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THE PROPENSITY INTERPRETATION OF PROBABILITY

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I

IN this paper I intend to put forward some arguments in favour of what I am going to call the propensity interpretation of probability.

By an interpretation of probability—or, more precisely, of the theory of probability—I mean an interpretation of such statements as,

'The probability of a given b is equal to r'

(where r is a real number); a statement which we can put in symbols as follows:

$$\text{'}p(a, b) = r.\text{'}$$

There have been many interpretations of these probability statements, and years ago I have divided these interpretations into two main classes—the subjective and the objective interpretations.[1]

The various subjective interpretations have all one thing in common: probability theory is regarded as a means of dealing with the *incompleteness of our knowledge*, and the number $p(a, b)$ is regarded as a measure of the degree of rational assurance, or of rational belief, which the knowledge of the information b confers upon a (a is in this context often called 'the hypothesis a').

The objective interpretations may also be characterised by a common feature: they all interpret

$$p(a, b) = r$$

as a statement that can, in principle, be objectively *tested*, by means of statistical tests. These tests consist in sequences of experiments: b in $p(a, b) = r$, describes the experimental conditions; a describes some of the possible *outcomes* of the experiments; and the number r describes the *relative frequency* with which the outcome a is estimated to occur in any sufficiently long sequence of experiments characterised by the experimental conditions b.

[1] See my *Logic of Scientific Discovery* (1934, 1959), section 48, and appendix* ii

It is my conviction that most of the usual applications of subjective interpretation of probability are untenable. There may be something like a measurable degree of the rationality of a belief in a, given the information b; but I assert that this belief cannot be adequately measured by a measure that satisfies the laws of the calculus of probability.[1] (I think it likely, however, that ' degree of confirmation or corroboration '—the latter term is preferable—will turn out to furnish, under certain circumstances, an adequate measure of the rationality of a belief; see my notes on ' Degree of Confirmation ', this *Journal*, 1954, **5**, 143, 334, 359; 1957, **7**, 350, and 1958, **8**, 294.)

As to the objective interpretations, the simplest seems to be *the purely statistical or the frequency interpretation.* (I take these two designations as synonymous.) This interpretation regards the statement

$$p(a, b) = r$$

as an estimate, or a hypothesis, asserting *nothing but* that the relative frequency of the event a in a sequence defined by the conditions b is equal to r. Or in other words, the statement ' $p(a, b) = r$ ' is interpreted to mean: ' events of the kind a occur, in sequences characterised by b, with the frequency r '. Thus, for example, ' $p(a, b) = \frac{1}{2}$ ' may mean ' the relative frequency of tossing heads with a normal penny equals $\frac{1}{2}$ ' (where a is getting heads upmost, and b is a sequence of tosses with a normal penny).

The frequency interpretation has been often criticised, but I believe that it is possible to construct a frequency theory of probability that avoids all the objections which have been raised and discussed. I have sketched such a theory many years ago (it was a modification of the

[1] The most characteristic laws of the calculus of probability are (1) the addition theorems, pertaining to the probability of $a \vee b$ (that is, of a-or-b); (2) the multiplication theorems, pertaining to the probability of ab (that is, of a-and-b); and (3) the complementation theorems, pertaining to the probability of $\bar{a}$ (that is, of non-a). They may be written

$$(1)\quad p(a \vee b, c) = p(a, c) + p(b, c) - p(ab, c)$$
$$(2)\quad p(ab, c) = p(a, bc)p(b, c)$$
$$(3)\quad p(\bar{a}, c) = 1 - p(a, c), \text{ provided } p(\bar{c}, c) \neq 1.$$

The form of (3) here given is somewhat unusual: it is characteristic of a probability theory in which

$$(4)\quad p(a, c\bar{c}) = 1$$

is a theorem. The first axiom system for a theory of this kind was presented, as far as I know, in this *Journal*, 1955, **6**, 56. See also my *Logic of Scientific Discovery*, appendix* iv, and the appendix to the present paper.

theory of Richard von Mises), and I still believe that (after some minor improvements which I have made since) it is immune to the usual objections. Thus the reason why I changed my mind in favour of the propensity interpretation was not that I felt I had to give way to these objections (as has been suggested by W. C. Kneale in a discussion of a paper of mine [1]). Rather, I gave up the frequency interpretation of probability in 1953 for two reasons.

(1) The first was connected with the problem of the interpretation of quantum theory.

(2) The second was that I found certain flaws in my own treatment of the probability of *single events* (in contrast to sequences of events), or ' singular events ', as I shall call them in analogy to ' singular statements '.

