonstration, as I show in my paper, may proceed in two ways: either directly from the calculus of probabilities, with or without some special assumptions about nature, or through the construction of a linguistic model, as is done by Carnap. I want to say a little bit about each of these, for the most part summarizing what I have said in my paper. Now the first way is made to look quite plausible because of the part played by the Law of Large Numbers. It is mathematically demonstrable, there is no question at all about that, that the larger the sample, the smaller the number of deviant cases, and as we increase the samples, this approaches unity asymptotically. I mean that when you get very large samples, then it is, in this sense, virtually certain that the distribution of a property in the sample, matches its distribution in the parent population. And this looks to be just what we want. It seems to bring us home. All we need do is increase our samples. Until you begin to look at what is meant by «virtually certain» here. For what is meant by «virtually certain» is, as in all these discussions, really something that has only a syntactical meaning. What «virtually certain» means simply is that if you take all possible samples, then, mathematically, the number of deviant samples diminishes with the size of the samples and diminishes towards zero. But, of course, this has no bearing at all on what is likely to happen, unless you translate this purely formal principle into something that is going to be empirically applicable. And what you need here, as I show in my paper, is a very strong principle of fair sampling. Taking Williams and Harrod, as my examples, both because their work is fairly recent, and because I think that they do this sort of thing as well as anybody else that I know, I think I succeed in showing that, for them to get what they want, they need to make the two large empirical assumptions that I mentioned on my page 5. First that the composition of our selections reflects the composition of all the selections which are available to us at the present time, an assumption which I allow to pass. I think that what is called scientific method, is very largely a matter of assuring that this first assumption is satisfied. This is the reason why we vary our instances, conduct experiments in different circumstances, etc. And the second assumption, which is a very strong one indeed, is that the distribution of a given property within a given population is approximately even in space and time, in the sence that the ratio of its incidence in a sufficiently large segment of space-time approximately reflects the ratio of its incidence throughout. Well, this assumption, I say, is an extremely strong one, and in fact stronger than we should ordinarily believe. Because I don't think we ordinarily believe that the composition of the world as we find it around us, is necessarily reflected throughout all space and time. But even if you restrict it to what we are interested in, namely projection over the neighbouring regions of space-time, you still, I think, need a very strong assumption of this kind. And I'm not even quite sure how to formulate the principle of fair sampling that we need, except in a totally question-begging way. Namely, we need to have the assurance that just those hypotheses we want to project, over just the areas we want to project them, are going to come out right. And, of course, this is indeed exactly what we want. But this is very much begging the whole question.

I then pass to Carnap, which I think is a very interesting case indeed, and I don't think it has yet been brought out in discussions of Carnap, how much his system is simply a variant of the other one. How much Carnap's system simply is a way of working with the calculus of probabilities, the main difference being of course that, instead of making explicit assumptions, he builds everything into his language. From this point of view, it's interesting to see that all Carnap's judgements of probability, of confirmation, are entirely *a priori*. Every single assessment of confirmation in Carnap depends logically on the assignment of the initial degrees of confirmation. The only factual thing is what evidence you have. I mean that it's a question of fact what constitutes our evidence, what propositions in the system turn out to be materially true, but given that, all the assessments of confirmation are totally independent of anything that happens. They are all simply built into the structure, the language. And it is a language very heavily weighted, as I show, on the side of uniformity. If you take my simple universe, the structure-description a, b, c all red has no isomorph, whereas a, b red, c not red has three isomorphs. And, in this way, you see that since the probability varies inversely with the number of isomorphs as well, of course, as with the predicates, this means that you have a strong bias in favour of uniformity which you build into your language in this way. There is I think one point which hasn't yet been made about Carnap, and again this surprises me, because it is very obvious, and that is you can prove the ontological argument in his system. Or rather, you can prove the ontological argument in Carnap provided you allow God to be defined in his system. You see, it can be proved in Carnap's system, and he himself admits it, that all universal propositions have probability zero. Which means that the negation of any universal proposition has probability one, so if the proposition «there is no perfect being», which is universal one, is statable in Carnap's system, its negation that there is a perfect being, is certain. In fact, any combination of the predicates allowed in Carnap's system turns up at least once. So, if he allows, for example, the predicates of being a chess-player and being a mosquito to be constructible in the system, then somewhere in the world there is a chess-playing mosquito. On the other hand, it's also almost certain that we never find it. And equally again, if there is a perfect being, it's infinitely improbable we shall ever come across him. Still, there is some comfort for those who want to believe in a perfect being that in Carnap's system he exists if his concept is allowed into it. Of course, Carnap would keep him out: he would not introduce such predicates. Nevertheless this is obviously a very great «Schönheitsfehler» in Carnap's system. But it isn't that which I reproach the system for. It is rather the whole conception of initial degrees of confirmation that worries me. I don't really see what this means. I see what it means syntactically. That is to say, within the system, to say that a sentence has an initial degree of confirmation, $\frac{m}{n}$, is simply a matter of saying: it is one of such and such a number of structural descriptions which have so many isomorphs. This is a perfectly good syntactical statement. What this comes to when we try to translate it into terms of application to the world, is very much more obscurs. I suggest that what it comes to, is that it's equally probable that any individual has any given primitive property. And, of course, this is again just the same principle of fair sampling, as we have seen to be needed by Williams, Harrod, and their school.

Well, as I said when I started talking, this is only a negative point. I think it's quite an important one, however, if it enables us to clear this kind of approach out of the way. I think it is important to show, once for all, that this way of trying to demonstrate the validity of inductive processes won't work. But, of course, it is much more interesting to know where we go from here. And my suggestion, I think, would be that we should perhaps stop looking for anything like a general justification of induction. One reason for this, in my view, is that there is no one set of methods that could count as inductive. Justification is always a much more limited affair. For example you justify a particular predication by adducing a general proposition. And then you may justify the general proposition by adducing a theory. And how do you justify the theory ? Perhaps by all sorts of things, the kinds of things that Professor Bunge is going to tell us about this afternoon. And I must say that, after talking about my paper, I rather wish I had written his.

B. Juhos

The problem investigated by Professor Ayer concerns the question, whether we are allowed to infer from apriori suppositions of the calculus of chances or of the logical theory of probability some statements about reality, especially some general statements which have the character of natural laws. The attempts to derive generalizations about natural phenomena from the principles of the calculus of chances make use, as shown by Professor Ayer, of the socalled «Law of Large Numbers». This «Law» is a purely mathematical principle and within the mathematical theory of chances it is logically justified to conclude that a given property, if it occurs with the frequency m/n in a large sample, will occur with a high probability with approximately the same frequency in the parent population. But here «probability» is nothing else than a relation between the extensions of defined concepts and thereby the question arises how is it possible to induce from a system of formal relations regularities of empirical phenomena ? Ayer analyzes examples of such derivations as given in the theories of Sir Roy Harrod and Donald Williams. He comes to the conclusion that the calculations in themselves are correct, surely we can logically derive from the supposed relations of sets that the majority of possible samples match the population. But that does not imply anything, as Professor Ayer pointed out, about the probability that the samples which we actually obtain belong to this majority. By analyzing his examples Ayer shows that we need to make two rather large empirical assumptions, in order to achieve statements about the probability of empirical events by application of the calculus of chances.

The second of the two assumptions is nothing else than the postulation of the uniformity of nature in a rather extensive degree. And we can not avoid this suppositions, as Ayer demonstrates, even if we confine our exemples to relatively narrow realms of the space-time continuum. So Ayer is right, I believe, maintaining that the attempt to justify induction on the basis of the calculus of chances fails and is fallacious. The derivation can be reached only by using empirical principles which are problematic in their content and validity. It is even hard to find a satisfactory formulation for them.

An other attempt to justify inductive generalizations in empirical science by apriori logical elements is Carnap's theory of «initial probabilities». Carnap's suppositions are discussed by Professor Ayer from the point of view whether the derivation by which Carnap tries to show that the stipulations of this theory are a guide of our empirical expectations does not make use of tacit presuppositions. «Initial probability» is, as defined by Carnap, the probability given to a statement by a tautology. That is, however, a purely formal assertion, as Ayer rightly comments. The further assumption of Carnap that equal initial measures of probability are to assign to the socalled «structural descriptions» implies the supposition that it is equally probable, that any given individual has any given primitive property. Applied to empirical reality this supposition means, as I believe Ayer is right in maintaining, «that whatever the proportion in which a primitive property is distributed among the total population, we are safe in assuming that it is distributed in roughly the same proportion in any reasonably large sample». But this again means nothing else than the assumption of the uniformity of nature, so that Carnap's derivation of the inductive generalization in empirical science from the apriori theses of his inductive logic, implies tacit empirical assumptions too.

