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Empirical Interpretations of Probability

It is clear that the classical principle of indifference was neither so clear-cut nor so compelling as to lead to universal agreement. Indeed, the more clearly was the principle formulated by its defenders, the more quickly and easily were counter-examples devised. It was also observed long ago that in many uses of probability—particularly those associated with insurance, with vital statistics, with scientific experiments in biology and agriculture, as these began to be performed in the nineteenth and twentieth centuries—the only way of arriving at probabilities was not to compute numbers of equally likely alternatives, but to count instances and to take the probability of an event to be indicated by the relative frequency with which that event occurred among the instances counted. For example, no insurance company would calculate the probability of death of an American male of the white-collar class during

his thirty-ninth year in any way but by looking at the records: What proportion of the recorded cases of thirty-nine year old white-collar American males are cases in which death occurred during the thirty-ninth year?

There are three ways in which we can utilize the insight provided by the use of probability in vital statistics, agriculture, and the like, in such a way as to come up with an empirical interpretation of probability. One may literally *identify* probabilities with relative frequencies, one may take probabilities to be the *abstract counterparts* of relative frequencies, or one may take probabilities to characterize certain *kinds* of events: chance set-ups, or events exhibiting certain kinds of *propensities*.

There are many variations on the view that probabilities are to be identified with relative frequencies. Most of these interpretations of probability are more or less derivative from the views of Richard von Mises, whose influential book, *Probability, Statistics and Truth*, was published in 1928. Among mathematical statisticians there are still many who regard von Mises' view that probability should be interpreted as the limit of a relative frequency in an unending sequence as the latest (reasonable) word. There are many more people, both mathematicians and philosophers, who have views regarding the interpretation of probability that are closely related to but not identical with von Mises' views. After we have looked at von Mises' interpretation of probability, we shall look at some of the variations on it.

According to von Mises, "The theory of probability [is] a science of the same order as geometry or theoretical mechanics. . .just as the subject matter of geometry is the study of space phenomena, so probability theory deals with mass phenomena and repetitive events" (p. vii). To talk to the probability of an event, for von Mises, makes sense only if we have in mind some definite *collective* relative to which we have defined this probability. At first, von Mises was unclear as to whether a collective was to be understood as an actual, practically unlimited sequence of events, or whether it was to be understood as strictly infinite, so that one might speak of the existence, in such sequences, of mathematical limits in the strict sense. The current view adopted by von Mises (in *Mathematical Theory of Probability*) explicitly takes a collective to be a mathematical entity, representing an idealization of an empirical reality. The empirical reality corresponding to a collective is a sequence of events of a certain sort. This sequence may be practically unlimited, in the sense that we can (in general) always extend it a little further; we can always toss the coin a few more times, or take a few more observations on the emission of alpha particles from X.

Within the collective, we single out a certain subsequence; when we speak about probability, we are speaking about a certain subsequence in a collective. The subsequence corresponds to an event of a certain *sort* (heads on a toss of a coin, death during the thirty-ninth year, etc.). To say that the probability exists is to say that within the collective the limit of the relative frequency of members of the subsequence exists; "the probability of heads is a half" is to be interpreted as meaning that there is a certain infinite collective (representing tosses of a coin); there is a certain subsequence of that collective (representing tosses yielding heads); and that the limit of the relative frequency of elements of the subsequence in the collective exists and has the value $\frac{1}{2}$.