2

Although the bulk of the present paper is devoted to a discussion of the second of these two points, I wish to mention first very briefly the reasons connected with the first point, because it was the first point in time and importance: it was only after I had developed, and tried out, the idea that probabilities are *physical propensities*, comparable to Newtonian forces, that I discovered the flaw in my treatment of the probability of singular events.

I had always been convinced that the problem of the interpretation of the quantum theory was closely linked with the problem of the interpretation of probability theory in general, and that the Bohr-Heisenberg interpretation was the result of a subjectivist interpretation of probability. My early attempts to base the interpretation of

[1] W. C. Kneale said in this discussion: ' More recently the difficulties of the frequency interpretation, i.e. the muddles, if not the plain contradictions, which can be found in von Mises, have become well known, and I suppose that these are the considerations which have led Professor Popper to abandon that interpretation of probability.' See *Observation and Interpretation*, edited by S. Körner, 1957, p. 80. I am not aware of any ' muddles ' or ' contradictions ' in the frequency theory which have become well known more recently; on the contrary, I believe that I have discussed all objections of any importance in my *Logic of Scientific Discovery* when it was first published in 1934, and I do not think that Kneale's criticism of the frequency theory in his *Probability and Induction*, 1949, presents a correct picture of the situation prevailing at any time since 1934. One objection of Kneale's (see especially p. 156 of his book) was not discussed in my book—that, in the frequency theory, a probability equal to one does not mean that the event in question will occur without exception (or ' with certainty '). But this objection is invalid; it can be shown that every adequate theory of probability (if applicable to infinite sets) must lead to the same result.

quantum theory upon an objective interpretation of probability (it was the frequency interpretation) had led me to the following results.

(1) The so-called 'problem of the reduction of the wave packet' turns out to be a problem inherent in every probabilistic theory, and creates no special difficulty.

(2) Heisenberg's so-called indeterminacy relations must not be interpreted subjectively, as asserting something about our possible knowledge, or lack of knowledge, but objectively, as scatter-relations. (This removes an asymmetry between p and q which is inherent in Heisenberg's interpretation unless we link it with a phenomenalist or positivist philosophy; see my *Logic of Scientific Discovery*, p. 451.)

(3) The particles have paths, i.e. momentum and positions, although we cannot predict these, owing to the scatter relations.

(4) This was also the result of the imaginary experiment ('thought-experiment') of Einstein, Podolski, and Rosen.

(5) I also produced an explanation of the interference experiments ('two-slit-experiments'), but I later gave this up as unsatisfactory.

It was this last point, the interpretation of the two-slit-experiment, which ultimately led me to the propensity theory: it convinced me that probabilities must be 'physically real'—that they must be physical propensities, abstract relational properties of the physical situation, like Newtonian forces, and 'real', not only in the sense that they could influence the experimental results, but also in the sense that they could, under certain circumstances (coherence), interfere, i.e. interact, with one another.

Now these propensities turn out to be *propensities to realise singular events*. It is this fact which led me to reconsider the status of singular events within the frequency interpretation of probability. In the course of this reconsideration, I found what I thought to be independent arguments in favour of the propensity interpretation. It is the main purpose of the present paper to present this line of thought.[1]

3

The subjective interpretation of probability may *perhaps* be tenable as an interpretation of certain gambling situations—horse racing, for

[1] The remaining sections 3 to 5 of this paper follow closely a section of my forthcoming book, *Postscript: After Twenty Years*. See also my paper 'The Propensity Interpretation of the Calculus of Probability and The Quantum Theory', in *Observation and Interpretation*, edited by S. Körner, 1957.

example—in which the objective conditions of the event are ill-defined and irreproducible. (I do not really believe that it is applicable even to situations like these, because I think that a strong case could be made —if it were worth making—for the view that what a gambler, or a ' rational better ', tries to find out, in order to bet upon it, is always and invariably the *objective* propensities, the *objective* odds of the event: thus the man who bets on horses is anxious to get more information about horses—rather than information about his own state of belief, or about the logical force of the total information in his possession.) Yet in the typical game of chance—roulette, say, or dicing, or tossing pennies—and in all physical experiments, the subjective interpretation fails completely. For in all these cases probabilities depend upon the *objective conditions of the experiment.*[1]

In the remaining sections of this paper, the discussion will be confined solely to the problem of interpreting the probability of ' *singular events* ' (or ' occurrences '); and it is the frequency theory of the probability of *singular events* which I have in mind whenever I speak here of the frequency interpretation of probability, in contradistinction to the propensity interpretation.

