PROBABILITY : A NEW LOOK AT OLD IDEAS

T. A. Brody*

Instituto de Física, Universidad Nacional Autónoma de México

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ABSTRACT: Probability is treated in this paper as a concept in natural science and having therefore a theoretical structure, associated measuring techniques, as well as entering into scientific theories the limitation of whose range of validity is explicitly taken into account. The notion of ensemble is generalised from statistical mechanics to account for the variability of the factors not included in a specific model, and the measure of a given type of event on the ensemble is taken as the theoretical definition of probability, while the observed frequencies yield the main experimental technique for measuring it. Some of the implications of this view both in the philosophy of science and in physics are considered.

The nature of probability theory may seem an odd subject to talk about in a symposium in honour of a man whose most outstanding contributions have been in the field of cosmic rays. Yet Professor Sandoval Vallarta has never

* Technical Adviser, Instituto Nacional de Energía Nuclear; work supported in part by the Consejo Nacional de Ciencia y Tecnología.

kept all his intellectual eggs in one basket; and he has always shown a deep interest in the philosophical problems that arise from physics. In fact, I have myself benefitted from his interest, in that he has allowed me twice to speak in his weekly seminars on this matter, and on each occasion enlivened the discussion with penetrating and useful comments. I am very grateful to him, and I am glad to have this opportunity to say so.

Probability, having been the poor cousin of physics during the whole of the nineteenth century, has since then grown, polyp-fashion, and pretty well swallowed physics whole. In spite of this, there are few concepts concerning which there are more confused and conflicting notions to be found in the literature. This is not the place to make an exhaustive catalogue, for it would also be exhausting. Let me just pick out the three most prominent points of view:

Firstly, there are those who in one way or another take a subjective view of probability and consider it as the "degree of rational belief" which we may accord to a statement. This was most carefully elaborated as a consistent theory by Keynes[1], and has been adopted by a good many physicists, notably Jeffreys[2]. (There are important differences in the views put forward by Keynes and Jeffreys; they have, however, in common, that probability is formulated in terms of mental constructs, and thus for the purposes of the present paper we may lump them together.) Secondly, there is the objective view which has been most explicitly formulated by v. Mises[3]: here probability is the "limit to which the relative frequency tends in an infinitely long sequence of events" - and the material basis of this point of view has recommended it to a great many physicists, including even v. Neumann[4]. Lastly, there is what one might call the agnostic position: the mathematicians have clearly defined probability as a special case of measure theory, and therefore the physicists need no longer bother about what probability might be. It is worth pointing out that Kolmogorov[5], who did so much to develop the mathematical theory of probability, was very far from sharing this idea.

The objective and subjective points of view - and of each there are many invariants - both have their advantages. But both suffer from serious difficulties. Thus the objective definition cannot deal with such things as the probability of single events, and there are still many unresolved questions concerning the existence and nature of the limit of a sequence of relative frequencies; the subjective theory, on the other hand, has produced the oddest puzzle of them all, namely why a purely mental state, such as a degree of belief, should turn up as a factor in physical situations where no human being is present; and there are also a number of problems concerning conditional probabilities which it cannot answer - let alone the well-known paradoxes involved in the applications of Bayes' theorem.

In my view this confusion in philosophical notions has also caused confusion in physical arguments. In quantum mechanics, above all, it seems to stand in the way of a sane development of new theories because it obfuscates our understanding of the present theories. In what follows I do not propose to develop any radically new views; rather it is my purpose to restate what has often been said: but I would wish to do it systematically and follow out the consequences. In a sense one could say that I am out to rescue the good points of both philosophic views of probability and throw out their bad ones. But this cannot be done, of course, in the eclectic manner of that famous gentleman who, when asked to give a lecture on Chinese philosophy, hauled down the Encyclopaedia Britannica, read the article on China and that on philosophy, and then combined the information.[6] Instead of such a procedure, I propose to treat probability as a concept in physics (and, indeed, in all of science) rather than a philosophical one, and begin by asking what characterises such a concept.

If one examines how a concept is used in physics, one sees at once that there is very much more to it than most philosophers would be willing to admit. Firstly, there is the theoretical notion, based on what physicists call "handwaving" and leading, where possible, to a mathematical structure which defines the most general properties of the concept; secondly, we have one (and usually several) experimental techniques for measuring either quantitatively or qualitatively what the concept expresses; and then, lastly, the concept is used, together with many others, only in the framework of a specific theory – or of several theories. The first two elements have often been discussed, though it has seldom been recognised that both are needed. The third has rarely been seen as important – yet it is precisely these specific theories in which a given concept occurs that make it meaningful and that at the same time delimit its range of validity. For a theory – any theory – has only a finite range of applicability, within which it yields results that we can use to make predictions with all the required accuracy, but outside which it goes increasingly wrong or even becomes meaningless. This is a very fundamental point, to which I shall return later on.