We demand here convergence to a limit in the strict mathematical sense. That is, if T is the infinite collective representing tosses of a coin, and H the subsequence representing tosses that result in heads, then we say that the limit of H in T exists and has the value p, if and only if: for every δ, no matter how small, it is possible to find an N with the property that for every n greater than N, if we calculate the proportion of members of H among the first n elements of the collective, that proportion will differ by less than δ from p. Let us denote the relation frequency of H among the first n elements of T by $f_n(H)$. The assertion that the limit of the relative frequency of H in T is p can then be briefly expressed symbolically:

$$(\delta)(\exists N)(n)(n > N \supset |f_n(H) - p| < \delta).$$

This is clearly an unverifiable theoretical assertion about a model which is taken to represent a certain empirically given sequence in the same sense that no set of observations will definitely verify it. It is not asserted—indeed it obviously could not be asserted—that there is some way of *calculating* N as a function of δ. Nothing is asserted but the bare existence of such an N. The assertion is therefore not refutable, either. This has been regarded as a defect of the limiting-frequency interpretation of probability. But in view of the fact that probability under this interpretation becomes a relatively high-level theoretical concept, this is not a serious shortcoming. Similar remarks concerning unverifiability and unfalsifiability could be made about such perfectly acceptable concepts as length. One could plausibly say that to assert that the length of a table is four feet is to assert that, for any δ you choose, there exists a process of measurement such that the result of applying that process of measurement to the table will yield a result that will (probably) differ from four by less than δ. It does not seem that the verification or falsification of assertions of probability are any more problematic, on the limiting-frequency interpretation, than that of many other empirical statements about which we feel no qualms at all.

There is one more requirement that von Mises imposes on collectives, and that is that they must be *random* with respect to the subsequence or type of entity under consideration. Randomness is defined as independence under place selection. This means that if we consider, instead of the whole sequence $E_1, E_2, E_3, \ldots$, the subsequence consisting of every other one of the E's, E_1, $E_3, E_5, \ldots$, or the subsequence consisting of every third or fourth of the E's, or the subsequence consisting of every E with a prime subscript, or...any other way of selecting places by subscript, then we will find the same relative frequency of H type elements in the subsequence as we found in the sequence as a whole.

It was thought for some time that the notion of randomness might lead to difficulty. In particular, of course, it is self-contradictory to demand that the limiting relative frequency be independent of *all* place selections, for there will be at least one place selection (actually, there will be an infinite number of them) which will select events in such a way that the limiting frequency of H type elements will be one, rather than p. If we cannot suppose the limiting frequency to be insensitive to all place selections, perhaps we may nevertheless suppose that it is insensitive to all of a certain set of place selections. One obviously relevant set of place selections is the set of place selections which select every second, or every third, or...or every nth element of the original sequence. If for every n it is true that the subsequence consisting of every nth member of the original sequence, starting from some arbitrary place, will exhibit the same limiting frequency of H as the original sequence, then the original sequence is called a Bernoulli sequence. It is possible to show that Bernoulli sequences exist; indeed one can give a *rule* for constructing sequences of 0's and 1's which are Bernoulli sequences. This possibility proves that there is nothing self-contradictory about the limiting-frequency model, combined with the stipulation that the limiting frequency be insensitive under all place selections of the sort just described. It can also be shown that if the limiting frequency is insensitive under all place selections of the sort just described, then it is also insensitive to place selections of the kind: take as the subsequence every element of the sequence that follows two non-H's and an H; or take as a subsequence every element of the sequence that follows five consecutive H's; and the like.

But there are still many place selections that are not taken account of by Bernoulli sequences. For example, the place selection consisting of the E's with a prime subscript, already mentioned, might still, in a Bernoulli sequence, exhibit a limiting frequency different from that in the original sequence. Indeed, no matter how many place selections we think of taking account of, there will be others that we may wish to add to our list. How far can we go in extending the list of place selections that will not lead to a different limiting frequency, or to no limit at all? The strongest result that has been proved so far is this: that given a sequence, the members of which possess certain properties, the assumption that the limits of the relative frequencies of these properties exist, and that they are insensitive to any finite or denumerably infinite set of place selections, cannot lead to a contradiction. Thus although we cannot claim insensitivity to all place selections in our theoretical model, we can claim insensitivity to any arbitrary denumerable set of place selections.

