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Equipossibility Theories of Probability

by IAN HACKING

This paper will explain why probability was for so long defined in terms of equally possible cases. The definition is usually attributed to Laplace. It preceded him by a century and survived him by another two. How could so monstrous a definition have survived three hundred articulate years? The trouble with the definition seems obvious enough. One could quote from a score of eminent critics. Here, for example, is Reichenbach discussing attempts to use a principle of indifference:

> Some authors present the argument in a disguise provided by the concept of *equipossibility*: cases that satisfy the principle of "no reason to the contrary" are said to be *equipossible* and therefore equiprobable. This addition certainly does not improve the argument, even if it originates with a mathematician as eminent as Laplace, since it obviously represents a vicious circle. *Equipossible* is equivalent to *equiprobable* [35, p. 353].

Even workers who, in our century, have defended equipossibility have done so because they have philosophical views about the impossibility of producing non-circular definitions. Thus Borel, to whom all probabilists owe so much, maintained that such circles were not vicious. It is an error of logicians, he thought, to try to produce a non-circular definition of probability [8, p. 16].

Reichenbach does not explain why equipossibility had such a successful career. The explanation has two parts. First we require an understanding of concepts of possibility. Second, we must show how those concepts solved or concealed problems about probability that still plague us. We shall show by analysis of four generations of theorists that equipossibility theories were central to the evolution of our concepts of probability.

Two concepts of probability

Some truisms about probability should be set out before beginning the analysis. There are two important distinct kinds of interpretation of our probability axioms. The distinction is nowadays called Carnap's, although long ago both Cournot and Poisson had the same thing in mind [33, p. 31; 12, pp. v, 437–40]. There is an epistemological interpretation and a physical interpretation of the axioms. Both get called probability.

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In the *epistemological* interpretation, *Prob* (a/b) expresses a relation between an hypothesis a and some evidence b. Writers who follow Jeffreys and Keynes maintain that this is a logical relation. *Prob* (a/b) is read as the degree to which a is supported by b, and they think this is a function of logical characteristics of the propositions a and b. Other students despair of finding any objective interpersonal relations of the sort Jeffreys sought. So, following de Finetti, they take *Prob* (a/b) to be a subjective relation, varying from person to person and time to time. It expresses something like the confidence some person thinks he would have in a if he knew just b. In what follows, I shall hardly distinguish these two sub-interpretations, for in the early period we are studying they are submerged.

In the *physical* interpretation, *Prob* (a/b) expresses a physical feature of a chance set-up on which one might make repeated trials. It denotes the relative frequency with which outcomes of kind a would occur among outcomes of kind b. Once again, this notion subdivides further. Some scholars, following von Mises, believe we must analyse this in terms of a limit in an infinite sequence. Others, in the manner of Peirce, favour an analysis as a dispositional property. Once again we shall seldom need to distinguish.

It is to be expected that both the epistemological and the physical interpretations should be wanted early. Thus the seventeenth-century "doctrine of chances" is mostly about objective properties of games and death rates in stable populations. But in that period Pascal could argue that a decision-theoretic problem, whether or not to act so as to come to believe in God, was isomorphic to decision in games [32, and cf. 17]. There are no trials on an objective chance set-up that determine the outcomes "God is", or "God is not". What Pascal needs is the epistemological interpretation. Or take a standard problem from the eighteenth century "Art of conjecture". Trying to locate a heavenly body, one takes imperfect astronomical readings. The distribution of error is believed to be an objective physical characteristic of the measuring device and its object. So the probability of error is open to the physical interpretation. But at the same time one wishes to assert that with some probability, the orb in question lies in a certain region of the sky. An epistemological interpretation is naturally placed upon such statements.

Even today we do not agree on interpretations. Plenty of careful writers find it convenient to use "probability" ambiguously between both interpretations. Some believe the interpretations must be sharply distinguished and try to find relations between them. Others, like Neyman, believe that only the physical interpretation is requisite. Still others, such as de Finetti, take the physical interpretation to deal in mysterious pseudo-entities, so they employ only an epistemological interpretation. In former times, as I shall show, these potential divergences of opinion were played down, partly thanks to the equipossibility idea.

Two kinds of possibility

I began this paper asking why "equally possible cases" could have seemed so valuable a concept in probability theory. The short answer is, that there were two well-established senses of the word "possible". One means epistemic possibility, the other, physical possibility. One of them was used to explain the physical interpretation of probability while the other served to define the epistemological interpretation.