From the point of view of the frequency interpretation, the probability of an *event of a certain kind*—such as obtaining a six with a particular die—can be *nothing but* the relative frequency of this kind of event in an extremely long (perhaps infinite) sequence of events. And if we speak of the probability of a *singular* event such as the probability of obtaining a six in the third throw made after nine o'clock this morning with this die, then, according to the purely statistical interpretation, we mean to say *only* that this third throw may be regarded as a member of a sequence of throws, and that, in its capacity as a member of this sequence, it shares in the probabilities of that sequence. It shares, that is to say, those probabilities which are *nothing but the relative frequencies* within that sequence.

In the present section I propose to argue against this interpretation, and in favour of the propensity interpretation. I propose to proceed as follows. (1) I will first show that, from the point of view of the frequency interpretation, objections must be raised against the propensity interpretation which appear to make the latter unacceptable. (2) I will next give a preliminary reply to these objections; and I will

[1] A criticism of the subjective theory of probability will be found in my notes in this *Journal*, quoted above, and in my paper ' Probability Magic, or Logic out of Ignorance ', *Dialectica*, 1957, 354-374.

then present, as point (3), a certain difficulty which the frequency interpretation has to face, though it does not, when first raised, look like a serious difficulty. (4) Ultimately I will show that in order to get over this difficulty, the frequency interpretation is forced to adopt a modification which appears to be slight at first sight; yet the adoption of this apparently slight modification turns out to be equivalent to the adoption of the propensity interpretation.

(1) From the point of view of a purely statistical interpretation of probability it is clear that the propensity interpretation is unacceptable. For propensities may be explained as possibilities (or as measures or 'weights' of possibilities) which are endowed with tendencies or dispositions to realise themselves, and which are taken to be responsible for the statistical frequencies with which they will in fact realize themselves in long sequences of repetitions of an experiment. Propensities are thus introduced in order to help us to explain, and to predict, the statistical properties of certain sequences; and *this is their sole function.* Thus (the frequency theorist will assert) they do not allow us to predict, or to say, *anything whatever* about a single event, except that its repetition, under the same conditions, will generate a sequence with certain statistical properties. All this shows that the propensity interpretation can add nothing to the frequency interpretation except a new word—'propensity'—and a new image or metaphor which is associated with it—that of a tendency or disposition or urge. But these anthropomorphic or psychological metaphors are even less useful than the old psychological metaphors of 'force' and of 'energy' which became useful physical concepts only to the extent to which they lost their original metaphysical and anthropomorphic meaning.

This, roughly, would be the view of the frequency theorist. In defending the propensity interpretation I am going to make use of two different arguments: a preliminary reply (2), and an argument that amounts to an attempt to turn the tables upon the frequency theorist; this will be discussed under (3) and (4).

(2) As a preliminary reply, I am inclined to accept the suggestion that there is an analogy between the idea of propensities and that of forces—especially fields of forces. But I should point out that although the labels 'force' or 'propensity' may both be psychological or anthropomorphic metaphors, the important analogy between the two ideas does not lie here; it lies, rather, in the fact that both ideas draw attention to *unobservable dispositional properties of the physical world,* and

thus help in the interpretation of physical theory. Herein lies their usefulness. The concept of force—or better still, the concept of a field of forces—introduces a dispositional physical entity, described by certain equations (rather than by metaphors), in order to explain observable accelerations. Similarly, the concept of propensity, or of a field of propensities, introduces a dispositional property of singular physical experimental arrangements—that is to say, of singular physical events—in order to explain observable frequencies in sequences of repetitions of these events. In both cases the introduction of the new idea can be justified only by an appeal to its usefulness for physical theory. Both concepts are 'occult', in Berkeley's sense, or 'mere words'.[1] But part of the usefulness of these concepts lies precisely in the fact that they suggest that the theory is concerned with the properties of an *unobservable* physical reality and that it is only some of the more superficial effects of this reality which we can observe, and which thus make it possible for us to test the theory. The main argument in favour of the propensity interpretation is to be found in its power to eliminate from quantum theory certain disturbing elements of an irrational and subjectivist character—elements which, I believe, are more 'metaphysical' than propensities and, moreover, 'metaphysical' in the bad sense of the word. It is by its success or failure in this field of application that the propensity interpretation will have to be judged.

Having stressed this point I proceed to my main argument in favour of the propensity interpretation. It consists in pointing out certain difficulties which the frequency interpretation must face. We thus come to the point (3), announced above.

(3) Many objections have been raised against the frequency interpretation of probability, especially in connection with the idea of infinite sequences of events, and of limits of relative frequencies. I shall not refer to these objections here because I believe that they can be adequately met. Yet there is a simple and important objection which has not, to my knowledge, been raised in this form before.