These results of Ayer's investigation lead to the general question, what is the reason for the failure of the attempts of deriving empirical induction and inductive generalization from logical systems of probability ? All the derivations discussed by Ayer can not avoid empirical assumptions, especially the assumption of the uniformity of nature, which contain even the thesis to be derived. We have to ask here has this failure of the derivations the character of logical necessity or do only the special examples investigated by Professor Ayer contain mistakes which possibly could be corrected ? I think in relation to these questions the explanations of Professor Ayer should be supplemented.

The belief that inductive generalizations as practised in empirical science can be justified by the calculus of chances or by the apriori principles of inductive logic is a result, to my opinion, of the confusion of two different meanings of «probability» or of the statements about probability respectively. There are many possibilities of construing axiom-systems of inductive logic. These systems will contain the traditional calculus of chances and the modern alterations of this calculus too. From the epistemological point of view such systems are nothing else than systems of analytical relations between the formal extensions of selected and defined concepts. They can be represented in the form of special set-theories or in the form of possible mathematical extra- and interpolations. All the modern systems of inductive logic are systems of special forms of mathematical extra- and interpolation. These conclusions often used in mathematics are not deductive derivations, but the prototypes of logical induction. It is, however, logically impossible to derive any statements about reality from analytically selected and stipulated relations. The concept of «logical probability» as used in inductive logic and in the calculi of chances expresses analytically constructed and stipulated relations between the extensions and parts of the extensions of defined classes, sets, or concepts in general. This «probability» can be represented also as relations between the statements of constructed language systems concerned. But from these analytical constructions nothing can be derived about the statistical order of real phenomena. It is an error to believe that the empirical probability of events, as it is recognized by observation, by statistical counting, could be derived from the stipulated formal relations of a system of inductive logic, because it is possible to apply the formulae and the rules of logical or mathematical systems of chances to the empirical probability values. We are concerned here with two concepts of probability and it is a matter of principle that it is not possible to derive from the suppositions, the selected and stipulated axioms and rules of any analytically constructed system its validity for reality.

The situation resulting from the distinction of logical and empirical probability can be illustrated by comparison with an analogous example well known in natural philosophy. The opinion that geometry describes the «space», in which all events happen, had to be dropped when the possibility of different geometrical systems incompatible with each other was recognized. Epistemological analysis showed that the geometrical systems of mathematics are purely analytical constructions and that it is therefore impossible to derive from the relation system of any of the mathematical geometries that it must be valid for the physical space. The description of the geometrical metric of the real space is attained in physics by measuring observation. Besides this empirical method it is possible to fix a geometrical metric for the physical space by convention and then to investigate by observation whether the objects and events correspond in their spatial order to the metric selected by convention. Both methods show that the geometry of the real space expresses a specific order of empirical phenomena and can be controlled by the methods of empirical investigation. Consequently we have to discriminate the analytic concept of mathematical spaces and the empirical concept of physical space.

Transfering this discrimination to the problem of probability we are now in the position to specify the meaning of the concepts of probability. As mentioned above «probability» as defined in inductive logic and the calculus of chances means nothing else than formal syntactical relations between the analytically constructed extensions of concepts or the corresponding statements respectively. In opposition to that, «probability» in empirical description characterizes an objective order of events which can be expressed in form of natural laws. The type of the order of phenomena described by probability laws in physics is formally different from the form of the order described by the so-called causality-laws, e.g. the «proximity-effect-laws». The latter ones maintain univocal, in their strict form one-to-one relations between the empirical states, whereas probability-laws describe non-univocal (one-to-many, many-to-one, many-to-many) relations between the phenomena. Both types of order can be expressed by mathematical functions. From the epistemological view probability-laws are therefore natural-laws of the same kind as the strict causality-laws. And the values of «empirical probability» ascribed to the phenomena are predicates of the same kind as the values of length, duration, mass, weight, energy and so on. They can be controlled equally by methods of observation.

Applying the calculus of chances or the formulae of inductive logic to real phenomena it is of course possible to join the analytical relations to certain empirical predicates. Thereby it is possible to treat the probabilities empirically ascribed to the real states according to the rules of logical or mathematical systems of chances. But in every case the validity of the (non-logical) statements about the probability of real events obtained by this method can be controlled and decided only by empirical probation. The application of logical and mathematical theories of probability in empirical description does not justify us to abolish the duality of the two probabilities and to unite the different meanings of the two concepts in one notion of probability. If we do so, as it is done when trying the derivation of the principle of the uniformity of nature from formal premises apriori, then we make a logical fault, called «equivocation», and the result is a false conclusion. That, I believe, was explained in an impressive manner by the examples discussed by Professor Ayer.

I think it must be mentioned, in order to complete our investigations, that Carnap has distinguished between two concepts of probability in certain analogy to our logical and empirical probability. He calls them «probability$_1$» and «probability$_2$». «Probability$_1$» he defines by the same criteria as we characterize the «logical probability». By «probability$_1$» we have to understand analytical relations between the extensions of the selected and defined concepts, sets, classes, or the corresponding statements respectively. Inductive logic concerns «probability$_1$» alone. «Probability$_2$» is called by Carnap «statistical probability» also, but he characterizes it by criteria which do not agree with the meaning of our «empirical probability». If series of average frequencies have the tendency to converge to limit values, then these convergent sequencies or their limit values, respectively, shall be called «probability$_2$», i.e. «statistical probability». At this point must be objected that convergent series of average frequencies are defined by mathematical functions in the same manner as any other mathematical metric of probability. Therefore according to the definition of «probability$_2$» given by Carnap this concept is again an analytical relation between constructed extensions, and is only a specific form of «probability$_1$». In opposition to that our «empirical probability» is not a formal relation between constructed concepts but characterizes an order of real phenomena and can be controlled by empirical observation.

A. J. Ayer

There is little I have to say about that, partly because it was so much in agreement. I think there are just three points that occur to me.

First, Professor Juhos says quite correctly that you can't derive propositions about the world from purely formal propositions and this is then illustrated in Carnap's system by the fact that his probability statements aren't about the world. In Carnap's system all probability statements are analytic statements, namely that such and such evidence stands in a purely formal relation to this or that hypothesis. They aren't about the world at all.

Secondly, I entirely agree with Professor Juhos that Carnap's probability two is only a specific form of probability one; therefore it is open to the same objections.

The third point which Professor Juhos didn't develop, I wish he had more, is the analogy with geometry. I am not at all sure how fruitful it is. Of course one can apply the probability calculus. No one denies this: in gambling games, for example, when you make the assumption that the actual empirical frequencies of dice, cards, etc., correspond to the *a priori* possibilities. Then, of course, you can apply the probability calculus. But whether there are any analogies to non Euclidian geometry in the theory of probability, I'm rather doubtful.

B. Juhos

In his comments to my explanations Professor Ayer mentions three points which seem to him remarkable. Upon the first two points we agree completely. Concerning the third point Ayer believes that the analogy between the geometrical systems and the systems of probability is at best a rather superficial relation, in so far one can apply the analytical systems of geometry and of probability resp., in order to describe orders of empirical facts. But Professor Ayer is rather doubtful «whether there are any analogies to non-Euclidian geometry in the theory of probability». I think, however, that there is a far reaching logical analogy between the conceptual order of the geometrical systems on the one side and the corresponding order of the systems of probability on the order side. It is of course not an analogy of the inductive logical systems to «non-Euclidian geometry» alone what I mean, but to the logical order of *all* geometrical systems, the Euclidian as well as the non-Euclidian. One can illustrate this relation in the following manner.

The order of the (Euclidian and non-Euclidian) geometrical systems can be characterized, as *Gauss* and later on in a more general manner *Riemann* have demonstrated, by a mathematical expression called «measure of curvation» («Krümmungsmass») or «tensor of curvation» («Krümmungstensor»). As values of this function all real numbers from $-\infty$ to $+\infty$ are possible. Each value of the curvation-tensor characterizes one certain geometrical system. The negative values characterize the «hyperbolical», the positive values the «elliptical» systems and the value O characterizes the Euclidian geometrical metric. An analogue continuous order of the systems of probability is characterized, as Johnson, Carnap and Kemeny have demonstrated, by the function:

$$G(s_i,s_n,k,w,\lambda) = \frac{s_i + \lambda\frac{k}{w}}{s_n + \lambda}$$

(s_n = number of the cases of an empirical series, s_i = number of the occurred positive cases, k = number of the favourable cases, w = number of the possible cases, λ = a parameter for which all real numbers from O to $+\infty$ are possible.) For each value of λ we get a certain form of the G-function which characterizes a certain system of probability. Thereby it is evident that the tensor of curvation and the G-function have the same logical and epistemological character and accomplish the same role. The one expression characterizes the order of the geometrical systems, the other the order of the probability systems. Thus it is clear that the application of the geometrical and the inductive logical systems to describe the empirical reality is performed by methods of the same epistemological character. Thereby I think a far reaching analogy between the geometrical systems and the systems of probability becomes obvious.