Given the existence of collectives, von Mises has little trouble in showing that the limits of the relative frequencies of properties in collectives satisfy the axioms of the probability calculus. Thus if K is a collective, and H is a property defined for elements of that collective, then if the limit of the relative frequency of H is p, the limit of the relative frequency of $\bar{H}$ is $1 - p$. The general form of the multiplication rule requires the combination of two collectives; the new collective, to which the new probability of H and T refers, must be construed as a set of pairs, one member of which belongs to the collective to which the probability of H is referred, the other of which belongs to the collective to which the probability of T is referred. This kind of argument shows that limits of relative frequencies in collectives are a model of the probability calculus, and thus that the limiting frequency interpretation is an interpretation of that calculus.

Reichenbach and, more recently, Salmon offer a limiting-relative-frequency interpretation of probability in which von Mises' formal requirement of randomness is regarded as inessential. Reichenbach takes probability sequences (corresponding to von Mises' collectives) to be empirically given sequences of events, ordered, usually, by time. He admits mathematically given probability sequences as useful for formal and illustrative purposes in the probability calculus, but denies them any practical import. Furthermore, these empirically given sequences may actually be finite, as in fact most if not all empirically given sequences are. Any sequence may serve as an appropriate sequence in which to consider a relative frequency; there is no need to stipulate any form of disorder in the sequence. The formal requirement of randomness is replaced by a pragmatic or epistemological requirement concerning the choice of an appropriate empirical reference sequence for each particular application of the theory. The particular sequence which it is appropriate to consider in a given context will depend on our state of knowledge at the time (as, in fact, it also does for von Mises, despite his requirement of randomness). The formal definition of probability in a sequence is precisely the same as von Mises'—it is the standard mathematical definition of a limit. Reichenbach, unlike von Mises, applies it to finite sequences as well as infinite ones.

Salmon and Reichenbach argue that the rules for the application of probability (and they both mention such natural rules of thumb as "base your expectations on the probabilities in the smallest relevant reference class about which you have information") are not part of the meaning of probability, but part of a network of pragmatic issues that concern its use. For example, we may have grounds for believing the relative frequency of death in the thirty-ninth year among American males to be 0.012, and among American male white-collar workers to be 0.009, and we may not have enough information to make a good estimate of the corresponding death rate among American male schoolteachers. If we are going to sell insurance to an individual whom we know to be a thirty-eight-year-old American male schoolteacher, we will use the death rate 0.009. There is no formal justification for this; the relative frequency (probability) 0.012 is just as "valid" as the relative frequency 0.009, and the latter is no more valid than the unknown relative frequency p of death during the thirty-ninth year among American male schoolteachers. Our use of the probability 0.009 depends on a large number of pragmatic factors (such as the competitive situation among insurance companies, our feeling that this is a more appropriate number than 0.012, and the like) which Reichenbach and Salmon take to be extraneous to the scientific meaning of the probability statement. It is only the scientific meaning of probability, they argue, that enters into the probability calculus, and it is this that is captured by the concept of a limiting frequency.

In addition to this standard relative-frequency interpretation of probability, Reichenbach offers a metalinguistic interpretation of probability in terms of truth frequencies: the probability of a statement of a certain sort is taken to be the relative frequency of truth of that type of statement in a reference class consisting of an infinite (or finite) number of statements. On the basis of this interpretation of probability he constructs a probability logic of an infinite number of truth values. Nevertheless this is *not* a logical interpretation of probability; it is still empirical, for in order to estimate the truth frequency of statements of certain kinds in reference classes, we must look to the world to provide us with evidence concerning the truth or falsity of the statements that enter into the sequences. On a genuinely logical interpretation of probability, such as Carnap's, we need not look at the world at all, but need merely examine the purely logical properties of certain groups of statements.