Let us assume that we have a loaded die, and that we have satisfied ourselves, after long sequences of experiments, that the probability of getting a six with this loaded die very nearly equals 1/4. Now consider a sequence *b*, say, consisting of throws with this loaded die, but including a few throws (two, or perhaps three) with a homogeneous and symmetrical die. Clearly, we shall have to say, with respect to

[1] See my 'Note on Berkeley as a Precursor of Mach', this *Journal*, 1953, **4**, 21 (4).

each of these few throws with this correct die, that the probability of a six is 1/6 rather than 1/4, in spite of the fact that these throws are, according to our assumptions, *members of a sequence* of throws with the statistical frequency 1/4.

I believe that this simple objection is decisive, even though there are various possible rejoinders.

One rejoinder may be mentioned only in passing, since it amounts to an attempt to fall back upon the subjectivist interpretation of probability. It amounts to the assertion that it is our special *knowledge*, the special *information* we have concerning these throws with the correct die, which changes the probability. Since I do not believe, for many reasons, in the subjective theory of probability, I am not inclined to accept this assertion. Moreover, I believe that the case before us even suggests a new argument (although not a very important one) against the subjective theory. For we may not know which of the throws are made with the correct die, although we may know that there are only two or three such throws. In this case it will be quite reasonable to bet (provided we are determined to bet on a considerable number of throws) on the basis of a probability 1/4 (or very close to 1/4), even though we do know that there will be two or three throws on which we should not accept bets on these terms, if only we could identify them. We know that, in the case of these throws, the probability of a six is less than 1/4—that it is, in fact, 1/6; but we also know that we cannot identify these throws, and that their influence must be very small if the number of bets is large. Now it is clear that, as we nevertheless attribute to these unknown throws a probability of 1/6, we do not mean by the word ' probability ', and cannot possibly mean by it, a ' reasonable betting quotient in the light of our total actual knowledge' as the subjective theory has it.

But let us now leave the subjective theory entirely aside. What can the frequency theorist say in reply to our objection ?

Having been a frequency theorist myself for many years, I know fairly well what my reply would have been.

The description given to us of the sequence b shows that b is composed of throws with a loaded die and of throws with a correct die. We estimate or, rather, we conjecture (on the basis of previous experience, or of intuition—it never matters what is the ' basis ' of a conjecture) that the side six will turn up in a sequence of throws of the loaded die with the frequency 1/4, and in a sequence of throws with the correct die with the frequency 1/6. Let us denote this latter sequence, that of throws with the correct die, by 'c'. Then our information as to the composition of b tells us (i) that $p(a, b) = 1/4$ (or very nearly so), because almost all throws are with the loaded die, and (ii) that bc—that is to say the class of throws belonging to both b and c—is not empty; and since bc consists of throws belonging to c, we are entitled to assert that the singular probability of a six, among those throws which belong to bc, will be $1/6$—by virtue of the fact that these singular throws are members of a sequence c for which we have $p(a, c) = 1/6$.

I think that this would have been my reply, by and large; and I now wonder how I could ever have been satisfied with a reply of this kind, for it now seems plain to me that it is utterly unsatisfactory.

Of course there is no doubt as to the compatibility of the two equations

$$\text{(i)}\quad p(a, b) = 1/4,$$
$$\text{(ii)}\quad p(a, bc) = 1/6,$$

nor is there any question that these two cases can be realised within the frequency theory: we *might* construct some sequence b such that equation (i) is satisfied, while in a selection sequence bc—a very long and virtually infinite sequence whose elements belong both to b and to c—equation (ii) is satisfied. *But our case is not of this kind.* For bc is not, in our case, a virtually infinite sequence. It contains, according to our assumption, at most three elements. In bc the six may come up not at all, or once, or twice, or three times. But it *certainly* will not occur with the frequency $1/6$ in the sequence bc because we *know* that this sequence contains at most three elements.

Thus there are only two infinite, or very long, sequences in our case: the (actual) sequence b and the (virtual) sequence c. The throws in question belong to both of them. And our problem is this. Although they belong to both of these sequences, and although we only know that these particular throws bc occur somewhere in b (we are not told where, and we are therefore not able to identify them), we have no doubt whatever that in their case the proper, the true singular probability, is $1/6$ rather than $1/4$. Or in other words, although they belong to both sequences, we have no doubt that their singular probability is to be estimated as being equal to the frequency of the sequence c rather than b—simply because they are throws with a different (a correct) die, and because we estimate or conjecture that, in a sequence of throws with a correct die, the six will come up in $1/6$ of the cases.

(4) All this means that the frequency theorist is forced to introduce a modification of his theory—apparently a very slight one. He will now say that an admissible sequence of events (a reference sequence, a ' collective ') must always be a sequence of repeated experiments. Or more generally, he will say that admissible sequences must be either virtual or actual sequences which are *characterised by a set of generating conditions*—by a set of conditions whose repeated realisation produces the elements of the sequence.