G.-G. Granger

Je voudrais d'abord dire que je suis d'accord, d'une manière tout à fait complète, sur le fond de la question, avec ce qu'a dit le Professeur Ayer, en ce qui concerne l'impossibilité d'une déduction d'un principe d'uniformité de la nature à partir du calcul des probabilités.

Le but des deux observations que je vais faire est le suivant: il s'agit simplement d'atténuer pour ainsi dire le caractère aporétique de la conclusion du Professeur Ayer et ce qu'elle a de très négatif. Je pense qu'il est possible de fonder un calcul des probabilités, et de comprendre dans quelle mesure ce calcul des probabilités peut avoir quelque succès en tant que formalisme applicable à la nature, sans faire des hypothèses trop fortes.

La première observation que je ferai, est de nature un peu technique si l'on veut, en tous cas plus particulière que la seconde. Il me semble que la conception carnapéenne de la probabilité fait intervenir seulement une hypothèse qui est la suivante: l'ensemble des propositions que l'on peut énoncer au sujet du monde est un ensemble mesurable, ou plus exactement, sur lequel on peut définir une fonction additive qui possédera les propriétés très simples que les mathématiciens attribuent à la mesure et qui peuvent se résumer en deux ou trois axiomes.

Je pense que cette seule hypothèse permet ensuite de définir la probabilité conditionnelle, car il s'agit toujours de cela, à savoir comment une certaine hypothèse confirme, — et dans quelle mesure elle confirme — une certaine conclusion. On définira cette crédibilité de la conclusion au moyen du quotient des 2 mesures comme l'a rappelé le Professeur Ayer.

Mais cette hypothèse de mesurabilité de l'ensemble des propositions que l'on peut énoncer au sujet de la nature me paraît une hypothèse relativement faible, en tous cas pas plus forte comme l'a, je crois, bien rappelé le Professeur Juhos dans son exposé, qui n'est pas sensiblement plus forte que les hypothèses que l'on fait pour construire une géométrie, car il s'agit là aussi de supposer qu'à un certain système abstrait on peut attacher une fonction numérique qui aura les propriétés d'une mesure. Et cette axiome n'enveloppe nullement l'hypothèse spécifique quant à la nature de cette fonction additive d'ensembles; elle peut être extrêmement large et ne dit rien sur ce que sera la probabilité de tel ou tel système d'événements dans la nature. Elle suppose seulement qu'il est possible de définir une fonction qui est une mesure.

Je dois dire toutefois que le Professeur Ayer a peut-être raison en un autre sens, de dénoncer le caractère un peu fort de cette hypothèse en ceci que dans le système de Carnap, il faut, pour que l'on puisse définir l'ensemble des propositions qui concernent l'Univers, supposer une exhaustivité possible de cet univers; autrement dit, il faut que l'on puisse énumérer l'ensemble des prédicats et peut-être l'ensemble des individus, et que l'on puisse dominer d'une certaine manière la multiplicité des propositions qui peuvent être énoncées.

Mais cette hypothèse, si elle est forte, me semble être absolument indispensable à toute constitution d'une pensée scientifique dans quelque domaine que ce soit.

La deuxième observation est de nature plus générale et elle est tout à fait liée à la première; il s'agit de ceci: il me semble que le Professeur Ayer en exposant la pensée de Carnap a laissé sous-entendre une distinction qui me paraît essentielle; à savoir la distinction entre probabilité considérée comme *propriété objective des événements du monde* ou des ensembles, sous-ensembles d'événements du monde, qui conduit à une théorie fréquentielle de la probabilité; et, l'autre conception, que l'on appelle parfois subjectiviste ou que certains auteurs récents appellent per-probabilité «personnelle», qui consiste à énoncer des relations entre propositions et à assortir ces relations d'un certain coefficient numérique qu'on appellera probabilité, qu'on pourrait appeler aussi crédibilité ou, comme on le fait encore souvent, coefficient de confirmation.

Je crois que la distinction entre ces deux conceptions de la probabilité, distinction qui remonte très loin mais qui est exprimée pour la première fois d'une façon précise chez les Britanniques, en particulier chez Ramsey, et aussi chez Wittgenstein; cette conception permet peut-être de lever les paradoxes, de lever les difficultés qu'a soulignées le Professeur Ayer. Par exemple, lorsque dans son raisonnement il fait apparaître le caractère paradoxal d'une conclusion faite à partir des hypothèses de la conception carnapéenne: à savoir que toute proposition quelle qu'elle soit a nécessairement un représentant effectif dans le monde; eh bien, ceci signifie en fait que le discours sur le monde est un discours supposé exhaustif. Lorsque l'on constitue une théorie de la probabilité, on suppose nécessairement que l'on envisagera seulement les propositions qui énoncent des descriptions de structure non contradictoire; le «paradoxe» de Ayer ne veut pas dire autre chose, il est inclus déjà dans l'hypothèse préliminaire à la construction, mais il ne dit rien sur la réalisation spécifique de telle ou telle situation dans le monde.

L'autre paradoxe, si l'on peut parler d'un paradoxe, est le fait que chaque événement singulier a une probabilité zéro. Cela ne veut pas dire du tout qu'il est irréalisable, ce serait confondre probabilité zéro avec irréalisabilité ou contradiction, et probabilité un avec réalisation effective. Je crois que si l'on se réfère à la conception originaire qui est celle d'une mesure des ensembles de propositions faites à propos du monde, on peut très bien concevoir qu'un ensemble de mesures nul ne soit pas vide. De même qu'un ensemble de mesures unité n'est pas nécessairement associé à la réalisation effective de quelque événement.

A. J. Ayer

Le premier point c'est si l'on pourrait étendre la méthode de Carnap à un langage de tous les jours ou à un langage scientifique. Ce me semblerait difficile. Il y a la difficulté que M. Granger a mentionnée, et il y en a d'autres et particulièrement que dans le système de Carnap, il s'agit d'un langage très pauvre à cause du principe que tous les prédicats de base, les prédicats primitifs soient logiquement indépendants. Il y a évidemment une très longue étape à faire entre un langage très simplifié de cette espèce et le langage quantitatif, parce que évidemment dans n'importe quel langage quantitatif ce principe est violé.

Je ne dis pas que Carnap n'aboutira pas à cela. Je signale seulement qu'il y a une très longue étape à faire. Mais même alors, même s'il pouvait faire cela, même si on pouvait construire des «state-descriptions» pour notre langage ordinaire, je soutiendrais qu'on aura quand même besoin, pour appliquer ce système, des assomptions empiriques, comme une assomption de l'uniformité de la nature, évidemment dans une forme assez spéciale.

Le deuxième point: évidemment Granger a raison de dire que je n'ai pas tenu compte de la probabilité-deux de Carnap, c'est-à-dire la probabilité qui s'applique aux faits statistiques. Je l'ai fait pour cette raison que dans le système de Carnap, pour justifier quoi que ce soit, même pour extrapoler les données statistiques, il faut se fier à sa probabilité-un.

Dans le système de Carnap, toute question de justification, toute question d'extrapolation, se base sur la probabilité logique, tandis que la probabilité statistique, se réduit, n'est-ce pas, à un simple assemblage des faits. On constate qu'il y a des précautions à prendre, mais pour raisonner là-dessus, dans le système de Carnap, il faut avoir recours aux probabilités logiques, comme l'a dit von Juhos.

Je viens au troisième point que Granger a soulevé. Ici je crois qu'il a peut-être tort; ce n'est pas une question que le discours soit exhaustif; évidemment dans n'importe quel système toutes les combinaisons non contradictoires sont possibles, ça c'est une proposition analytique. C'est une question ici que, non pas les événements singuliers, mais les lois générales ont une probabilité zéro. Donc il s'ensuit que les négations de ces lois, c'est-à-dire les propositions singulières qui contredisent les lois ont une probabilité d'unité. Or, si on va dire que la probabilité zéro d'une loi ne veut pas dire qu'elle ne tient pas, alors le système n'a absolument aucun rapport avec les événements, les faits, et je ne vois pas à quoi ça sert. Par contre, si on peut en déduire qu'aucune loi ne tient, il sera certain qu'à chaque loi il y a quelque part une exception, un contre-exemple, et alors vous avez votre argument ontologique.