It should be observed that in the empirical interpretations of probability which undertake to define probability in terms of the limits of relative frequencies, there is a strong element of idealization. The empirical sequences we encounter in the world are always finite, and we often have good reason to suppose that they cannot be infinite. We cannot have an infinite number of people living under given historical and social conditions; we cannot suppose in a physical experiment that we have an infinite number of helium nuclei in various stages of excitation. Furthermore any initial segment of an infinite sequence is compatible with any *limiting* frequency, or indeed with there not being a limiting frequency at all. In the case of von Mises' interpretation of probability, there is the added element of idealization involved in the supposition that the sequences are random, in his special sense. There have been two reactions to these elements of idealization in the empirical interpretations of probability. One has been to limit the idealization involved by taking the theory of probability to be concerned not with just any sequence of events, but with certain sequences encountered in the world which come close to meeting the ideal conditions; the other has been, in the best mathematical tradition, to make the sort of idealization, and to leave the applications to the applied scientists and the shopkeepers. The former reaction seems to be closer to the style of von Mises; the latter closer to the style of Reichenbach.

Most mathematicians and statisticians prefer the latter approach, in which, nowadays, one begins with an abstract axiomatic formulation, and takes 'probability' to be anything that satisfies the axioms of the probability calculus. The first to adopt this pure-mathematical approach was Kolmogorov; he was quickly followed by other mathematicians whose concern was with probability as a branch of measure theory rather than with the empirical applications of the theory. Even among those who have been concerned with the applications of probability theory, or with the interpretation of probability, this approach has often been adopted. Harald Cramér, for example, takes probability to be simply an "abstract counterpart" to observable relative frequencies in large classes of phenomena. We simply observe that the relative frequencies of certain types of events, within certain classes of experiments or trials, seem to be fairly stable; and we propose a stochastic (probabilistic) model to account for and represent this stability. The model is simply the abstract mathematical system itself. It in no fundamental way differs from other mathematical models. Sometimes one sort of model is useful and sometimes another sort. It is no more a part of the job of the pure mathematician to predict the applicability of a probability model than it is a part of his job to predict when a model involving differential equations will be useful.

One other group of writers who offer an empirical interpretation of probability, somewhat more in the direct tradition of von Mises, feel that the kind of phenomenon to which probability theory can be applied can be characterized abstractly and generally. Not just *any* large group of events may reasonably be described by probabilistic statements, but only those sequences of events which have a certain structure (or lack of structure) corresponding in an empirical way to von Mises' mathematical concept of randomness. Karl Popper, for example, takes as the most interesting and useful concept of probability one that is related to relative frequencies, but not one which is defined in terms of limiting frequencies, and not one which is simply an abstract counterpart of relative frequencies. Probability is a concept characterizing the behavior of certain entities or kinds of entities under certain conditions; it is an abstract property which can best be described as a kind of potentiality or "would-be" that may be (but need not be) expressed by mass behavior. Thus when we ascribe the probability $\frac{1}{6}$ to a one in the case of a certain die, we do not mean to say that the die has been thrown many times, or even that it will be thrown at all. We mean that the die has under the usual circumstances a certain *propensity* to land with the one uppermost; it has this propensity from the moment of its manufacture, whether or not it is thrown. Furthermore, *if* it has this propensity to land with a one up about a sixth of the time, it has it regardless of the outcome of any series of tosses. A startling sequence of ones might (or might not, if we have other and stronger evidence) constitute evidence concerning that propensity; but the finite relative frequencies we can observe do not constitute the only or even the most important evidence we have about propensities.

Another view which limits the application of probability concepts to a rather special class of events is that developed by Ian Hacking. He eschews the word 'probability' altogether, but says that statistics is concerned with a certain, presumably physical, property which, when it has a chance, expresses itself as a long-run frequency. This property is not taken to be a property of an object but a property of a *chance set-up*. A chance set-up is "a device or part of the world on which might be conducted one or more trials, experiments, or observations..." (p. 13). For Hacking the word 'chance' performs the function which the interpretations discussed in this chapter assign to the word 'probability'. "What the long-run frequency is, or was, or would have been (on the chance set-up) is to be called the *chance* of the outcome" on that chance set-up. It is an empirical concept, characteristic of certain kinds of states of affairs in the world.