If this modification is introduced, then our problem is at once solved. For the sequence *b* will not be any longer an admissible reference sequence. Its main part, which consists only of throws with the loaded die, will make an admissible sequence, and no question arises with respect to it. The other part, *bc*, consists of throws with a regular die, and belongs to a virtual sequence *c*—also an admissible one —of such throws. There is again no problem here. It is clear that, once the modification has been adopted, the frequency interpretation is no longer in any difficulty.

Moreover, it seems that what I have here described as a ' modification ' only states explicitly an assumption which most frequency theorists (myself included) have always taken for granted.

Yet, if we look more closely at this apparently slight modification, then we find that it amounts to a transition from the frequency interpretation to the propensity interpretation.

The frequency interpretation always takes probability as relative to a sequence which is assumed as given; and it works on the assumption that a probability is *a property of some given sequence*. But with our modification, the sequence in its turn is defined by its set of *generating conditions*; and in such a way that probability may now be said to be *a property of the generating conditions*.

But this makes a very great difference, especially to the probability of a singular event (or an ' occurrence '). For now we can say that the singular event *a* possesses a probability $p(a, b)$ owing to the fact that it is an event produced, or selected, in accordance with the generating conditions *b*, rather than owing to the fact that it is a member of a sequence *b*. In this way, a singular event may have a probability even though it may occur only once; for its probability is a property of its generating conditions.

Admittedly, the frequency theorist can still say that the probability, even though it is a property of the generating conditions, is equal to the relative frequency within a virtual or actual sequence generated by these conditions. But if we think this out more fully it becomes quite clear that our frequency theorist has, inadvertently, turned into a propensity theorist. For if the probability is a property of the generating conditions—of the experimental arrangement—and if it is therefore considered as depending upon these conditions, then the answer given by the frequency theorist implies that the virtual frequency must also depend upon these conditions. But this means that we have to visualise the conditions as endowed with a tendency, or disposition, or propensity, to produce sequences whose frequencies are equal to the probabilities; which is precisely what the propensity interpretation asserts.

4

It might be thought that we can avoid the last step—the attribution of propensities to the generating conditions—by speaking of mere possibilities rather than of propensities. In this way one may hope to avoid what seems to be the most objectionable aspect of the propensity interpretation: its intuitive similarity to 'vital forces' and similar anthropomorphisms which have been found to be barren pseudo-explanations.

The interpretation of probabilities in terms of possibilities is of course very old. We may, for the sake of the argument, suppress the well known objections (exemplified by the case of the loaded die) against the classical definition of probability, in terms of *equal* possibilities, as the number of the favourable possibilities divided by the number of all the possibilities; and we may confine ourselves to cases such as symmetrical dies or pennies, in order to see how this definition compares with the propensity interpretation.

The two interpretations have a great deal in common. Both refer primarily to singular events, and to the possibilities inherent in the conditions under which these events take place. And both consider these conditions as reproducible in principle, so that they may give rise to a sequence of events. The difference, it seems, lies merely in this: that the one interpretation introduces those objectionable metaphysical propensities, while the other simply refers to the physical symmetries of the conditions—to the equal possibilities which are left open by the conditions.

Yet this agreement is only apparent. It is not difficult to see that mere possibilities are inadequate for our purpose—or that of the physicist, or the gambler—and that even the classical definition assumes, implicitly, that equal *dispositions, or tendencies, or propensities to realise the possibilities in question*, must be attached to the equal possibilities.

This can be easily shown if we first consider equi-possibilities very close to zero. An example of an equi-possibility very close to zero would be the probability of any definite sequence of 0's (heads) and 1's (tails) of the length n: there are 2^n such sequences, so that in the case of equi-possibility, each possibility has the value $1/2^n$ which for a large n is very close to zero. The complementary possibility is, of course, just as close to one. Now these possibilities close to zero are generally interpreted as 'almost impossible', or as 'almost never realising themselves', while, of course, the complementary possibilities, which are close to one, are interpreted as 'almost necessary', or as 'almost always realising themselves'.

But if it is admitted that possibilities close to zero and close to one are to be interpreted as predictions—'almost never to happen' and 'almost always to happen'—then it can easily be shown that the two possibilities of getting heads or tails, assumed to be exhaustive, exclusive and equal, are also to be interpreted as predictions. They correspond to the predictions 'almost certain to realise themselves, in the long run, in about *half* of the cases'. For we can show, with the help of Bernoulli's theorem (and the above example of sequences of the length n) that this interpretation of possibilities $1/2$ is *logically equivalent* to the interpretation, just given, of possibilities close to zero or to one.