Alors vous devez choisir. Vous pouvez protéger Carnap en disant que ça n'a absolument rien à voir avec le monde, avec ce qu'on doit espérer, et alors évidemment il n'y a pas paradoxe; ou vous vous mettez à appliquer le système et alors vous tombez sur le paradoxe qu'on peut démontrer dans le système l'argument ontologique, ce qui me paraît un peu trop fort.

A. MERCIER

What I am going to say are not objections, but I'd like to beg Professor Ayer to take some precautions. Usually the logicians ask the scientists to take precautions, but, if I may once put things upside down, I would first say that a statement like «all swans are white», even if it were true, is not a law of nature. So, as a first precaution, we must look at physics to learn how laws of nature are expressed. It would be a law to say, for instance, that if such and such procedure is applied to a white swan, its descendants may be black with such and such probability.

A second precaution refers to what is written in the manuscript, where the author refers to the so-called four-dimensional continuum and says: «for these reasons, we need to make two rather large empirical assumptions». However, the space-time continuum in which the physical and the mathematical properties would coincide, does not exist as such. The reason is that time flows according to the sense of the arrow of time. There is no go back into the past. One might say perhaps, that there is a great majority of signals received later than issued at their sources, or that nature prefers retarded to advanced potentials, to use the technical language of physics. For these reasons, I beg Professor Ayer to add to his assumptions some reference to the impossibility to look back into the past. We can only rely upon the future. For the specialist of the philosophy of science, I might say that the consequence of this is that we cannot deduce thermodynamics from the mechanics of reversible processes, whereas, conversely, we can make of mechanics a special case of thermodynamics. If one refers and he gives in meaning two individuals have exactly the same exhaustive properties, it is not possible to name them by two different letters *a* and *b*. For there is no physical way to decide which is *a* and which is *b*. For instance, the six faces of a die cannot have the same probability to fall up, not even by cinematography as was wrongly believed by Borel, because of Heisenberg's uncertainty relation. Therefore, there are, paradoxically enough, probably no strictly possible experiments on pure cases in probability. For there may not be two single identical objects in the world. Therefore, the only reasonable applications of the calculus of probability to physical nature must be either to macroscopic or semi-macroscopic systems or, must be made on the basis of either quantum field theory, or some super-quantum theory which we do not yet possess, and in which the objects of nature have not a basically discontinuous, but the continuous nature of fields, such that its secondary discontinuity follows from quantization. We know today of no other possibility to describe physical reality correctly. In that picture, particles are not the primary constituants of reality, they are names for secondary things which we think there are. But we can never, in any experiment whatsoever, see a single particle as a primary thing existing in its splendid isolation. Particles are constructs of our mind. Now particles are the prototype of individual objects. Therefore Carnap makes a good case in assigning equal probabilities not to hypothetical states of an assumed reality, but to structural descriptions, i.e. constructs which may be working models of an ever-escaping reality. In this sense I think that Ayer's conclusion remains.

A. J. AYER

I thank Professor Mercier very much. I think, on the whole, he's got a lot here. I shall take up two or three of his points.

On the question of examples I quite agree, I mean, examples like «all swans are white» or «ravens are black» that keep cropping up in the literature are very, very unlike scientific laws. I think they are put in because they are supposed to have features which are common to all the cases one is interested in, because they are obviously easier to handle, and also because they are understood by the people who read philosophy books, whereas science mostly isn't. I would claim further, accepting Mercier's statement that law is now generally taken in its statistical form, that what I have to say about the application of the *a priori* procedure applies to statistical laws too. And in fact, if you look, you'll see that in William's formulation of this argument, it is applied exclusively to statistical laws. And the same difficulties arise with regard to the extrapolation of the recorded frequencies as arise in the more simple cases. I don't think this makes any difficulty of principle.

On the point about the spatio-temporal continuum, I think again that he rather strengthens my position. I was criticizing the Briareus model, the assumption that is made by writers on probability, that nature, as it were, is divided into separate bags, where you can dip your hand in any one, and I said, in fact, we can dip our hands into very little. And I think Mercier brought out the point that we certainly can't dip our hands into the past bags, and only, of course, into a very limited number of the future ones. In speaking of the spatio-temporal continuum, I am afraid that I was showing off in a way that isn't necessary here. I could as well have referred to what occurs in different parts of space at different periods of time.

Now, the third point that Mercier brings up is a very interesting one. A kind of fundamental one which I didn't put in. That Carnap's system is strange in this way, that not only can there be distinct individuals, which have exactly the same predicates, but there can in the system be individuals with no predicates at all. Because the negation of every primitive predicate in the language can be ascribed to an individual, and this makes one state description. One state description of the whole universe could be one in which no individual had any predicate. Now, this clearly is to make the system even more difficult to apply, than Professor Granger and I had ever seen it was. I think this is a very important point for Professor Mercier to have brought out. And I think he is absolutely right in saying that here logicians are much less strict than the physicists. I mean they do play with their letters, their marks on the board, without any consideration of what physical meanings to give to them. I think this is entirely true of Carnap's system. And I don't think he avoids this objection by concentrating on structural descriptions. For while there are certainly structural descriptions, you can't get them independently from knowing what individuals have got what properties.

As to the final point about particles being constructs of our minds, I don't know. It has a nice, old-fashioned positivist ring, and I like it for that, but I feel a little more doubtful whether it is true.

A. C. Ewing

I should like to ask Professor Ayer if he is still assuming a Humean regularity theory of causation. If we take this kind of view it is obvious that we cannot justify induction by playing about with the notion of probability. But suppose different things are connected in such a way that from their inherent nature they tend to bring about changes in each other, this would make all the difference. If when throwing a die we ruled out irrevocably the hypothesis that the die was weighted, then even if we threw the die 100 times and it fell with the six uppermost each time, this would not make it any more probable that it would fall with the six uppermost the 101st. time. But if we did not rule it out, we should have an argument which made the hypothesis extremely probable.

A. J. AYER

I am still assuming the Humean theory because I believe it to be true. But I don't think I make here an essential use of it in this paper, and I think all my results would still hold if I were taking Ewing's view of causation. Let's take his example of the die. Certainly, if one is simply operating within the calculus of probabilities, and therefore starting with the presupposition that this is true die, the fact that it turns up six a hundred times in succession would have no influence at all on the probability of the hundred and first throw. The odds would still be one in six. But, of course, in these circumstances, I, like Ewing, would begin to suspect that this was not a case where the presuppositions of the *a priori* calculus were satisfied and that the die was weighted. Now, when I say the die is weighted, I wouldn't commit myself to more than a Humean statement, that when the centre of gravity is displaced in certain ways, then, as a matter of fact, it invariably happens that the six, or some such number, tends to come up more often than the other ones. Ewing puts in something stronger. He says «The centre of gravity's being displaced necessitates this result». Let us use Broad's word 'convey' for this kind of necessitation. Now, it doesn't seem to me that this emendation of Ewing's, though it may be advisable for some other philosophical reason, helps us at all here, because we still want to know what conveys what. And if there is any doubt whether in the next case, or set of cases, the centre of gravity's being displaced will have this effect as a matter of fact, there is that doubt, and more, as to whether it will, in Ewing's stronger sense, convey this effect, I mean, if I can't prove my weaker proposition. Ewing can't prove his stronger one.

N. RESCHER

I have a very small observation to make about Ayer's formulation of the uniformity of nature principle. It seems to me that this formulation of the principle, which is characterized by Ayer as being strong, and which in the context of his discussion looks like a philosophically powerful principle, so powerful in fact that he suggests a little later on the next page that it is quite likely false, is really from another point of view a quite weak and a feeble principle. Moreover, if we are interested in the application of reasonings of the inductive type — in making the step from a sample to a population in some context like that of demography, demographic sampling or election forecasting — then even if, in fact, we had this principle available to us, it would not do us very much good. And the reason for this lies in the details of formulation of the principle. When we speak of the *distribution* of a given *property,* this means, of course, for any given property we might like to consider; but as the cooked-up properties of Goodman have shown, this must be limited. So in formulating the principle we must say for any *regular* property, or for any *nice* property. Moreover, when we go on with Ayer to speak of any such «property within a given population is approximately even in space and time in the sense that the ratio of its incidence in a sufficiently large segment of space-time» then «sufficiently large» is another one of these sort of *weasel* words that is essential, I think to formulate this kind of uniformity principle in a viable way. Well, if we are actually interested in a concrete case of making this step from property or distribution in a sample to that in the population of a whole, what, of course, we have to know is, first of all that this is a *nice* regular property, and secondly, that our information, in the sample, relates to a *sufficiently large* segment of space-time. And this in practice is, of course, something of which we could not gain adequate assurance. So that, this formulation of the uniformity of nature principle is strong in the sense that, if we are interested in some sort of very broad, abstract, theoretical justification of a generic principle of induction, it might do us some good. But if we are interested in questions of application or practice, or implementation of specifically inductive procedures, then even this purportedly very powerful principle would not suffice to help us very far.