To apply theory and obtain concrete results, we have to reconcile two elements: on the one hand a physical system – tangible, what the experimenter deals with – and on the other a theoretical model to describe the behaviour of the system. Now the experimental system is isolated from the rest of the universe as far as we can achieve it; but not completely so, or else we could not even observe it (unless we are part of it, and then we could never tell the rest of the universe about it). Thus there are a great many factors that influence its behaviour which escape our control and mostly our knowledge as well. Moreover, in the model we build to describe the system's behaviour we restrict ourselves to those few factors which are relevant to our purpose; all others are neglected. Hence the theoretically modelled part of the system moves and changes under the additional influence of an enormous multitude of factors which we have neglected - either deliberately or through our ignorance of them - and so is only approximately described by our model.

All of this is well enough known; and to deal with the resulting fluctuations in the experimental values, the statistical theory of errors was developed. The idea is that by means of this theory we can get rid of the fluctuations, and thereafter forget there ever was such a thing. Often enough (for instance, all through classical mechanics) this works very well. But are we sure that it will do so always?

The picture we have, then, is that the connections between the factors we study in the experimental set-up in reality "bathe" in a sea of neglected outside and inside influences, while in the corresponding theoretical model we only have the replicas of the limited set of factors under study. In order to improve on this kind of model we clearly need to create a theoretical representation of the sea of neglect. If this sea contains elements which decisively influence the behaviour of the system, then we must change the model and include them explicitly. This, however difficult it may prove in practice, is simply the standard technique of improving one's model until it fits sufficiently well; it will not serve our purpose here, since we want to take into account the effect of a multiplicity of factors which are not to be treated explicitly. But of course our problem has already been solved in a very specific context, in statistical mechanics, where the construction of ensembles is used to allow us to neglect the individual motions of molecules while obtaining global properties of the macroscopic system.

What I am saying, then, is that we can obtain a theoretical model for the concept of probability by generalising the procedures of statistical mechanics and considering ensembles of theoretical replicas of the physical system under study. That we can define a measure on such ensembles and that it satisfies the mathematical requirements needed for us to call it a probability hardly needs demonstrating. Another, and more difficult, question is how to build such an ensemble so as to obtain the necessary representation of the endless multiplicity of neglected and unknown factors; in other words, how to make it imitate sufficiently closely the behaviour of a probabilistic system. This is what has been called, in another context, the external ergodic problem[7].

To put it in another way: how do we define a distribution function over the ensemble when we do not even know what are the underlying variables?

It should be pointed out, however, that in the first place we do not need an explicit form of the distribution function in order to solve a great many problems: often enough some very general properties of it are sufficient, for instance that we can apply the central-limit theorem to the ensemble functions that interest us. And it may be enough merely to postulate that the distribution function exists, so that we can talk of the probability of something or other happening, and be sure we know what we are talking about.

A case of the first sort is the statistical treatment of errors: we do not know what influences the statistical fluctuations of our measurements (or we ignore it deliberately), but the central-limit theorem yields the normal law of errors, the variance in which of course is not predicted by the theory, it is measured. And a case of the second sort is, as I will discuss in a moment, that of quantum mechanics; here we are at best beginning to stretch out our fingers towards the underlying variables; yet the relations among the ensemble averages are surprisingly well given by a mathematical apparatus which does not explicitly involve any ensemble averaging. (We need not consider here the problems treated by means of density matrices; it is sufficient to take the case of pure states.) And in the one case where we do know something about the variables that define the ensemble and how they are related, namely statistical mechanics, the question is rather the other way around: why are the results so general, so that quite different classical ensembles, for instance, yield essentially the same entropy law? This question was first asked by Einstein in the unjustly neglected papers where he introduced the ensemble concept[8]; it has not yet received an answer, although we can now extend it to the quantum ensembles.