To put the same point somewhat differently, mere possibilities could never give rise to any prediction. It is possible, for example, that an earthquake will destroy tomorrow *all* the houses between the 13th parallels north and south (and *no* other houses). Nobody can calculate this possibility, but most people would estimate it as exceedingly small; and while the sheer possibility as such does not give rise to any prediction, the estimate that it is exceedingly small may be made the basis of the prediction that the event described will not take place ('in all probability').

Thus the estimate of the *measure* of a possibility—that is, the estimate of the probability attached to it—has always a predictive function, while we should hardly predict an event upon being told no more than that this event is possible. In other words, we do not assume that a possibility as such has any tendency to realise itself; but we do interpret probability measures, or 'weights' attributed to the possibility, as measuring its disposition, or tendency, or propensity to realise itself; and in physics (or in betting) we are interested in such measures, or 'weights' of possibilities, as might permit us to make predictions. We therefore cannot get round the fact that we treat measures of possibilities as dispositions or tendencies or propensities. My reason for choosing the label '*propensity interpretation*' is that I wish to emphasise this point which, as the history of probability theory shows, may easily be missed.

This is why I am not intimidated by the allegation that propensity is an anthropomorphic conception, or that it is similar to the conception of a vital force. (This conception has indeed been barren so far, and it seems to be objectionable. But the disposition, or tendency, or propensity, of most organisms to struggle for survival is not a barren conception, but a very useful one; and the barrenness of the idea of a vital force seems to be due to the fact that it promises to add, but fails to add, something important to the assertion that most organisms show a propensity to struggle for survival.)

To sum up, the propensity interpretation may be presented as retaining the view that probabilities are conjectured or estimated statistical frequencies in long (actual or virtual) sequences. Yet by drawing attention to the fact that these sequences are defined by the manner in which their elements are generated—that is, by the experimental conditions—we can show that we are bound to attribute our conjectured probabilities to these experimental conditions: we are bound to admit that they depend on these conditions, and that they may change with them. This modification of the frequency interpretation leads almost inevitably to the conjecture that probabilities are dispositional properties of these conditions—that is to say, propensities. This allows us to interpret the probability of a *singular* event as a property of the singular event itself, to be measured by a conjectured *potential or virtual* statistical frequency rather than by an *actual* one.

Like all dispositional properties, propensities exhibit a certain similarity to Aristotelian potentialities. But there is an important difference: they cannot, as Aristotle thought, be inherent in the individual *things*. They are not properties inherent in the die, or in the penny, but in something a little more abstract, even though physically real: they are relational properties of the experimental arrangement—of the conditions we intend to keep constant during repetition. Here again they resemble forces, or fields of forces: a Newtonian force is not a property of a thing but a relational property of at least two things; and the actual resulting forces in a physical system are always a property of the whole physical system. Force, like propensity, is a relational concept.

These results support, and are supported by, my remarks about the role of b—the second argument—in '$p(a, b)$'; and they show that, although we may interpret 'b' as the name of a (potential or virtual) sequence of events, we must not admit every possible sequence: only sequences which may be described as repetitions of an experiment are admitted, and which may be defined by the method of their generation, that is to say, by a generating set of experimental conditions.

5

There is a possibility of misinterpreting my arguments, and especially those of the preceding two sections. For they might perhaps be taken as illustrating the method of *meaning analysis*: what I have done, or tried to do, it could be said, is to show that the word 'probability' is used, in certain contexts, to denote propensities. I have perhaps even encouraged this misinterpretation (especially in section 3) by suggesting that the frequency theory is, partly, the result of a mistaken meaning analysis, or of an incomplete meaning analysis. Yet I do not suggest putting another meaning analysis in its place. This will be clearly seen as soon as it is understood that what I propose is *a new physical hypothesis* (or perhaps a metaphysical hypothesis) analogous to the hypothesis of Newtonian forces. It is the hypothesis that every experimental arrangement (and therefore every state of a system) generates physical propensities which can be tested by frequencies. This hypothesis is testable, and it is corroborated by certain quantum experiments. The two-slit experiment, for example, may be said to be something like a crucial experiment between the purely statistical and the propensity interpretation of probability, and to decide the issue against the purely statistical interpretation.

Note added in proof. In the February number of this *Journal* (1959, **9**, p. 307), Dr I. J. Good has referred to my propensity interpretation. Since this reference contains a misunderstanding, it may be useful, in the interest of clarity, to explain this misunderstanding here.

Dr Good assumes, as basic, a logical or subjective interpretation of $p(a, b)$; we may indicate this by writing $P(a, b)$ and read

$$P(a, b) = r$$

approximately as follows: 'The information b makes it rational to believe in a with a degree of belief equal to r.' Now Good asserts that propensities, in my sense (or, as he prefers to say, physical probabilities), may be defined as special cases of logical or subjective probabilities, as follows. Let H represent all true laws of nature; then we can call

(PP) $$P(a, bH)$$

the physical probability of a given b.