A. J. AYER

I entirely agree. Not only is the expression «a sufficiently large segment of space-time» very vague, but as I remark on the next page, what we are mainly concerned with is what can be expected tomorrow, and this means that we have to postulate fair sampling over the very small area of space-time which practically interests us. So I'm inclined to accept the implications of what Rescher was saying, and advance the view that it isn't really possible to formulate any principle at that level of generality, which is going to give us what we want. All that I was saying in my paper was that we needed at least the principle which I formulated, and I wrote badly if I implied that I thought that even with that we should be home.

R. McKEON

I find what Mr. Ayer has said about induction and probability clear, and I am convinced that the position he takes is right. Being clear and right, however, is not sufficient in a philosophic discussion. Mr. Ayer ends on a perplexity within the limited region of the calculus of probabilities. I suggest that directions of inquiry into means of clarifying, or straightening out, that perplexity might be found by placing the limited region and rigid definitions he has imposed on the problems of probability in the larger context of ambiguity introduced by alternative senses and references to those he has imposed and used.

What is Mr. Ayer's problem ? Stated in the terms of this conference, which is the context of his statement of the problem, he is seeking a "justification" of probability calculations. He begins by telling us that we can no longer think of factual reasoning as a kind of valid syllogism with the principle of the uniformity of nature as a major premiss. "Demonstration" is excluded, and therefore we seek foundations of factual reasoning in probability. "Verification" or a kind of probable appeal to nature or to the facts is a possibility. Mr. Ayer distinguishes two approaches to probability: one based on suitable assumptions about the constitution of nature, the other based on an *a priori* calculus of chances or a logic of probability. He chooses the way of the calculus of probabilities, based on the Law of Large Numbers, but he finds that the formulations of Harrod and Williams introduce initial probabilities and their attempt to "justify induction on the basis of the calculus of chances fails unless it is supported by a principle of fair sampling" (pp. 100-101), and that Carnap, who does not introduce initial probabilities, produces a formal theory of probability which lacks the material concepts required for confirmation. Verification fails for lack of "justification" (p. 104).

The theme of the discussion of principles of probable or contingent or material reasoning — or of objective or concrete or factual reasoning — has a long history antecedent to its mathematical formulation. Since the beginning of the modern statement of probability in the seventeenth century, our three terms are separated nicely along three strands of the theme: the "principle of indifference" is associated with the "demonstration" of probabilities; the "principle of large numbers" with "verification"; and Mr. Ayer has associated the "principle of fair sampling" with "justification." J. M. Keynes used the principle of Indifference or the principle of Non-sufficient Reason to separate his logic of probability from the frequency theory of probability, which reduced probability to statistical statements of frequency based on the Law of Great Numbers or the Law of Succession. The achievement of the eighteenth century had been to reconcile the philosophical positive idea of probability, based on equivalent quantities of experience, and the mathematical negative idea of probability, based on equal degrees of ignorance, by means of the principle of non-sufficient reason. On the basis of this reconciliation the nineteenth century developed an *a priori* logic of probability, based on intuition or initial probability, and an empirical calculus of probabilities, based on experience or on the frequency of occurrence of events with certain attributes. Mr. Juhos has correctly observed that Mr. Ayer is involved in the opposition of an empirical theory of probability which applies to the real world or to the phenomenal world and a logical theory of probability which makes *a priori* assumptions. Mr. Ayer tries to avoid the *a priori* with Carnap, but finds Carnap trapped in the *a priori* of a principle of fair sampling.

I have a prejudice against dichotomized worlds even when they take the form of a dichotomy of probable worlds into a world of empirical probability and a world of formal probability. In the development of probability theory neither the method of demonstration nor the method of verification led to dichotomies in their development of logics of probability and frequency theories of empirical probabilities; the dichotomies arose in the controversies between the two approaches. Mr. Ayer is right therefore in seeking a principle of justification, but I think that he makes his task needlessly difficult by trying to make the principle of *justification* conform to the requirements of *verification* of either the empirical or frequency theory of probability. The orientation of W. E. Johnson to the problems of probability might be susceptible of fruitful transformation in seeking a principle of justification rather than a principle of verification. Johnson claims derivation from Mill, but he does not appeal to the principle of the uniformity of nature. He does argue that induction is a syllogism with the peculiar characteristic that its conclusion is broader in extension than its premisses. He avoids the mathematical and the psychological interpretation of probability, and attaches the quantity or degree of probability exclusively to the proposition, regarded as based upon rationally certifiable knowledge acquired by any supposed thinker. Mr. Ayer may suspect that Johnson's distinction between the "epistemic" and the "constitutive" interpretations of a proposition conceals remnants of the "subjective" and the "objective." But they may also be viewed as purely logical devices to distinguish the two necessary ingredients of any probability judgment: a formal structure stated propositionally, and a structure of occurrences with a basis in observation or the past and an application in expectation or the future.

The preliminary exploration of the region of ambiguity suggests that Mr. Ayer sets himself an impossible goal if he hopes to find a principle totally free from assumptions about the uniformity of nature. All principles of probability calculation assume the uniformity of nature: as a principle of demonstration, it is the law of causation or the principle of indifference; as a principle of verification it is the law of great numbers or the law of succession; as a principle of justification it is the principle of Fair Sampling. In all three it is an assumption concerning the uniformity of nature which makes it susceptible of rational analysis. Kant called it "purposiveness."

A. J AYER

... I'd like to make a few comments on that. First on Keynes. Keynes, of course, is a pure logical theorist. His system is very like Carnap's. And Keynes, and also Nicod make use of an application axiom which is exactly in line with those of Williams and Harrod. As to frequency theory, the difficulty is, of course, that it doesn't apply at all to the individual case. Frequency theory is all about classes, and there also are difficulties over the extrapolation of ascertained frequencies. I think the notion of going to a limit is not very satisfactory logically, and also very difficult to apply empirically. What one has to do, if one is operating with frequency theory as in statistics, is simply to make more or less arbitrary provisions. When the actual frequencies deviate too far from the predicted ones, one revises the hypothesis. In fact, statisticians do lay down quite arbitrary limits. They tolerate fluctuations within a certain range, but beyond that range they say that the hypothesis is falsified. And, of course, you can perfectly well operate in this way. This may even be the answer to the whole problem which we are discussing here.

On the last point, I've forgotten my Johnson. It's so many years since I've read him, and I don't know what his version of the probable syllogism is. All I know is that what is called the probable syllogism in the literature is a total fraud and leads to contradiction. Hempel's example is quite a good one here. Ninety percent of Swedes are protestant. Petersen is a Swede, therefore there is a ninety percent probability that Petersen is a protestant. But equally ninety percent of those who go to Lourdes are Catholics. Petersen went to Lourdes last year, therefore there is a ninety per cent probability that Petersen is a catholic. So that this leads to a contradiction if you take it as applying to individual cases. But, of course, the conclusion of the probable syllogism is a total fraud, because the ninety per cent probability that Petersen is a protestant isn't about Petersen at all, it's just a way of stating that Petersen belongs to a class, ninety per cent of which are protestant. I can't say without the text, whether Johnson's argument is exposed to this criticism, but I have no doubt that it holds against any syllogism of this type.

J. Horovitz

Professor Ayer disappointedly finds that an a priori conclusion obtained in the framework of the logical theory of probability «tells us no more than what we know already». When he then goes on wondering «what does the talk of probability come to», it looks as if he seriously expected the calculus of chances to yield synthetic information. No, indeed, the logical theory of probability does not yield new knowledge about the world. But it serves as a framework for adapting aquired knowledge to the guidance of action. Let us reconsider the example of the woman who is expecting a child, and knows that the proportion of male births in the population to which this child belongs is over 50 per cent. Suppose now that she wins a prize at the fair and has to choose between a male and a female baby's outfit. If no further relevant information is available, it must be granted, I think, that it would be somewhat more reasonable for her to choose the male baby's outfit.