If the notion of submerging all our theoretical models in an ensemble provides us with the general idea of probability and with the mathematical apparatus to handle it, the specific constructions of ensembles for specific models allow us to incorporate probability in a given theory and connect it with the other concepts occurring in it. But at the same time we now see that a series of experimental determinations of a fluctuating quantity will be - if we have constructed our ensemble correctly - a sample from it, and the experimental frequency of occurrence of an event in such a series will be an approximation to the ensemble measure for the event. This frequency will approach the theoretical value in a way which is predicted by probability theory as the length of the experimental series goes up; but it will always differ from it by an error whose average our ensemble model allows us to calculate (at least in principle). Thus v. Mises' approach here becomes a measuring technique for probabilities, and the question of going to the limit of an infinite series, physically not realisable, is relegated to the theoretical construction, where we can let the number of replicas of the model in the ensemble tend to infinity without any conceptual qualms.

Let us look at some philosophical consequences of this view. Firstly, probability is "real" in the sense that it does not arise from a human limitation on our comprehension of Nature, but rather expresses the partial character of the separation we can achieve between the factors to be studied in a system and the remainder of the universe, together with the fact that we can still apply our results in situations when all the neglected factors will have changed: in any given situation what is "probabilistic" is thus fixed by the purpose we had in mind when we built the teoretical model. Probability is therefore also relative (however much its existence is a "fact of life"), in that a factor which figures among the explicitly treated ones in one model - and thus behaves in a well-determined way - will be outside the pale in another model of the same physical system and must thus be treated as generating the ensemble. And as a result we may have more than one probability for a given system, differing among each other because they belong to different models; this idea, which will appear quite natural to a physicist, is yet so strange to many philosophers that the frequency theory has sometimes been criticised because it does not offer a unique rule for defining the selection of events to be included in the sequence that v. Mises called a collective[9].

Another point to be made is that we have here an objective notion of probability that allows us to assign a probability to a singular event - on condition that we can conceive of a suitable ensemble of which it is to be an admittedly poor statistical sample. Such is evidently the case for the traditional discussions of whether a certain horse will win in a given race: for we need no actual, experimentally realised, series, we need only be able to imagine replicas of "theoretical races". In practice it may be very doubtful whether this is feasible; but the difficulties are not conceptual. Similarly the definition of conditional probabilities presents no problem, since they are simply calculated over a suitable subensemble of the originally defined one and so have, quite naturally, all the properties of probabilities; at the same time, we need no longer entertain two different "rational degress of belief" concerning the same statement, according as it is surrounded by one or another set of further statements.

However, for one type of object traditionally accorded the honour of having a probability we must now deny it: namely scientific theories. It seems indeed awkward to consider a set of different situations in which a certain theory is deemed to be valid, while everything else varies; presumably "everything" here must refer to other theories with which this one is in one way or another linked, and perhaps even such things as the general logical structure on which scientific research is based should be supposed to vary from replica to replica in the ensemble. We might almost be in Wheeler and Everett's multiplicity of different universes – but will the main features of the theoretical picture of probability I have proposed hold in all of these different universes? If not, then we cannot any longer calculate (or even think of calculating) an ensemble average and thus obtain a probability for the theory we are studying. I cannot see a way in which such a conception can be made definite enough to bear the weight of such a construction as the probability of a theory, and I prefer to abandon it.

But this turns out to be an advantage. For, as I mentioned before, what we can assign to theories are regions of validity. It is understandable that probability notions should have been applied to theories, since the region of validity of theories (as, in fact, the term itself implies) has many of the properties of a measure – all, that is to say, except the vital one of possessing a fixed upper bound; moreover, we do not usually feel certain as to the validity of a theory. To consider the region of validity (or perhaps better, of applicability) for a theory has many advantages which this is not the place to discuss. Let me only mention that while the probability of a statement and its logical contrary (if such a concept holds) must be the same, their regions of applicability will, on the other hand, be mutually exclusive; and since they have no known upper bounds, one may expand without affecting the other: this removes Hempel's famous paradox of the black ravens in the theory of confirmation.

Before discussing some consequences of my ideas in physics, let me mention a mathematical problem which is interesting in its own right and may help to make clear the fundamental notions I have proposed. It is that of the completely deterministic algorithms used in computers to generate random numbers; or, as has been insisted, pseudo-random numbers: "Anyone who considers arithmetical methods of producing random digits is, of course, in a state of sin"[10]. In the typical algorithm of this kind, if x_i is a sequence of such numbers, then $x_{i+1} = f(x_i)$; it seems indeed incredible that there should be functions such that among the sequence no ordering or correlation can be detected, and all statistical tests show it to be a random sequence. Within the framework of the ensemble theory of probability that I am proposing, this becomes much more reasonable once it is observed that all the functions that generate random sequences have in common that they are n:1 and hence have no unique inverse. In other words, with each step of applying the function, some of the information about the previous members of the sequence is excluded from further consideration; there are many different trajectories through the possible numbers which lead to the given value of x_i for any i, and the set of trajectories constitutes an analogy to the ensemble we have discussed. The analogy may be made even closer, but I cannot enter here on this point.