The interpretation provided by each of these writers does eventually involve references to mass phenomena, repeatable experiments, and the like. There is no doubt whatever that the concept which these people have in mind and are trying to capture and characterize is an important one in the whole of science. It is central to the theory of measurement (making a measurement may be construed as making a trial on a chance set-up, sampling from a hypothetical and idealized sequence or population, etc.), and it is central to the attempt to organize and simplify any massive body of data. The concept that these writers have focused on is thus indisputably important to all of science, from pure physics to demography. The double question remains: To what extent is it appropriate to call that concept probability? And to what extent is that the concept which is involved in statements that use the word 'probability' and its cognates?

The former part of the question is largely terminological, and much less important than the latter part. To the extent that there is another prevalent concept that goes by the name 'probability', it seems appropriate to have different names for the two concepts. Carnap has proposed distinguishing them by subscripts: 'probability$_2$' for the frequency concept, and 'probability$_1$' for the logical concept corresponding to degree of confirmation. There are a number of writers, whom we shall consider in Part Two of this book, who regard probability$_1$ or degree of factual support as a concept with a completely different structure from that provided by the probability calculus, and who therefore argue that the word 'probability' should be used only for the empirical concept—at least in technical literature. On the other hand there are those who claim that close analysis reveals that the vast majority of uses of the word 'probability', even in technical literature, are uses that are not open to an empirical concept. Since there are many perfectly good terms for the empirical concept ('chance', 'relative frequency', 'proportion' [in an extended and idealized sense], and so on), it seems better to reserve the word 'probability' for the logical concept, and to use the word 'proportion', or better, 'measure' for the empirical concept. Perhaps Hacking's proposal, to do without the word 'probability', is the most rational, but it is very hard to amputate a word from the language. And it is perhaps more politic to provide analytical splints to stiffen its meaning than to let it degenerate altogether.

The main objection to any blanket empirical interpretation of probability statements, whatever particular form that empirical interpretation may take, is that it does not apply to many of the uses to which the word 'probability' is put. Reichenbach is the frequency theorist who has claimed the most for his theory of probability: "The results may be transferred to every application of the probability concept in science or daily life" (p. 10). And yet there are many probability statements that do not seem to adopt themselves comfortably to Reichenbach's analysis. In particular, when one talks about the probability of a scientific hypothesis (the quantum theory), or about the probability of a certain historical event (the probability that Caesar occupied a certain part of France), it seems very questionable indeed that one should be able to find an appropriate long probability sequence. It is even more questionable, of course, that one should know the value of the limit of the relative frequency in the sequence; but as an objection to Reichenbach's theory of probability, this observation is beside the point. Reichenbach asserts that we can never *know* the limits of relative frequencies, and that the best we can do is to make estimates of these limits. The difficulties inherent in this view we shall return to when we consider Reichenbach's contributions to inductive logic in Part Two. The point to observe here is that these difficulties are essentially difficulties of the *application* of Reichenbach's probability concept, and not difficulties in the concept itself. Reichenbach's argument for the universal applicability of his theory is that we are no worse off in making statements about the relative frequencies of truth-values in infinite sequences of statements of an historical nature than we are in making statements about the limiting relative frequency of heads in an infinite sequence of tosses of a well-tested coin.

What is a relevant objection, though, is that on an empirical interpretation, whenever we use the word 'probability' we are uttering a conjecture about a mass phenomenon. This limitation is as relevant to statements about gambling apparatus as it is to statements about historical events. To say that the probability of heads is one half is to utter a conjecture about the collective character of a set of coin tosses, or the potentialities of a chance set-up. It is not to say anything at all about any particular coin toss. Probability is applicable only to *sets* of entities or events, or sequences of events, or experimental arrangements, or chance set-ups, but never to an individual event, or the particular outcome of a particular trial.