As against this assertion we should realise that many, or perhaps all, of the laws involved in H will be laws asserting a probability; that is to say, H in its turn will be of the form (or it will entail statements of the form)

(H) $$p(a, b) = r,$$

In this case, H is the assertion that under the conditions b, there is a propensity equal to r for a to realise itself, according to my view of the matter.

Now we may accept, as a *principle of logic*, that whenever H is (or entails) '$p(a, b) = r$', then

(PP) $$P(a, bH) = r$$

is *logically true*. Perhaps this is what Dr Good has in mind. But if we accept this principle, there is still a need to interpret the probability statement H. This need is quite independent of (PP), and cannot be replaced by accepting (PP), since H, which occurs in (PP), has to be given some meaning or interpretation.

Dr Good suggests to take H in (PP) as 'understood', and omit it, writing

(P) $$P(a, b) = r,$$

provided we have agreed that this should mean exactly the same as (PP).

Now (P) looks, of course, very much like H; and this may explain why Dr Good takes it for H (that is, for my propensity statement). Yet (P) is very different from H. This may be best seen as follows.

According to our *principle of logic* (PP) or (P) will be *logically true whenever* $H =$ '$p(a, b) = r$'; therefore, logical probability of (P) will be equal to 1. But nobody will assert that the logical probability of the empirical statement H equals 1. (On the contrary, if H is the product of all laws of nature—which we may never discover—its logical probability will be, according to all authors, very small; and according to some authors—for example myself—it will be zero.)

Thus $H \neq$ (P); and the identification of the logical statement (P) with the empirical statement H about propensities is mistaken: it is impossible to subsume in this way propensities (or any other objective probabilities) under logical or subjective probabilities.

Appendix

In conclusion I wish to add a historical remark, and some remarks on axiom systems for probability.

The distinction between subjective, logical, and objective (statistical) interpretations of probability which I made in 1934 in my *Logic of Scientific Discovery* (referred to in what follows as '*op. cit.*'), section 48, was used for arguing the thesis that within physics, at any rate, only statistical probability is relevant. (I now wish to replace, in this thesis, the statistical by the propensity interpretation.) But I made considerable use, in this work, of the logical interpretation also (especially in order to show that content = logical improbability). In 1938 I argued in favour of a '*formal*' theory of probability, based upon an axiom system 'constructed in such a way that it can be . . . interpreted by any of the proposed interpretations . . . and by some others also (*op. cit.* p. 320). In looking for these other interpretations, with a view to the needs of quantum theory, I found the propensity interpretation. I also found that formerly (in *op. cit.* section 71, especially p. 212) I had explicitly argued against accepting an interpretation of this kind.

In my own mind, the freedom of operating with different interpretations was closely connected with the adoption of a formal or axiomatic treatment of probability, as envisaged, for example, by Kolmogorov (see *op. cit.*, p. 327).

In Kolmogorov's approach it is assumed that the objects a and b in $p(a, b)$ are sets (or aggregates). But this assumption is not shared by all interpretations: some interpret a and b as states of affairs, or as properties, or as events, or as statements, or as sentences. In view of this fact, I felt that in a formal development, no assumption concerning the nature of the 'objects' or 'elements' a and b should be made; and it appeared to me desirable not even to assume that these 'objects' or 'elements' satisfy the laws of Boolean algebra (although I found that they do): it became desirable to assume axioms of a 'metrical' character only. Another point was that it became desirable to construct a theory in which the formula (4) mentioned in the second footnote to the present paper, i.e.

$$p(a, c\bar{c}) = 1$$

was a theorem: this, it turned out, was a condition of adequacy for the logical interpretation, and desirable on general grounds.

The first system of the kind here described was published by me in this *Journal*, 1955, **6**, 56, and I simplified its axioms in 1956.[1]

This simplified system, and a number of variants, were discussed in some detail in appendix * iv of *op. cit.* I shall state here one more of its variants.[2] The system uses as undefined terms the class S of the 'objects' or 'elements' a, b, . . .; the product element ab of the elements a and b; and the complement-element $\bar{a}$ of the element a. There are three axioms.[3]

Postulate A. If a and b are elements of S then $p(a, b)$ is a real number, and the following axiom holds:

A $$(\mathrm{E}c)(\mathrm{E}d)\ p(a, b) \neq p(c, d).$$

Postulate B. If a and b are in S, then ab is in S, and the following axiom holds:

B $$(p(a, a) = p(bc, d)\ \&\ p(bc, c) = p(d, c)) \rightarrow p(ab, c) = p(a, d)p(b, c) \leqslant \leqslant p(a, c).$$