There are of course a great many applications of the probability concept in physics which raise no particular questions; it is perhaps fortunate for cosmic-ray studies, so beset by problems about the origin of what they examine, that one need not also worry about the basic concepts of the stochastic theories used in cascade theory, for instance. But where there are problems, they are usually fundamental.

The simplest sort of problem crops up where the effect of the ensemble on the model predictions can be taken into account simply as fluctuations about the predictions obtained without the ensemble; where, in other words, first-order effects are absent. Such is clearly the case with almost all interpretations of experimental data. Nevertheless, our treatment of probability does underline that this is only a limiting case, and that we cannot have any confidence that it will always apply. To put this differently, we must not, as 19th-century physicists tended to think, try and find a purely deterministic model to explain everything, once we have removed the experimental fluctuations by calculating error limits. On the other hand, we must not fall into an extreme which has become popular in this century, in the sense that everything reduces to a probabilistic situation. As Einstein ought perhaps to have said: "God plays dice, certainly; but quite often He also plays chess."

The situation is more complicated in the case of statistical mechanics, even though the probability model we are using has been derived from this field. The reason is that in formulating the ensembles of statistical mechanics, only the classical and hence deterministically describable behaviour of the molecules is taken into account (or, similarly, that part described by quantum mechanics). It is, however, clear that we should also include the effects of the neglected factors, such as the irregularities and small movements of the container walls, and much more important, such as those due to the fact that classical or quantum mechanics is only an approximation. It is the exclusion of such factors that allows us to formulate the ergodic problem in its present form[11]. The conception of probability I have tried to expound suggests that a reformulation of the ergodic problem may be required. Moreover, its rôle will change: on the one hand, it will to a certain extent merge with the universal problem of adapting theory to experimental knowledge, since that is the meaning of what will certainly survive – the question whether the ensemble average of a phase function $F(q_i, p_i)$ adequately represents the average of the experimental values, $F_{\exp}$, say; the questions involving the time average, where the time averaged over tends to infinity, may lose their meaning in the reformulation. On the other hand, the ergodic problem will acquire much greater importance than it has at present, for its impact will be felt in every physical theory, and not merely in statistical mechanics. All this is, of course, guesswork: most of the work concerning these questions remains to be done. One very fundamental problem will have to be tackled, namely how far the results of ensemble theory can be made independent of the measure used on the ensemble; for in the vast majority of cases to be studied there will be no Liouville theorem to guide our steps.

The last field I shall discuss is, of course, quantum mechanics. Here probability is supposed to play a special rôle, unlike that in other parts of physics. Such ideas have even been developed to the point of proposing negative or complex probabilities. Since nobody has yet been able to give a clear account of what such objects could mean, I propose to see how far we can get by applying the ensemble notion I have outlined. And this is, of course, a critical test: should it fail to yield a consistent account, it would have to be thrown on the scrap heap. Fortunately nothing of the sort happens; we obtain a conceptually much cleaner view of quantum mechanics by means of it, where moreover the doors for reaching beyond the present quantum theories begin to be visible.

Let us then suppose that we really interpret quantum-mechanical expectation values as averages over an ensemble which is a theoretical image of the experimentally accessible values. One immediate result is that the irritating puzzle of the reduction of wave packets disappears. As we saw before, probability is relative in that for a given system we can adopt any one of a number of different points of view, according to the purpose we have in mind when formulating the model; and what is "probabilistic" in one model is "deterministic" in another. Applied to the collapsing wave function, it becomes clear that the collapse is a complete fraud as a physical process: we have a wave function that is spread out over a large volume, and another one that is highly concentrated - and we choose whichever fits our aim best. In other words, it must be explicitly recognised that an electron, say, has more than just *the* wave function, in the same way that a macroscopic object has more than just *the* distance from other objects, that a relativistic particle has more than just *its* mass[12]. The change from one wave function to another is essentially like the change from one *frame of reference* to another. And if we expand our system to include some of the objects surrounding the electron, we can even formulate a wave function which at one time is spread over a large volume and then contracts to go through a minimal concentration, after which it expands again.

