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Chance—II

By *D. H. Mellor*

The term "chance" is used in a variety of ways, but chiefly as a kind of probability, and this is the use discussed here. This kind of probability, sometimes called "physical" or "statistical" (as opposed to "inductive") probability, has long been familiar and is a concept central to statistical science. *See* **Probability, Foundations of—**I. It has nonetheless not proved easy to make sense of, and its still current rival theories are all open to objection. The problem is that chance is credited with several characteristics that are not readily combined, and no one theory accounts well for all of them.

The concept of chance applies to **events** (trials) with a number of possible consequences (outcomes), no one of which is certain to ensue: e.g., heads or tails as outcomes of tossing a coin; the decay or otherwise of a radium atom in a specific stretch of time. There is a distribution of chances over these possible outcomes, and this is a **probability distribution** in the sense of being a measure satisfying the axioms of the standard mathematical calculus of probabilities. *See* **Axioms of Probability**. The chance distribution is supposed to be a property of the trial and to have the following characteristics. The chance of an outcome is the fair betting quotient for a bet that the outcome will occur. It is also the limit toward which the proportion or **relative frequency** of occurrence of that outcome would tend if sufficiently similar trials were repeated endlessly. The property is supposed to be empirical: the fair betting quotients and relative frequencies involved cannot be deduced a priori. It is also objective: these quotients and frequencies exist whether we know it or not, and their actual values are independent of what we believe them to be. And although the content and value of our beliefs about the trial and its properties are no doubt relative to the evidence we have about it, the chance distribution is not.

No theory of chance makes equally good sense of all these aspects of it, and which theory one adopts will depend on which one takes to be its central and which its peripheral and dispensable aspects. In what follows the historically more important theories are sketched and what seem to the author the most promising current developments are outlined.

In **Laplace's classical theory** [3], chance is not an objective feature of the real world, which **Laplace** took to be deterministic, i.e., such that only the actual outcome of a trial was ever really possible. Chance on this theory is merely a measure of our ignorance of that outcome. A number of outcomes would be consistent with what we do know about a trial: e.g., a number of trajectories of a coin tossed in a certain way. Of these, some proper fraction will result in the coin landing heads, and this fraction, on the classical theory, is the chance of that outcome.

Since proper fractions trivially satisfy the probability calculus, the classical theory explains at once why chances are probabilities. Indeed, historically it was under this classical conception that the probability calculus was originally developed, with application especially to gambling on **games of chance**. In contexts of drawing cards and throwing what are supposed to be unloaded dice, the classical idea of chances as

This article was originally published online in 2006 in Encyclopedia of Statistical Sciences, © John Wiley & Sons, Inc. and republished in Wiley StatsRef: Statistics Reference Online, 2014.

fractions of a finite number of possibilities is very natural. The **laws of large numbers** in particular have a natural interpretation in these contexts and under this theory, since repeating a trial simply multiplies the possibilities; and by these laws the theory readily explains why the chance of an outcome is also its limiting frequency in endlessly repeated trials. It also seems intuitively right to match one's betting odds to the proportion of ways one thinks a bet can be won rather than lost, and to that extent the theory explains why a classical chance is a fair betting quotient.

The classical theory fails, however, to explain the *objective* and nonrelational aspects of chance. Indeed, as already remarked, it denies them, since it presupposes determinism and makes chances relative to the evidence we have about a trial. Moreover, once given the evidence, the empirical character of chance is obscure: it is a fraction, not of actual events that could be counted empirically, but of *abstract* possibilities which have somehow to be counted a priori. Before possibilities can be counted, they must be identified, and this the theory attempts to do by means of a principle of indifference. Each possibility is equally probable, since any two outcomes that can each occur in only one way have by definition the same chance. The principle of indifference (or "insufficient reason") therefore attempts to identify these equally probable possibilities as those that we are, in Laplace's words, "equally undecided about in regard to their existence." The eventual supersession of the classical theory by frequency theories of chance resulted largely from the seemingly insuperable difficulties of giving this principle a usable, intelligible and nontrivial content, objective and free of paradox.

Another theory, more influential as an account of other kinds of probability, is the logical theory that treats probability as a quasi-logical relation of partial entailment between evidence and some hypothesis to which the evidence relates. The object of the theory, advanced by Keynes [2] and later developed by Carnap [1] and others, is to account in terms of this relation for how logically inconclusive evidence may still support (or "confirm") a hypothesis to a greater or lesser extent. Where the relation is taken to have a measure satisfying the probability calculus, it forms a basis for probabilistic confirmation theories, and the probabilities involved are called "inductive" probabilities. These are prima facie quite a different kind of probability from chance. The two are alike in being objective, but there the similarity ends. Chance is empirical and nonrelational, whereas inductive probability is a priori and relative to evidence. Indeed, hypotheses about chances will be among these which have inductive probabilities relative to inconclusive evidence about chance trials. Carnap thought as a result that there were two distinct concepts of probability: inductive, to which the logical theory applied; and statistical, to which the frequency theory applied. These concepts must of course be connected: in particular, an event's inductive probability, relative to the evidence that its chance is p, must obviously be p; and theories of chance and of inductive probability must between them explain why this is so.