The following analysis demands no study of how the word "possible" is ambiguous. It suffices to accept the fact that some possibilities are physical and some are epistemic. "It is possible that John Arbuthnot was joking when he wrote about chance." That means that *for all we can tell* he may have been joking. "It was possible for Arbuthnot to joke about Queen Anne." That is, Dr Arbuthnot *was able to* joke about his royal patient. The first possibility is relative to our state of knowledge and has long been called epistemic. The second possibility says it was physically possible for that Jacobean wag to joke about his sovereign—neither a dull brain nor a cruel monarch prevented him. The distinction is so sharp that in modern English purely grammatical devices will distinguish the two kinds of possibility [18]. But the following analysis does not need to rely on such deep matters of grammar: almost any theory about kinds of possibility will do.

In the seventeenth century the word "probability" came to indicate new concepts for investigation. In so doing it inherited dualities already extant for "possibility". Apparently the word "probability" comes to mean something that can be measured numerically only with the publication of the Port Royal Logic in 1662 [2, Book iv, ch. 16]. In the sixteenth century the word was chiefly used in assessing theological disputes for matters on which doctors of the church could not unanimously agree. Words like "chances", "odds", "hazards", were used ever so long ago (and are the words of the Pascal-Fermat correspondence) but they become alloyed with something actually called probability only in the seventeenth century. "Probability", then, became a word for new ideas. In so doing, it picked up some features that current speech had already encoded in the word "possibility". That is why equipossibility theories of probability were so durable. Two kinds of probability were needed. The relation between them is obscure even today. By explaining probability in terms of possibility writers of an earlier period could usefully equivocate.

Von Mises (who is even harsher on equipossibility theories than Reichenbach) scrupulously noted that the phrase "equally possible" was used in different senses, and he thereby provides one clue for our enquiry:

Ordinary language recognizes different degrees of possibility or realizability. An event may be called possible or impossible, but it can also be called quite possible or barely possible (*schwer oder leicht möglich*) according to the amount of *effort* that must be expended to bring it about. It is only 'barely possible' to write longhand at 40 words per minute; impossible at 120. Nevertheless it is 'quite possible' to do this using a typewriter. . . . In this sense we call two events equally possible if the same effort is required to produce each of them. This is what Jacques Bernoulli, a forerunner of Laplace, calls *quod pari facilitate mihi obtingeri possit*. . . . But this is not what Laplace's definition means. We may call an event "more possible" (*eher möglich*) than another when we wish to express our *conjecture* about whatever can be expected to happen. There can be no doubt that equipossibility as used in the classical definition of probability is to be understood in this sense, as denoting equally warranted conjectures [30, p. 78].

That observation fits well with a more recent remark by Boudet, who says that the perennial question about probability is whether it is *de re* or *de dicto* [9]. This novel thought adapts a distinction of the schoolmen. A modality is *de re* if it is predicated of an individual, and it is *de dicto* if it is predicated of what is said or thought. In Boudet's terminology, an epistemological probability is *de dicto*, for it concerns relations between propositions or beliefs. Physical probabilities pertain to the state of things in the world, and so are *de re*. In olden times the *de dicto/de re* distinction applied to possibility. [18] shows how the old *de re/de dicto* distinction closely matches the more recent physical/epistemic distinction for possibility. And, as indicated in [20, sec. 9], any kind of possibility carries with it a corresponding concept of probability.

Following Boudet and agreeing with von Mises one might infer that Laplace meant something different from Bernoulli. Bernoulli's possibility has to do with what is physically possible. It might be *de re*. The probability associated with it is therefore also *de re* and is a physical probability. Laplace's possibility is epistemic. It must be *de dicto*. The probability associated with it is therefore also *de dicto* and is an epistemological probability.

On examination of the documents the tentative conclusions just suggested prove superficial but they are a fair preliminary guide to the facts. One inadequacy in the conclusions is this: Bernouilli commenced the transition between the doctrine of chances and the art of conjecture, and as shown in [16] he wavers between the epistemic and physical account of probability. Far being unequivocally *de re* (as one might infer from my selective extracts from Boudet and Mises) he is (as Boudet insists) toying with the epistemic idea. This duality is assisted by relating probability to an ambiguous "possibility". Conversely, Laplace who is generally reported as being thoroughly epistemic reserves the word "possibility" in some mathematical writing for the old *de re* concept—even though this is flatly inconsistent with what he says in his more philosophical asides. Here it is notable that although in our day Laplace's "possibility" definition of probability is taken in an epistemological sense, this was not always so. Cournot, who scrupulously distinguishes objective and subjective probabilities, says that the objective probability "may be considered as measuring the *possibility* of the event, or the ease with which it may be produced". He makes clear that this has nothing to do with our state of knowledge [12, p. 438].