The reservation «if no further relevant information is available» is of course essential. Professor Ayer, however, seems to ignore this reservation when he opposes the assumption «that any one sample is antecedently as likely to be selected as any other». 'Antecedently' here must be taken to mean 'prior to the consideration of any further relevant information'. As for the assumption «that the distribution of a given property within a given population is approximately even in space and time», it is in fact overruled by what we know empirically about the structure and dynamism of the universe. The assumption of permanence, indeed, is (antecedently!) no better warranted with respect to the colour of ravens than it is with respect to, e.g., the climate of a region or the general characteristics of a nation.

Professor Ayer wonders «what can possibly be meant by saying that a given statement is confirmed by any tautology, that is by zero evidence, to such and such a degree». Let me try to illustrate this by a simple example. John challenges Paul: «A is an individual, and B is a property appropriate to A, positive or negative, simple or compound, Would you bet either that A has the property B or that A does not have it ? Paul: «What, more precisely, is A and what is B ?» John: «I know nothing more about it, but the answer is in George's pocket». Paul: «I'll not bet, for I'd be as likely to loose as to win». John:

«I offer you two to one. Will you bet now ?» Well, isn't it antecedently more reasonable for Paul to bet now, either way, than not to ? As I think it is, I find Carnap's idea of the initial degree of confirmation very attractive.

A. J. AYER

Why is it more reasonable ? Take the case of the unborn child. If I know that so far this year the percentage of boys has been slightly more than fifty per cent, I might equally well assume that my child is going to help to change the statistics. And even if I do conclude that it is slightly more likely to be a boy, this has nothing to do with the *a priori* calculus; neither is it simply on the basis of this particular statement of frequencies. It is also because I believe, rightly or wrongly, that the sex-ratio of births of children does not vary very sharply, that there haven't been climatic conditions that would affect the ratio, that there hasn't been any instance of disease that could have altered it, and so forth. It might, for example, be the case that in my own family there had for a long time been a preponderence of girls, and then I should bet on its being a girl, even though the proportion of boys in the whole population were seventy per cent, and not just over fifty. All these things come in, and the whole point I was making here was that if you just keep to the statistics alone, without bringing in any further consideration at all, then, in the individual case, there is no particular reason to expect anything rather than anything else. Of course if you know what the total statistics are, for the period in question, and if you also know what all the recorded statistics are, then you may be able to say: well, it must be a boy, otherwise the sum won't come out right. But this only works if you know what the answer is, and since your own case is part of what makes the answer you can't know this. There is far too much nonsense talked about the so-called law of averages.

On the last point, of course, your man would be a lunatic if he took a bet of two to one, given the actual state of the world and given the enormous numbers of properties that there are, and the comparatively few number of individuals that he is likely to be concerned with. It would be absurd to take odds of only two to one against Tommy's having the property of being radio-active, or whatever else you like to put in. But this whole way of putting it is absurd. Part of what I was trying to say was that this sort of question «would you bet in these sorts of conditions in the absence of empirical knowledge of any kind» is senseless. This is not to say that you cannot give it a sense.

R. KLIBANSKY

You were arguing on Carnap, whose position seems to me somehow to be inspired by Bolzano. Mr. Ayer stated that he was not too sure. Will we go from here ? If I'm not quite wrong, here is at least an incline where we should go. And then, I'm sure, he would not like to leave us completely in the dark with a purely negative conclusion. It would be unfair to ask him to produce his justification of induction. But perhaps he may care to comment on four or five of the main positions held nowadays by those who do not agree with Carnap.

First, attempts have been made to provide a kind of practical justification, for adopting inductive policies without any implied commitment of their truth, or even their probability. This may be regarded as a way of accepting Hume's sceptical conclusions. while still claiming to have reasons of a practical nature for using inference from the known to the unknown. It is held that, in practical situations, we have nothing to lose, and everything to gain, by acting as if inductive policies were truth-producing. Now this has been called a "vindication of inductive policy" in the form of a tautological demonstration. That adherence to such a policy is, in some sense, reasonably justified. In other words, we are thrown back to what *James* would call "false options", and I wonder whether Mr. Ayer would agree that this, in the end, boils down to the truism that induction works if it works. And that this is not a real solution.

Then, we have, secondly, the position that induction has to be considered as a kind of elimination, and emphasis has been laid on eliminative procedures I think, with great elegance, by *von Wright*.

Then, we have the emphasis on the subjective estimate of probability in *de Finetti*'s work, and others.

Then there are those who reject induction altogether, in favour of deduction.

And, in the end, there are those who deny that easier problem of justifying induction, and consider it as due to a confusion of thought.

If he would briefly comment on these five positions,

AYER. — What was the third ?

KLIBANSKI. — The third was a subjective estimate. A subjectivist theory by *de Finetti,* and others.

Now, if he would care to comment on these, he would, I'm sure, show us in which direction the answer should lie.

A. J. AYER

All right, then. Very briefly.

The first one is the *Peirce-Reichenbach* view, the defence of induction as the best policy. The position taken is that either no policy works at all, or inductive policy works. This is a strong position, because in a certain sense the very word «policy» comprises induction. For instance, if you try to adopt a counter-inductive policy, crystal-gazing or whatever, then if it works, induction takes it over. There's only a question of a time-gap. But I think that there are two difficulties in this position: one is that Peirce's claim that induction is bound to succeed in the long run is valid only if the long run can take an infinite time. And one hasn't got an infinite time. You can prove that, if there is an answer, you're bound to get it sooner or later. But if sooner or later can extend to an infinite time, this means that however long you go on, there may still be an infinite time before you get the answer. At the least this is practically disadvantageous.

The second difficulty, which is still more serious, is that this approach has been rather ruined by Goodman. We talk about inductive policy as though there were such a thing, clearly demarcated. But, I think, Goodman has shown in his remarkable little book *Fact, Fiction and Forecast,* that if you don't put any restrictions on your predicates, then for any hypothesis which is supported by evidence *e,* you can always find a second hypothesis *h′* which is incompatible with *h* and equally supported by *e.* Now, this being so, you see, it means that it isn't a question of whether to practise induction because, in a certain sense, if you have any policy at all, it's bound in a broad way to be inductive. But what kinds of induction to practise ? What are we going to extrapolate ? And of course to this, which is the crucial problem, what Goodman called «the new riddle of induction», the Reichenbach-Peirce approach gives you no answer.

The second method is that of elimination. Here again there are two difficulties: first that of arriving at a restricted set of hypotheses, one of which must be the right one, and secondly that of making sure that your elimination is complete. To get anything like Mill's methods going, you need very strong assumptions. You need the strong assumption that you could have a finite number of candidates and the second very strong assumption that you can decide when the candidates have failed. Beyond that you still need a uniformity assumption. You have to assume when a candidate has failed, he can't represent himself. It might be that hypotheses were like childish diseases, on the analogy that when you had had the measles once, you couldn't ever get it again. And so the one thing we should want would be to see all our hypotheses falsified. They would have their measles, and then we could play around with them safely. In other words, the idea that if a hypothesis has not yet been falsified you have the right to go on trusting it is also an empirical assumption. This also deals with your fourth case, since it shows that Popper's pretence of dispensing with induction is a fraud. Popper claims not to use induction because he asks no more of a hypothesis than that it should not be falsified, but then he allows it to gain credit from not being falsified. A proposition which has been put to a severe test in Popper's system is supposed to acquire greater verisimilitude from passing it. But this comes back to treating the test-instances as constituting a fair sample.

On the last thing, yes, and no. It seems to me that the old English manner, my own earlier manner, of saying that it is wrong to ask for a justification of induction slides over important problems. I think that what we want to do first of all is exactly the kind of work that Bunge is doing. We want to get clear exactly what scientific procedure is. What counts as a good argument in science ? What kinds of propositions are advanced in the various sciences ? And on what sort of evidence ? Then we can try to formalize this procedure, if we are interested in formalization. And then we can look at these principles. I do not think that we shall ever be able to give a non-circular justification of induction. In the end we are going to have to make our principles support each other, as in a house of cards that one builds for a child. But equally, there is no non-circular justification of deduction.

Deduction is equally circular. The rules we accept are the rules which give the results we want, and conversely we measure our results by the rules. There is this circularity in deduction and I don't mind there being circularity in induction either, provided there is a respectable circle. What we haven't yet got is a respectable circle. I mean a nice round one.

F. GONSETH

J'aimerais tout d'abord donner mon entière adhésion aux réserves que M. Ayer fait envers M. Carnap.