It is pertinent to note that the concept of inductive probability has proved so problematic that many thinkers have wished to dispense with it. Some have abandoned the idea of objective probabilities in this context, and resorted instead to Bayesian principles for changing **subjective probabilities**, i.e., **degrees of belief**, in acquiring new evidence about a hypothesis. But others have tried to make chance do the work of inductive probability. The **Neyman–Pearson theory** is a notable instance, and the role of chance in this theory should be briefly illustrated here. Prima facie a measurement of a length, for example, makes various possible values of the actual length more or less probable, depending on the accuracy of the measuring device. These probabilities measure the reliability of inferences drawn from the measurement: e.g., that, with 98% probability, the actual length is between 25 and 26 centimeters. But these probabilities are clearly inductive. They cannot be chances, because the measurement cannot be regarded as a trial with different actual values of the length as possible outcomes. The one and only actual length was there before the measurement was made, and is in no sense an outcome of it (setting aside the disputed case of quantum measurement). So if inductive probability is taboo, a different way must be found of assessing the safety of inferences drawn from the measurement, these still being that the actual length lies between limits containing the measured value. Now the result of the measurement, the measured value, can certainly be regarded as an outcome of a chance trial; given the actual length, there may well be definite chances of an interval, centered on the measured value, including or excluding it. So sense can be made of the chance of a measurement leading one, by a rule of inference, to say that the length is between certain limits when it is not. This is the chance of the *rule* leading one into the error of accepting a false hypothesis about the length; and there may also be a definite chance of the rule leading one into the error of rejecting a true hypothesis. The same account can be given of measuring any quantity, including in particular the measurement of chance itself by observing, e.g., the frequency of heads in many tosses of a coin. So in terms of the chances of these ***two types of error***, and of the relative importance of avoiding them, the Neyman–Pearson theory is enabled to evaluate the reliability of a number of important rules of statistical inference* without appealing to inductive probabilities and without retreating into subjectivism. Chance therefore has an important, if controversial, role in the foundations of statistical inference.

In many respects the most obvious account of chance is that given by the frequency theory of Venn [5] and his successors, which superseded the classical theory and which has remained the dominant theory until quite recently. On it the chance of a trial of some kind F having an outcome of kind G is the relative frequency f with which G outcomes occur in the class of all F trials. Thus the chance of heads being the outcome of tossing a coin is taken to be the relative frequency with which heads is the outcome of all tosses of that coin and of coins just like it. Chances are thus identified with the content of statistical laws to the effect that $100f$ percent of F trials have G outcomes.

For some F, the set of F events may be infinite (e.g., all possible trials to see whether any radium atom decays in any period of a year), and proportions in infinite sets are strictly undefined. Here the chance of a G outcome is identified with the limiting proportion of G outcomes in a sequence of successively more inclusive finite sets of F trials. Not all such proportions or limits are chances, however; we should not speak of chance if a G event were the outcome of every second F event. So arbitrary selections from the set of all F events are also required to have the same limiting proportion of G outcomes—a proviso known for obvious reasons as the principle of the impossibility of gambling systems! *See* **Games of Chance**.

A single trial of course belongs to many sets, with generally different proportions of their members having G outcomes. To get an individual trial's chance of being so followed, these sets are intersected until further intersections cease to give different proportions. Only then are the statistics taken to yield chance as a property of an individual trial, a statistical law applicable to the so-called "single case." Suppose, for example, that only smoking and gender affect the proportion of people getting cancer, and that I am a nonsmoking male. My chance of cancer is equated with the eventually cancerous proportion of nonsmoking males, regardless of their other attributes.

If the world is in fact deterministic, this process of repeated intersection will not yield nontrivial chances unless some limit is put on the sets to be intersected. Otherwise, any apparent chance other than 1 or 0 will be destroyed by intersection with the sets corresponding to different values of whatever "hidden variables" in fact determine the actual outcome of the trial (e.g., the imperceptible differences of initial conditions that make the difference between a tossed coin landing heads rather than tails). In practice, however, intersections are usually limited by the taxonomy of some theory within which the supposed statistical law is explicitly or implicitly formulated. (The completeness of the taxonomy usually remains an open question, and so therefore do the theory's implications for determinism.)