But, as should be very clear from the instances of the use of the probability concept with which this book opened, there are many kinds of probability statements in which the general function of the probability concept is to distinguish between sound and unsound conjectures regarding *individual* events, *individual* trials, or *particular* outcomes. In particular, we make probability statements about all kinds of individual propositions: we say that the probability of rain tomorrow is high; that the constant-creation theory of cosmology is more probable than its competitors, that the probability of being killed in a particular plane flight is negligible. Reichenbach would interpret such statements as elliptical formulations of such statements as: on days following days like today, the frequency of rain is high; theories of the type of the constant-creation theory, supported by the kind of evidence that supports that theory, are more frequently true than theories of other specifiable sorts supported by their sort of evidence. It is more plausible to interpret the third statement as an ellipsis, but even here most probability theorists—even those of a frequentist persuasion—would refuse to follow Reichenbach. His formulation simply does too much violence to common usage. When I say that the probability of rain tomorrow is very low, I am not saying something about the set of days like today in some unspecified sense of 'like'. I may be implying that I know (or have reason to believe) something about that set of days, and the relative frequency with which they are followed by rainy days, but that set of days is not what I am talking *about*; I am talking about *tomorrow*. When I talk about the probability that my nervous friend Bob Wilson will survive his flight across the Atlantic next Monday, I am talking about the particular flight that Bob is planning to take, not about the whole set of first-class passenger flights across the Atlantic carried out in Boeing 707 planes.

Most of those who adopt an empirical interpretation of probability would be inclined to agree with all this as an argument against construing *all* probability statements as statistical. They would tend to follow von Mises in denying that probability statements that concern single individuals or events are always elliptical, and in asserting that they are often simply without formal meaning: "The phrase 'probability of death', when it refers to a single person, has no meaning at all for us" (p. 11). Such statements, of course, are not asserted to be without *any* sort of meaning, but only to be without the sort of meaning appropriate to the probability calculus.

Limited to statements employing the indefinite article 'a' (the probability that *a* male American will die in his thirty-ninth year; the probability that *a* roll of this die will result in a one) the empirical interpretation of probability is plausible enough, in general. But the limitation is a surprisingly serious one. In the first place the number of statements in which probability is clearly being used in a way that conflicts with the empirical interpretation is very large. It is a tough-minded empiricist indeed who will assert that "The probability of a six on the next roll of this die is $\frac{1}{6}$" is nonsense and has no empirical meaning. The same will have to be said of "The probability that applicant number 1149-A will die within the next year is 0.0017," "The probability that the first time Hume ever tossed a coin it landed heads is $\frac{1}{2}$," and so on. Nevertheless, there are a goodly number of people who are this tough-minded. Not all of these objectives (as we might call them) are so tough-minded as to see that precisely the same considerations apply also to such probability statements as: "The probability is very high that on the next thousand rolls of this well-tested die, nearly $\frac{1}{6}$ will result in a six," "It is overwhelmingly probable that not more than three out of one-thousand applicants of such and such a character will die during the coming year," and even "Given that the limiting frequency of heads is exactly one half, the probability is overwhelming that the proportion of heads on the next ten-thousand tosses will be less than three quarters." When it comes to statements of this latter category, a great number of objectivists slip into the easy practice of regarding a very high probability as a practical certainty, where *practical certainty* is the sort of concept that *does* apply to definite individual events. Von Mises himself belongs to this group. His assertion that the probability of death of a particular individual is not a subject for the probability calculus has already been quoted. But he also writes, "... the solution of a problem in the theory of probability can teach us something definite about the real world. It gives a prediction of the result of a long sequence of physical events; this prediction can be tested by observation" (p. 63). This statement is simply false. From statements of probability we can only derive other statements of probability, and none of them will concern the probability of any definite event, for such a probability is flatly meaningless on von Mises' theory. It is meaningless whether that event is conceptually simple like the death of a particular individual or conceptually complex like that occurrence of death for less than two-hundred of the ten-thousand individuals to whom we have issued insurance policies. Under no circumstances can we arrive at a probability statement concerning any definite experiment or observation, except by taking a high probability that applies to a characteristic in an infinite class of events of a certain sort to be a practical certainty that we can apply to each member of that class individually.