All this is in no way new. The statistical interpretation of quantum mechanics has been advanced time and time again, notably by Einstein[13], and has been expounded in a particularly clear form in a recent paper by Ballentine[14]. It must be pointed out, however, that several authors have been misled into stating that the statistical interpretation means that quantum mechanics can only apply to a large set of quantum particles. It should be clear from what I have said concerning single events that according to the ensemble view of probability quantum mechanics applies to individual particles: but they are a statistically poor sample and hence the errors which the theory predicts will be large. As we increase the number of observations in the sample, the sample average will approach the theoretical prediction more and more closely.

In a similar way Heisenberg's uncertainty relations will be seen to apply to the ensemble expectations of fluctuations of the non-commuting quantities, and not to the individual experimental errors. In fact the ensemble underlying quantum mechanics has been formulated without any reference to the factors that go to make up experimental errors, and hence predicts nothing whatever concerning them[15]; it thus comes as no surprise that we can determine a pair of non-commuting variables with a precision greatly exceeding that given by the uncertainty relation. In a backhanded sort of way this was recognised by Heisenberg when he stated that his relations are valid for the future, but not for the past[16].

There is, however, one important point to be mentioned. The ensemble interpretation of probability implies that even if for the moment we do not know the variables involved in the ensemble, we can at least in principle deal with them (though if we do, there will be further variables constituting another ensemble around them in their turn). Put more plainly, there should be hidden variables behind quantum mechanics. Yet there are two theorems that argue against this, v. Neumann's well-known one[17] and a more recent one due to Bell[18]. Or at least that has been the traditional view.

Feyerabend[19] has shown, however, that what v. Neumann's theorem does is simply to exclude the possibility of purely non-dispersive hidden variables. Put differently, the hidden variables behind quantum mechanics must in their turn have non-vanishing variances and must thus be treated by ensemble techniques. But if the ensemble view of probability is accepted, this is only natural. In fact Feyerabend points out that v. Neumann's demonstration is valid also for classical systems; and it may therefore prove possible to turn the theorem around after generalising it suitably, and use it to deduce that in statistical mechanics the hidden variables must have dispersions: this could obviate the need for the rather unexplained hypothesis of molecular chaos (work is being carried out on this question).

The situation concerning Bell's theorem is different. He himself interprets it to mean that only grossly non-local hidden variables are possible, so that systems that were once connected but are now macroscopically independent would still be linked in a quite essential and highly mysterious way; since very few physicists would accept such a view, which runs counter to the (partial) separability of physical systems I have mentioned, this would spell the end of hidden-variable theories. But it has been shown[20] that this interpretation rests on a misconception and cannot be maintained once the effect of a measurement process on the variables characterising the particle (including the hidden ones) is taken into account explicitly. Yet Bell's result, properly interpreted, still shows up a curious correlation between previously connected systems which we certainly do not understand at present. I suspect that some very fundamental principles lurk behind it, making well worthwhile the effort at further study: but only time will show.

Thus the two apparent obstacles to hidden-variable theories disappear; one of them, indeed, turns out to be helpful in that it characterises the type of hidden variables to be expected, and the other may yet prove to be even more illuminating. We thus arrive at the conclusion from our point of view concerning probability that hidden-variable theories are viable and should remove the many perplexities that still surround quantum mechanics. Such a conclusion is indeed fitting, since in this same symposium a very promising hidden-variable theory will be presented[21].

To sum up: I hope to have shown that the ensemble view of probability - in which probability, like other physical concepts, has a much richer structure than is usually assumed - proves helpful in clearing up a number of problems in the philosophy of science as well as in physics, and yields a new perspective on a number of physical questions.

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RESUMEN

En el presente trabajo se trata a la probabilidad como un concepto de las ciencias naturales, el cual por lo tanto posee una estructura teórica y técnicas asociadas de medición, además de que entra en teorías científicas cuya región de validez finita se toma en cuenta explícitamente. La noción de ensemble se generaliza desde la mecánica estadística para rendir cuenta de la variabilidad de los factores que no se incluyen dentro del modelo específico; la medida sobre el ensemble de un determinado tipo de acontecimiento se toma como la definición teórica de la probabilidad, mientras las frecuencias observadas constituyen la principal técnica experimental para medirla. Se comentan algunas de las implicaciones de este punto de vista tanto en la filosofía de la ciencia como en la física.