The frequency theory explains many of the characteristics of chance listed earlier. Like the classical theory, it explains chance being a kind of probability, because relative frequencies automatically satisfy the probability calculus. It obviously explains why a chance is a limiting frequency on repeated trials, since it makes this the definition of chance. This property, moreover, has the virtues of being plainly empirical, objective, and not relative to the evidence we have about the trial; and we have seen above how a frequency sense can be made of chance as a property of a single trial.

The theory's Achilles' heel is its inability to explain why the chance of an outcome is the fair quotient for betting that it will occur. The reason the frequency theory fails to explain this aspect of chance is that it makes the chance of a G event depend logically on the existence and fate of all other F trials in the whole universe: past, present, and future; and this seems quite irrelevant to the prospects of winning a bet on the outcome of one particular trial.

The usual response to this objection is to abandon *actual* frequencies (and their limits) and to redefine chance instead as the limiting frequency of *G* outcomes in an endlessly long run of sufficiently similar *hypothetical* trials. This, however, is to abandon the frequency theory in all but name, since "sufficiently similar" means "similar enough to have the same chance." But then the laws of large numbers* will suffice to get as high a chance as anyone may demand for the individual's chance being as close as one likes to such a hypothetical limiting frequency; and these laws are theorems of the probability calculus, however probability is then interpreted. Chance's connection with merely hypothetical long-run frequencies neither depends on nor supports a frequency interpretation of what chance itself is.

Once the shift is made from actual to merely hypothetical frequencies, chance is being regarded as a disposition or "propensity". The theory of chance as a propensity was explicitly distinguished from frequency theories by Popper in 1957 [4], since when several propensity theories of chance have been developed. The analogy is with nonstatistical dispositions such as fragility: what gives an object this property is that it would break if dropped, but it has the property whether it is dropped or not. Analogously, a coin has its chance of landing heads on a toss whether it is tossed again or not, even though the chance is defined by what the limiting frequency of heads would be in an infinite class of similar tosses.

This type of propensity theory, however, is still based on frequency and, although it shares the virtues of frequency theory, it also fails to explain why chances are fair betting quotients. Why should a proportion of cancers in other people, actual or hypothetical, be the measure of *my* prospects of getting the disease? The other type of propensity theory defines chance instead in terms of the prospects of an actual *F* trial having a *G* outcome, letting the laws of large numbers take care of chance's connection with the limiting frequencies in hypothetical sets of similar trials.

This alternative approach starts from the subjective or personalist concept of degree of belief. My degree of belief in a trial's having a *G* outcome measures how much I expect it to. To say there is a definite prospect that it will is to say that the trial is such as to make some definite degree of belief* in the outcome objectively right; and this degree of belief is what the chance is taken to be.

In saying this we do not need to credit people with actual degrees of belief admitting of indefinitely precise measurement, any more than ordinary objects actually have indefinitely precise shapes, temperatures, masses, etc. Any quantitative state, whether physical or psychological, is no doubt best represented by an interval of values rather than by a single value. But we can still say that chances make some degrees of belief objectively right, meaning that the intervals of values representing the strengths of people's actual belief states should include these values.

Chances defined in this way are probabilities, because degrees of belief have been shown by subjectivists to have a measure satisfying the probability calculus. Specifically, so-called coherent betting quotients (CBQs) satisfy the calculus. *See* Coherence—I.

The next problem is to say what makes a degree of belief objectively right in these contexts, and to do that this theory also invokes statistical laws. What such a law says in this theory is that all trials similar to this one in a certain respect (e.g., in being *F*) have the same prospect, i.e., chance of having a *G* outcome. These chances being supposed to be objective, and the trials independent, the laws of large numbers apply to them. It can then be shown that in repeated bets on such trials (under the restrictions prescribed to make CBQs measure degrees of belief), a gambler can know he or she will eventually break even only at a CBQ equal to the chance, and this in the circumstances is the best result he or she could know of. The law therefore gives this CBQ a peculiar virtue in these hypothetical compulsory bets, and consequently gives an objective rightness to the degree of belief which the CBQ measures.

The advantage this propensity theory has over its frequency-based rivals is that it can explain why chances are objectively fair betting quotients. The objection to it is that, when it says that certain trials have an empirical objective, nonrelational property of chance, it fails to say what this property is. In other words, it fails to say what in the real world, if not frequencies, makes statements of chance objectively true; and unlike its rivals cannot even offer hypothetical frequencies in lieu of actual ones.