Postulate C. If a is in S, then $\bar{a}$ is in S; and provided b, c, and d are also in S, the following axiom holds:

C $$p(a, a) \neq p(b, c) \rightarrow p(a, c) + p(\bar{a}, c) = p(d, d).$$

Both B and C are immediate consequences (using only substitution and *modus ponens*) of the following more complicated formulae BD and CD which, however, have the advantage that they may be regarded as *explicit definitions* of ab and of $\bar{a}$, respectively (BD is an improved version of the corresponding formula in *op. cit.* p. 336):

BD $$p(ab, d) = p(c, d) \longleftrightarrow (e)(\mathrm{E}f)(p(a, d) \geqslant p(c, d) \leqslant p(b, d)\ \&\ (p(a, d) \geqslant \geqslant p(a, a) \leqslant p(b, d) \rightarrow p(a, a) \leqslant p(c, d))\ \&\ ((p(b, e) \geqslant p(a, a) \leqslant \leqslant p(d, e)\ \&\ (p(b, f) \geqslant p(a, a) \leqslant p(d, f) \rightarrow p(a, a) \leqslant p(e, f))) \rightarrow \rightarrow p(a, e)\, p\,(b, d) = p(c, d))).$$

CD $$p(\bar{a}, c) = p(b, c) \longleftrightarrow (d)(e)(p(a, a) \neq p(d, c) \rightarrow p(a, c) + p(b, c) = = p(e, e)).$$

[1] See my paper 'Philosophy of Science: A Personal Report', in *British Philosophy in the Mid-Century*, edited by C. A. Mace, 1956; the axiom system can be found on p. 191.

[2] As compared with the system of *op. cit.*, p. 332, the present system combines, in B, A2 with B1 and B2. C is the C^s of p. 334.

[3] The following *abbreviations* are used: '(x)' for 'for all elements x in S'; '$(\mathrm{E}x)$' for 'there is at least one element x in S such that'; '. . . → . . .' for 'if . . . then . . .'; '⟷' for 'if and only if'; '&' for 'and'.

Aesthetically both of these definitions suffer from the disadvantage that one half of the double arrow is redundant: in deriving the axioms B and C we have to use the arrow from the left to the right only. Definition Cd, which can replace CD, is free from this disadvantage.[1]

$$\text{Cd} \qquad p(\bar{a}, b) = p(c, c) - p(a, b) \longleftrightarrow (\mathrm{E}d)p(c, c) \neq p(d, b).$$

In BD we may put '$p(e, e)$' for the second occurrence of '$p(a, a)$'. (This makes A3, *op. cit.* p. 332, deducible from BD.) We may then simplify CD or Cd, writing '$p(a, a)$' for '$p(e, e)$' or '$p(c, c)$'.

Compared with the system in *op. cit.* p. 332, both B and BD incorporate A2. Incorporating A2 with *any* of the axioms has the advantage that the resulting system is 'completely metrical' in the sense that *the independence of all axioms can be proved with the help of examples that satisfy Boolean algebra.* (Thus 'completely metrical' is a stronger property of a system than 'autonomously independent' in the sense of *op. cit.* pp. 343-344.) We can achieve a completely metrical system without sacrificing the 'organicity' (in the sense of the Warsaw School) of our axioms, by retaining all the axioms (including B1) of *op. cit.* p. 332 except A2; for A2 can be incorporated organically with B2, for example, by omitting '$\leqslant p(a, c)$' from the formula B, above. Alternatively, we can leave even B2 in its original form and incorporate A2 organically with postulate AP of p. 333, as follows:

$$\text{AP} \quad p(a) = p(a, b) - p(a, c) + p(a, d),$$
$$\text{provided } p(b, c) = p(c, b) = p(d, e) \text{ for every } e \text{ in } S.$$

In this case AP—that is to say, a definition of absolute probability—becomes an integral and indispensible part of the system.

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[1]This is due to the fact that Cd is logically stronger than C since it allows us to replace A by a logically weaker conditional formula; for in the presence of Cd, we may add to A the proviso, 'provided (Ee) (Ef) $p(e, f) \neq 0$' (or in words, 'provided not all probabilities are equal to zero'). The strength of Cd is due to the fact that, with the arrow from right to left only, Cd would be the same as C, while the arrow from left to right allows us, in addition, to deduce that not all probabilities are zero.

It may also be mentioned here that the condition of B, as formulated in the text, may be replaced by the (stronger) condition, '$(e)\ p(bc, e) = p(d, e)$'. (This replacement corresponds to the transition from formula A2+, on p. 335 of *op. cit.*, to A2 on p. 332.)