We require a detailed textual examination of at least four generations of theorists to sort these matters out. Before proceeding it is instructive to fix one distinction in mind. It is the distinction between: (*i*) physical, *de re*, possibility; with the *de re*, or physical, interpretation of probability; and (*ii*) epistemic, *de dicto*, possibility; with the *de dicto*, or epistemological, interpretation of probability. Equipossibly theories of probability are intelligible only when one understands the interplay between (*i*) and (*ii*).

The "proclivity" theory

Following Popper's [34] many students speak of a propensity theory of probability, by which they mean the theory that physical probabilities are to be conceived in terms of a Peircian dispositional property rather than in terms of limits in a Misian collective. Whatever name one likes to give it, this is the idea most current in the early days of probability, which I tend to call the doctrine of chances. One of the words used of the elements of the Fundamental Probability Set of equal alternatives was "*aequae proclives*". Proclivity is a virtual synonym for propensity: had Popper sought for historical antecedents, he might have called his opinion a proclivity theory.

The Latin word has to do with the ease with which something can be produced, and is explicitly a word to do with tendencies or propensities. The other root word used by early authors is the Latin *facile*. Such usage is discernible even in the very first textbook, Cardano's mid-sixteenth century [10]. It is present in Galileo's brief memorandum on our topic [15]. It is used in Huygen's textbook of 1657 [21]. It persists at least until the 1730s, when Daniel Bernouilli uses it in his famous study of utility ([4] p. 175). His translator reads *proclives* as our English "probable". In most contexts such a translation would be amazing, but here it is grist for my mill. Such authors had a proclivity, or propensity, interpretation of probability in mind. There is more to the doctrine of chances than this, but it requires an extended essay. The present brief observations suffice for our purpose. We need only add that in the course of time, proclivity is gradually mixed with possibility, but still in the *de re*, physical sense of "possible". Then the physical sense of "possible" is supplemented by the epistemic sense.

Thus, by the end of the eighteenth century, Laplace, defining probability by equipossibility, could seem to be following an old tradition. In fact he is canonising a recent transition. Thanks to the duality of possibility, he did it inconspicuously. Moreover, for all his professed subjectivism, Laplace very often had physical probabilities in mind, but he can speak of these equivocally in his epistemological framework. Indeed, when he has a specifically objective, physical, probability in mind, we shall see he often calls it not a probability but a possibility, just because he wishes to avoid his usual equivocation. No harm is done, for one is concerned with isomorphic concepts, and Laplace is interested in mathematics, not philosophical niceties.

Jacques Bernoulli

As von Mises noted, Jacques Bernouilli does use the terminology of equipossibility. But this is altogether rare. His *Ars conjectandi* [5], which begins by paraphrasing Huygens' text of 1657, starts with the traditional terminology of cases that can be made with equal ease. We read *aequè facilè*, in proposition 1, and proposition 3 has *casus aequè in proclivi*.

Such standard terminology is preserved until the celebrated Part IV, which both proves the first limit theorem of probability mathematics, and also publishes the epistemological interpretation of probability. As shown in [16] Bernoulli gradually reverts back to at least partial use of the physical interpretation. It is notable that while he is in his first full flight of treating probability as "degree of certainty", there is no mention of equipossible cases. When we do come to use the phrase, "equally possible", it occurs rarely. Page 219 is the first instance of a noteworthy explanation of what it means. "All cases are equally possible, that is to say, each can come about as easily as any other" (*omnes casus aequè possibiles esse, seu pari facilitate evenire posse*). The discussion immediately following is couched in terms of ease of obtaining certain outcomes.

Shortly after this several examples are compared. First come dice. Here the cases are called *aequè proclives*. Next, balls in an urn, all of which are said to be equally possible. In explanation, Bernoulli says all are equally possible because there is no reason why one should be drawn rather than any other. The third example concerns mortality statistics. Here, then, is where equipossibility is made public. I think that no well circulated work before *Ars conjectandi* contains the equipossibility idea. But there is at least one place where Bernouilli himself certainly read it, namely in a letter from Leibniz, of 1703.