It does not, however, follow that nothing objective makes probability statements true and therefore that subjective theories of probability must be adopted *faute de mieux*. Truth conditions can be supplied for any kind of chance statement at least; i.e., by statistical laws. Thus the laws of radioactivity show the nuclear structure of radium to be what makes true the statement of its half-life, i.e., makes objectively appropriate a degree 0.5 in the belief that a radium atom will decay in that time. There is again an analogy with nonstatistical dispositions such as fragility: what makes true the statement that a glass is fragile (when it is not being dropped and its fragility is not directly observable) is something about its microstructure. The microstructure cannot, of course, be deduced from the meaning of "fragile," nor need any one microstructure be common to all kinds of fragile objects; yet statements about the fragility of objects can be objectively true, and it is the microstructure of those objects that makes them so. So it is with statements about chances.

Probability, then, need not be denied objectivity just because it corresponds rather to degrees of belief in other things than to full belief in some one thing called "probability.". We might, on the contrary, say that a full belief is true just in case a degree close to 1 in that belief is objectively appropriate. So far from objective truth being beyond the reach of probability statements, truth can plausibly be regarded as an extreme case of objective probability.

So much may briefly be said in defense of objective probability, and of a belief-based propensity theory's declining to say with what other objective property of trials chances are to be identified. Objective chance may be distinguished from merely inductive probabilities by the fact that chances, like other physical attributes of events, are credited with having causes and effects. Thus it is held that smoking causes cancer, although the connection between the two is only statistical, not deterministic. What this means is that smoking causes an increase in the chance that the smoker will contract cancer. Conversely, an atomic explosion is caused by changing the chance of a mass of fissile material absorbing its own fission products. No merely subjective or relational account of chance does justice to the way chances are thus embedded in the network of causes and effects which determine the history of the world and both enable and limit our actions within it.

Indeed, the clearer it becomes that the fundamental laws of physics are irreducibly statistical, the greater the pressure to involve chance itself in the analysis of causation. The relation of cause to effect can no longer be credibly restricted to deterministic situations; rather, these must be regarded as extreme cases of probabilistic causation. Therefore, attempts have recently been made to develop theories of physical causation based on the concept of chance; these involve distinguishing causal factors by how the chances of events depend on their presence or absence.

These developments make it unlikely that the concept of chance can be analyzed in terms of **causation**, since causation is itself being taken to depend on chance. They do seem to the author to strengthen the appeal of a belief-based propensity theory of chance, since a central function of the cause-effect relation is to give reason for expecting an effect once its cause is observed. If being a cause involves raising the chance of an effect, where this is understood as raising the objectively right degree of belief in the effect's occurrence, this aspect of causation will be readily accounted for.

Serious problems remain to be overcome in the theory of chance, not least in the problematic area of quantum theory. But the progress made in recent decades in devising an objective theory, free of certain difficulties inherent in the frequency theory, which have driven many philosophers and statisticians to deny the existence of objective probabilities, bodes well for the rehabilitation of the concept of chance.

1 Literature

The following are classic sources or provide useful expositions and further references for the theories of chance discussed. For the classical theory, see P. S. de Laplace, *A Philosophical Essay on Probabilities* (Dover, New York, [3]), and W. Kneale, *Probability and Induction* (Clarendon Press, Oxford, 1949). For the logical relation theory, see J. M. Keynes, *A Treatise on Probability* (Macmillan, London, [2]). For the distinction between statistical and inductive probability, see R. Carnap, *Logical Foundations of Probability* (2nd ed., University of Chicago Press, Chicago, [1]). For more general discussion of kinds of probability, see B. Russell, *Human Knowledge* (Humanities Press, New York, 1948). For the frequency theory, see J. Venn, *The Logic of Chance* (3rd ed., Chelsea, New York, [5]), and W. C. Salmon, *The Foundations of Scientific Inference* (University of Pittsburgh Press, Pittsburgh, Pa., 1967). For frequency-based propensity theory, see K. R. Popper, "The propensity interpretation of the calculus of probability . . ." and discussion in S. Körner, ed., *Observation and Interpretation* (Dover, New York, [4]). I. Hacking, *Logic of Statistical Inference* (Cambridge University Press, Cambridge, 1965), and I. Levi, *Gambling with Truth* (Alfred A. Knopf, New York, 1967). For belief-based propensity theory, see D. H. Mellor, *The Matter of Chance* (Cambridge University Press, Cambridge, 1971). For subjective theory, see F. P. Ramsey, *Foundations* (Routledge & Kegan Paul, London, 1978), and L. J. Savage, *The Foundations of Statistics* (Wiley, New York, 1954). For the link between chance and causation, see W. C. Salmon, "Theoretical explanation" and discussion in S. Körner, ed., *Explanation* (Blackwell, Oxford, 1975).

2 Acknowledgement

This article was written during the author's tenure of a Radcliffe Fellowship and of a British Academy Overseas Visiting Fellowship, for which he is indebted to the Radcliffe Trust and the British Academy.

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