What kinds of statements do we have left, then, to which the objectivistic theory in one form or another might apply? Statements of probability that employ the indefinite article 'a' to make an assertion about an unspecified member of a class. "The probability of heads on *a* toss of this coin is $\frac{1}{2}$," "The probability that *a* birth is the birth of a boy is 0.51." But even these statements become slightly strange under the objectivistic interpretation. It does seem to be curious that a statement about a whole class of events (an imaginary class at that, generally) is offered as the *meaning* of a statement that in ordinary or scientific language appears to be about a single (though unspecified) individual.

Many of the most important uses of the probability calculus concern distributions and frequencies explicitly (How often will North–South have all the aces in Bridge? What is the distribution of shoe sizes among army recruits?). Many of the most common technical uses of the term 'probability' are adjectival: one speaks of a probability measure, of probabilistic independence, of probability distributions, and probability laws. In all of these cases the important concept is an empirical one, related to relative frequencies. Which of the various empirical interpretations seems most plausible here? The limiting-frequency interpretations of von Mises and Reichenbach seem close to the observed data (finite frequencies), but they seem to involve a surplus of idealization. It is no part of any *use* of probability theory that it refer to unending sequences; it suffices that we be able to use the mathematics that accompanies the limiting-frequency interpretation. This mathematics is available to the purely mathematical treatment of probability. By far the most prevalent empirical interpretation of probability nowadays is that which takes probability to be simply an abstract counterpart of empirically observable frequencies (but, like all mathematical abstractions, better behaved). The hope of any peculiarly intimate connection with observations is abandoned. Probabilistic or stochastic theories are theories like any other theories; it is just as hard to define empirical support for these theories as for any others. They shed no more light on their own confirmation or disconfirmation than any other theory does. As long as all of this is kept clearly in mind, the use of the word 'probability' in these contexts need not be confusing; and it is perhaps easier to keep these things in mind on the "abstract counterpart" interpretation than on a limiting-frequency interpretation or any other interpretation involving reference to a specialized model.

EXERCISES

1. Provide the details of the argument that in an infinite sequence, an assertion about a limiting frequency, '$(\delta)(\exists N)(n)(N < n \supset |f_n - p| < \delta)$', is neither definitively verifiable nor definitively refutable.
2. Show that in an infinite sequence, '$(\delta)(\exists N)(n)(n > N \supset |f_n - p| < \delta)$' is not tautologous.
3. Show that if the limit of the relative frequency of a certain property in a certain sequence exists, it has a value less than or equal to one.
4. Von Mises shows that limits of relative frequencies in collectives satisfy the axioms of the probability calculus. In order to do this, he is required to show that there are ways of operating on collectives to arrive at new collectives with new limiting frequencies related in the appropriate ways to the old limits. For example, the proof of the addition rule proceeds by applying what von Mises calls the *mixing* operation to a collective. Let a collective be given in which the limiting frequency of H is p, and in which the limiting frequency of E is q, and in which the properties H and E are exclusive. The mixing operation consists in mixing together E and H, forming, say $G = E \cup H$. We then assert (1) the limit of the relative frequency of G in the given collective exists; (2) it has the value $p + q$. Prove these assertions.
5. Another operation on collectives is that of *partition*. Given a collective and a property E, one may consider the sequence consisting of those members of the original collective that have the property E; one may take this as a new collective (being a partition of the old one), and in it consider the relative frequency of another property, H. Suppose that the limit of the relative frequency of E in a collective is p, and that the limit of the relative frequency of H in the partition of the same collective by E is q. Then, in the original collective, the limit of the relative frequency of $G = H \cap E$ exists, and has the value pq.
6. Construct (i.e., prove a rule for constructing) an infinite reference class A in which the probability of B does not exist.
7. Show that if A is a finite sequence, the limit of the relative frequency of B in A always exists.
8. Show that the probability axioms hold for Reichenbach's interpretation of probability.
9. Examine the appropriateness, pro and con, of the empirical interpretation of 'probability' in the following statements.
   (a) The probability is $\frac{1}{6}$ that a roll of this die will result in a six.