Ce n'est cependant pas là l'objet principal de mon intervention. J'aimerais revenir sur ce que M. Juhos a dit de l'espace et que M. Ayer ne me semble pas avoir interprété tout à fait justement. M. Juhos avait tout d'abord indiqué qu'il y a deux aspects sous lesquels la probabilité peut être envisagée. Il a pensé, me semble-t-il, qu'il se ferait plus facilement comprendre en se référant au cas de l'espace. Je pense que quelques observations historiques sont susceptibles de jeter une certaine lumière sur le sujet. On sait que Gauss fut le premier à découvrir l'existence de la géométrie non euclidienne dite hyperbolique. Il le fit avec une entière rigueur, ses cahiers de notes ne laissent aucun doute à cet égard. Il tint cependant cette découverte secrète. Il le fit, selon sa propre expression, par crainte du *«Geschrei der Beotier»*. Cette découverte amena-t-elle Gauss à opérer une distinction entre un espace physique (l'espace de tout le monde des physiciens) et un espace abstrait (celui des géomètres) ? Pas du tout. Selon lui, il ne pouvait exister qu'une seule géométrie vraie, mais quant savoir laquelle — de l'euclidienne ou de l'hyperbolique — était la vraie, la créature humaine n'avait pas le pouvoir d'en décider rationnellement. Peut-être les anges... Gauss se taisant, le mérite d'avoir édifié la géométrie hyperbolique revint à Bolyai et à Lobatschevski. Font-ils la distinction entre un espace physique et un espace géométrique ? En aucune façon. Bolyai estime avoir découvert la géométrie absolument vraie (...*geometriam absolute veram exhibens*...). Quant à Lobatschevski, il estime que la vérité de la géométrie reste incomplète si l'on ne peut pas développer en même temps un système de mesures adéquates. Pour tous deux aussi, il ne pouvait y avoir qu'une seule forme de géométrie qui soit à la fois vraie pour le mathématicien et juste pour la physicien. La découverte des géométries non euclidiennes devait cependant provoquer une crise grave, crise de l'évidence et crise de la géométrie rationnelle. Elle ne fut conjurée que par le clivage de celle-ci en ses deux aspects complémentaires. Je ne saurais dire à quel moment le principe de cette dualité d'aspect s'est imposé. Le fait est qu'il joue un rôle de premier plan dans les réflexions épistémologiques de Poincaré. Quant à Einstein, il allait jusqu'à déclarer que, sous son aspect de théorie idoine de l'espace (physique), la géométrie ne regarde plus le mathématicien. Depuis lors, c'est dans mille et mille travaux que la distinction a été mise en lumière et a trouvé sa place.

Je pense que Juhos a simplement voulu montrer qu'il est possible de parler de l'espace géométrique sans l'identifier de prime abord à l'espace physique et qu'il convient souvent de mettre en face l'une de l'autre ces deux conceptions de l'espace.

En fait Juhos a illustré par un exemple à mon avis bien choisi le principe de dualité qui oppose et allie à la fois la théorie et l'expérience. En disant que comme en géométrie il y a deux façons complémentaires d'envisager la théorie des probabilités, Juhos n'a fait qu'étendre à ce dernier cas la validité du principe de dualité.

(Je m'étonne d'ailleurs que Juhos ait qualifié d'analytique ce que d'habitude on nomme théorique ou simplement abstrait. Il surajoute ainsi à l'idée du théorique une certaine explicitation à laquelle je ne puis pas adhérer. Il engage la méthodologie des sciences et la théorie de la connaissance en général dans une hypothèse (leur caractère analytique) que, somme toute, je tiens pour arbitraire.

Dans ces circonstances, il y a deux choses à faire et à ne pas confondre: la première est de bien voir ce que les mathématiciens ont fait de la théorie des probabilités et la seconde est de comprendre comment cette théorie s'applique, c'est-à-dire comment elle devient un outil du côté de la connaissance pratique. Ce que les mathématiciens en ont fait, M. Granger l'a dit: une théorie générale de la mesure. De ce côté et spécialement en considération des travaux de Choquet, on a bien l'impression que, provisoirement tout au moins, un certain plafond a été atteint. Tout autre est le problème de savoir si et comment la mesure ainsi précisée (et qu'on appellera probabilité) s'applique au sens voulu et attendu par les utilisateurs. Il faut tout d'abord souligner que sous cet angle, le problème est d'une considérable, quelquefois même d'une inextricable complexité. Qu'il me soit permis de me servir d'un exemple. Supposons qu'une firme productrice de produits pharmaceutiques ait à prendre la décision de mettre ou de ne pas mettre un certain remède en vente. Cette décision va, d'une double façon, dépendre de l'expérience. On aura fait la statistique des applications du remède en question, la statistique de ses succès et de ses insuccès. L'élaboration mathématique de cette statistique aura, supposons-le, conduit à fixer la valeur λ' d'un certain paramètre λ. λ' dépend donc de l'expérience.

Mais, dans un cadre plus général, l'expérience aura conduit à fixer une autre valeur λ^* de λ telle que ξ si λ' n'est pas inférieur à λ^*, les risques inhérents à la mise en vente du produit ne doivent pas être encourus. Ainsi, quel qu'ait été le travail du mathématicien, si poussée qu'ait été l'analyse mathématique des statistiques, il y a toujours un instant où, une décision devant être prise, c'est en se fondant sur l'expérience qu'elle l'est ou qu'elle ne l'est pas. La part faite à l'expérience peut être plus ou moins grande, elle ne peut être réduite à zéro.

Cet exemple illustre le fait que le principe de dualité est aussi valable dans la théorie des probabilités. Sous cet angle, Juhos n'avait donc pas tort d'établir un parallèle entre cette théorie et la géométrie et surtout avec une géométrie non euclidienne.

Maintenant, on peut se demander si les deux cas dont il vient d'être question, celui de la géométrie et celui de la théorie des probabilités, ne sont pas deux cas particuliers de l'application d'un principe général gouvernant les rapports de la théorie et de l'expérience. En d'autres termes, on peut se demander s'il n'existe pas une méthodologie de la connaissance scientifique dans le cadre de laquelle ce principe serait simplement l'un des principes fondamentaux. La question serait alors surtout de savoir comment un tel principe pourrait être justifié.

Or, et c'est là que j'en voulais venir, je pense qu'aucun principe de ce genre ne saurait être justifié par lui-même et pour lui-même. Il ne peut l'être que dans le cadre d'une méthodologie, disons mieux d'une théorie de la connaissance qui recouvre l'ensemble des connaissances reconnues efficaces. Et c'est non seulement par son aspect logique mais autant par sa validité pratique que par sa cohérence qu'une telle méthodologie devra être jugée. Ici encore, il n'y a pas de dernier critère de justesse qui puisse échapper au principe de dualité [1].

Or, au vu de ces exigences, je ne puis me rallier à la façon dont Carnap introduit et applique la notion de probabilité. Je tiens pour arbitraire le principe selon lequel il imagine, en dehors de tout rapport à l'expérience, l'existence d'une mesure, interprétable comme probabilité, dans un certain univers des énoncés. A mon avis, il n'y a rien dans la pratique du calcul des probabilités qui permette de rejoindre une telle vision des choses.

A. J. Ayer

Je suis en plein accord avec M. Gonseth, y compris sa conclusion. Il n'y a que deux points que je veux très brièvement souligner.

Premièrement, un point très important qui n'a pas été fait ce matin, et qui certainement aurait dû être fait, et qu'il a fait maintenant: que quand on applique le calcul des probabilités, les mesures, si vous voulez, ça ne repose pas, comme ont tendance à le supposer les philosophes, sur des principes très généraux, mais sur ce qu'il appelle des expériences, ce que j'appellerais moi-même des hypothèses spécifiques. Il s'ensuit évidemment que, plutôt que de fonder l'induction sur la probabilité, c'est dans le sens inverse qu'on doit procéder; au fond on fonde la probabilité sur l'induction.

Mais alors, s'il y a un problème d'induction, ce problème serait: comment peut-on se fier à ces expériences ? Et là, je suis encore d'accord avc M. Gonseth: ce n'est pas une question d'avoir quelques principes magiques, mais d'avoir une théorie que l'on peut appliquer comme ensemble et une théorie qui se justifie comme ensemble. Je suis donc absolument d'accord avec ce que vient de dire M. Gonseth.

Je crois que c'est une conclusion très importante dans nos discussions.

(1) C'est à ces exigences que s'efforce de répondre la méthodologie dite ouverte.

Ch. Perelman

Je voudrais dire en quelques mots en quoi je crois que M. Ayer est tout à fait dans les perspectives qui nous intéressent concernant le problème de la justification.