(b) The probability of rain tomorrow is 0.4.
(c) The probability of snow this Christmas is high.
(d) The probability of getting at least one head on the next ten tosses of a coin is about 999 out of 1000.
(e) The probability of getting about $\frac{1}{6}$ sixes on a thousand rolls of this die is very high.
(f) The probability that the train will be on time is high.
(g) The probability that a strong and warlike nation will be invaded by one of its weaker neighbors is rather small.
(h) It is unlikely that Russia would have been invaded by Czechoslovakia.

BIBLIOGRAPHICAL NOTES FOR CHAPTER 4

John Venn (*The Logic of Chance*, Macmillan, London, 1886) was the first to propose that probability be *identified* with the limiting frequency in an infinite reference class; Venn argued that the probability of heads on a toss of a coin was to be regarded as p if and only if, as the sequence of tosses was extended indefinitely, the relative frequency of heads got arbitrarily close to p. Venn did not work out the mathematical details of his proposals; those details were provided by von Mises in his *Wahrscheinlichkeit, Statistik, und Wahrheit*, J. Springer, Berlin, 1928. Quotations are from the most recent English version, *Probability, Statistics, and Truth*, a translation of the third revised German edition of 1951, The Macmillan Company, New York, 1957.

Hans Reichenbach's limiting-frequency interpretation of probability first appeared in German in 1934, although his ideas were talked about well before that. *The Theory of Probability*, University of California Press, Berkeley and Los Angeles, 1949, is a translation and revision of the German *Wahrscheinlichkeitslehre* of 1934. It is in that work that the most far-reaching claims for the frequency theory of probability are made, and where the attempt is made to construct an empirically interpreted probability logic. The currently most active defender of this view is Wesley Salmon; but his remarks on the subject are to be found mainly in his discussions of various problems of induction, to which reference will be made later.

The basic reference for Kolmogorov's axiomatic treatment of probability is "Grundbegriffe der Wahrscheinlichkeitsrechnung," in *Ergebnisse der Mathematik*, **2**, No. 3, Berlin, 1933. The translation is *Foundations of the Theory of Probability*, Chelsea, New York, 1950. For the current approach to these questions in mathematical statistics, see Harald Cramér, *The Elements of Probability Theory*, John Wiley and Sons, New York, 1955. Chapters I and II lead up to and present a statistician's interpretation of probability as a counterpart of relative frequencies, connected only by relatively informal and flexible criteria to actual observed frequencies.

R. B. Braithwaite, a philosopher, presents a conceptual-counterpart view in his book *Scientific Explanation*, Cambridge University Press, 1953, Chapters V and VI. Braithwaite, however, proposes to make the connections between the model and the reality of observed frequencies explicit through the use of a formal rule of rejection for statistical hypotheses.

Ian Hacking, in *The Logic of Statistical Inference*, Cambridge University Press, 1966, shows that Braithwaite's rule of rejection is not adequate as a formal rule providing the connection between model and reality; he offers instead a whole new approach to statistical inference (to which we shall recur later), and bases it on his chance set-up view of probability. Karl Popper's somewhat similar views are to be found in "The Propensity Interpretation of Probability," *British Journal for the Philosophy of Science* **10**, 1960, pp. 25–42.

Details concerning the consistency of the *collective* in which a characteristic occurs randomly and yet approaches a limit are to be found in A. H. Copeland, "Consistency of the Conditions Determining Kollektives," *Transactions of the American Mathematical Society* **42**, 1937, pp. 333–57, and Abraham Wald, "Die Widerspruchsfreiheit des Kollektivebegriffe der Wahrscheinlichkeitsrechnung," *Ergebnisse eines mathematischen Kolloquiums* **8**, 1937.