Il nous a bien montré qu'il n'y a pas de principe concernant la nature dont on puisse démontrer qu'il est vrai, parce qu'il est toujours pré-supposé. Mais comme M. Klibansky l'a fait remarquer: après il faut agir. Il faut donc avoir une règle d'action. Cette règle d'action je voudrais la suggérer d'une façon extrêmement générale qui est la suivante: Nous devons traiter de la même façon des cas et des situations essentiellement semblables. Remarquez que cette règle, il n'y a pas moyen de l'écarter, parce qu'elle permet toujours une échappatoire qui est la classification des situations essentiellement semblables ou n'étant pas essentiellement semblables, c'est-à-dire à l'application des concepts et des théories à des situations que l'expérience nous fournit. On dit que l'induction est efficace. Pourquoi ? Parce que chaque fois qu'elle ne fonctionne pas bien, on modifie les concepts, on modifie les théories pour pouvoir de nouveau continuer à s'en servir.

Ce qui montre que l'induction est efficace à condition de ne pas avoir élaboré au préalable tous les concepts que l'on veut appliquer à la probabilité et à l'expérience; c'est pourquoi justement cette théorie «aprioristique» de la description du monde à la Carnap ne peut justement pas être défendable. Elle est défendable dans la mesure où les différentes théories et les concepts sont le résultat de l'application de notre catégorisation à l'expérience.

Mais alors ce principe de traiter de la même façon l'explication extrêmement semblable n'est pas quelque chose qui est lié ni à la nature, ni à l'expérience, mais un principe méthodologique qui nous permet de structurer l'expérience de façon qu'elle soit utilisable.

Mais ce n'est que le détail qui va nous permettre d'apprendre si la structuration a été faite d'une façon à donner des résultats. Remarquons que si on nous dit de justifier ce principe, je dis: «mais qu'est-ce que vous avez contre ce principe ?»

Chaque fois qu'on me montre un argument contre l'utilisation de ce principe, je peux toujours montrer que l'argument vaut contre la mauvaise assimilation des cas considérés comme semblables et qui ne sont pas effectivement semblables. Et c'est pourquoi un aspect de la justification apparaît ici: c'est que je n'ai pas à justifier ce contre quoi des critiques et des objections ne peuvent pas être présentées.

La justification sera essentiellement le fait de réfuter ces objections et ces critiques.

A. J. Ayer

En pratique je suis d'accord. Mais évidemment le problème serait toujours de savoir quels sont les cas essentiellement semblables.

Par exemple on dit: sont essentiellement semblables ceux où il n'y a qu'une différence dans l'espace et dans le temps. Mais là il y a toute une théorie scientifique déjà présupposée.

Il y a aussi toutes les difficultés qu'à soulevées Goodman.

Pour introduire n'importe quel prédicat on peut regarder presque n'importe quels cas comme semblables; il faut, déjà auparavant, comme l'a dit le Professeur Gonseth, disposer d'une distinction, d'une leçon qu'on a tirée de l'expérience, qui dit que ces différences sont importantes ou ces autres différences ne le sont pas. Alors en entendant tout cela, je suis d'accord.

E. Poznanski

I take that the paper by Mr. Ayer is a concrete illustration of the general problem of justification we are discussing. It is much easier to discuss general features of a problem when one is presented with a concrete example. The example has well been chosen because the problem of justification of induction is very topical and the literature around it vast. Mr. Ayer explodes one attempt to justify induction by deriving it from the principle of the uniformity of nature, whatever it means. The problem of justification of induction can be formulated simply as the question why induction works and why it should be applied, or to use the title of a recent paper by Max Black, what is the «raison d'être» of induction. Inductive logic is not in the same convenient situation as deductive logic, although we should not abandon the hope that one day we shall have an organon of inductive logic.

After all, it took about 2300 years for deductive logic to become what it is to-day, while — in comparison — inductive logic is still a baby. So I do not want to defend or to criticize Carnap's attempt, especially since he himself states that his is a very first attempt and other people may devise better systems.

The general problem which I would like to raise here is the problem of the *limits* of justification. We should ask what is the starting point of inductive logic. It has its own rules, not as strict and well formulated as those of deductive logic, but the situation is in certain aspects similar. I would like to draw a parallel between the situation concerning the rules in both logics. In deductive logic we have all kinds of derived rules. To justify them we go back to simpler rules, from them to even more simple, until we arrive to some basic rules like *modus ponens* or the rule of substitution. And then we stop. We cannot do anything else because every attempt to justify modus ponens is doomed, as it seems to me, to failure. If somebody does not accept modus ponens unless it is justified, I am at loss what he expects from me. The same situation, more or less, may occur in inductive logic. There too we have derived and simple rules. And there too we may justify complicated, derived rules by going back to the starting point, to some basic rules. There may be a divergence of views what is this starting point, what inductive rule is to be taken as the basic one. But we arrive finally to a situation where we have to stop. We cannot deductively derive an analytic organon of deductive logic from the world of experience, exactly as we cannot derive Euclidean geometry from the experience of physics. There is, of course, a relation between analytic, mathematical theory and the world of experience. By the way, this relation is not as simple as it is sometimes assumed. I would like to draw your attention to a recent book by Stephan Körner «Theory and Experience» where the relationship between these two realms is carefully analysed. But this analysis shows — among others and as far as I understand it — that one cannot *derive* a theory (be it mathematical or logical) from experience.

If we want to justify the basic rules of induction, it seems to me that the only answer we can give is simply a pragmatic one. Induction has been devised in such a way that it works. The question why inductive logic works is similar to the sometimes discussed problem why mathematics is applicable to the physical world. And the answer is also trivial: it was built in such way that it is applicable. We are confronted with experience and we have to talk about it. We create tools for this purpose, linguistic, logical, mathematical. Carnap's inductive logic has a connection with the principle of the uniformity of the world simply because this kind of uniformity has been built into his language (and our language too). Carnap's language is rather primitive. But it presupposes a kind of uniformity. Probability calculus is also an expression of some kind of uniformity. It works because every one believes in this or that way in the uniformity of nature. I cannot imagine a scientist, a physicist who could work in physics without believing in some sort of uniformity. He has the uniformity before him in the form of general laws, strict or statistical. And he applies induction, but not because the world is uniform but because the principle of uniformity is implicit in his language and inductive procedures are part and parcel of his language.

CH. PERELMAN

L'analyse fort intéressante du problème de l'induction de M. Ayer, nous a montré, d'une façon que je crois convaincante, qu'il n'y a pas moyen de démontrer le principe de l'uniformité de la nature, mais qu'il est *présupposé* dans l'application aux phénomènes naturels, du calcul des probabilités.

Faut-il dont renoncer à ce principe parce qu'il est sans fondement théorique ? Comme M. Klibansky l'a bien fait remarquer, une analyse théorique ne peut pas nous condamner à l'inaction, car nous sommes obligés d'agir. Il nous faut donc des règles d'action.

Je voudrais formuler une règle d'action, utilisable pour le problème de l'induction, mais suffisamment souple pour pouvoir s'adapter à l'expérience. Cette règle, que j'ai analysée ailleurs, sous le nom de *règle de justice,* je la formulerais ainsi: *nous devons traiter de la même façon des cas et des situations essentiellement semblables.*

Seule l'expérience ou notre conscience (quand il s'agit d'appréciation morale) nous dira si des situations concrètes sont ou non essentiellement semblables. Le principe ainsi conçu permet des inductions efficaces, parce que la notion «d'essentiellement semblable» est suffisamment souple pour s'adapter à des situations nouvelles et imprévisibles. En effet les théories et les concepts que l'on élabore, et que l'on modifie lorsque l'expérience nous en montre l'insuffisance, précisent ce qu'il faut entendre, chaque fois, par essentiellement semblable.

Nous voyons ainsi que la règle de justice permet de fournir un principe d'induction efficace, à condition de n'y voir qu'un principe méthodologique dont l'application n'est pas liée à une description du monde a priori, mais à des théories et à des concepts que nous élaborons en fonction d'une adaptation à l'expérience.

Chaque fois qu'une application de ce principe se trouve prise en défaut, c'est à une mauvaise structuration du réel qu'il faudra l'imputer. Le principe s'avère fécond et son utilisation est justifiée, car il nous fournit un instrument permettant de préciser de mieux en mieux, grâce à l'expérience, ce qu'il faut entendre par essentiellement semblable, dans les situations les plus variées. Il s'agit d'un principe heuristique, qui ne peut rester fécond, qu'à condition justement d'admettre l'existence d'un élément d'indétermination sur lequel porteront les modifications et les adaptations indispensables.