Leibniz

Leibniz had been telling Bernoulli about de Witt's 1671 book on annuities. [7] has a full account of the background for this correspondence. In December 1703, Leibniz says that, so far as he can recall, de Witt follows the usual procedure of computing according to "equally possible cases" [28, 3/1, p. 84].

Despite the 1703 letter, mortality statistics do not provide a source for equipossibility. We must turn to a more metaphysical moment. Leibniz wrote a memorandum on probability in September 1678. It is headed, *De incerti aestimation* [6, for an abridged version, see 29, pp. 569–71]. Here is the first attempt to provide an axiomatic foundation for probability theory. Here also is the first statement of the principle of indifference. Leibniz says such a principle can be "proved by metaphysics", presumably as a methodological application of the principle of sufficient reason. Here also we find the remarkable statement, that *probabilitas est gradus possibilitas*.

It is not at first clear what Leibniz meant. At a later date, he became unequivocally attached to an epistemological interpretation, and conceived a programme very much along Carnap's lines [19]. It was to be a new sort of logic, which would enable one to determine probabilities *ex datis*. Probabilities were objective relations of hypotheses to the data, and were to be computed as part of the universal characteristic. If probabilities were degrees of anything, it was not possibility, but certainty, an idea that Jacques Bernouilli in due course popularized.

In 1678 Leibniz had not yet formed these grand schemes. Possibility is linked to the ease with which one can achieve something: Here too we find a seeming equivalence between equal possibility and equal facility: "*aequè facile seu aequè possibile.*" So probability as degree of possibility seems to be the old-fashioned degree of facility or proclivity. Nor did he give this up. Thus he wrote to Bourget on 22 March 1714 that,

> The art of conjecture is founded on what is more or less easy (the French word *facile*) or, to put it better, more or less feasible (*faisable*), for the Latin *facilis* derives from *faciendo*, which is an exact translation of feasible [27, 3, p. 569].

He goes on to describe cases of what he calls *a priori* ways of working out how feasible or makeable an outcome is, using the example of dice. But Bernoulli's correspondence and the now published *Ars conjectandi* have persuaded him that *a posteriori* determinations may be needed. He continues:

One may still estimate likelihoods (vraisemblances) *a posteriori*, by experience, to which one must have recourse in default of *a priori* reasons. For example, it is equally likely that a child should be born a boy or a girl, because the number of boys and girls is very nearly equal all over the world. One can say that what happens more or less often is more or less feasible in the present state of things, putting together all considerations that must concur in the production of a fact [ibid. p. 570].

For once, Leibniz has not kept up with the literature, for Arbuthnot had already published his proof that regularly more boys are born than girls [1].

Further connections between the relevant concepts are briefly set out in the definitions Leibniz proposed for his universal characteristic [26,6/2. p. 496]. We learn that what is "*facile* is what is very possible, that is to say, for which little is required" in order to bring it into being. And then, *Quod facile est in re, id probabile est in mente.* One can put these scraps together into an admirable sketch of a probability theory as follows.

Leibniz has come to associate the word "probability" with the epistemological interpretation, and, as he makes plain elsewhere, it is a logical relation. It is a *de dicto* modality, or, in what some would find an improvement on Boudet's description, it is *in mente.* But there is also a physical interpretation, about facility *in re.* Such facility, feasibility, or proclivity cannot always be determined *a priori*, but must sometimes be found out *a posteriori*, from observations of how often different alternatives occur. Bernouilli's law of large numbers is evidently in Leibniz's mind at this point. As for possibility, thanks to the stated connection with facility, it is *de re.* Moreover, since *facile est valde possibile*, it does not matter whether we say "equally possible" or "equally facile" for although facile $\neq$ possible, equally facile $=$ equally possible. Moreover, if we want to know the inductive probability of a in the light of some observed frequency data b, we first estimate the facility (i.e. physical probability) of events of kind a and obtain an estimate of a fraction about *de re* propensity. Then in virtue of the rule, *facile in re* $=$ *probabile in mente*, we obtain an epistemological probability of a in the light of the data b.

Thomas Bayes

The opening salvo of the great part v of *Ars conjectandi* says that probability is degree of certainty. Thus Bernoulli made public what Leibniz had been telling correspondents for some time. The epistemological interpretation of probability had been declared. It gradually came to dominate French thought on the topic.

Work in English, which includes the fundamental contributions of de Moivre, Simpson, and Bayes, did not follow suit. This is not for insular lack of interest in epistemology. Quite the contrary. De Moivre, ever cautious about statistical inference from data to a distribution of chances, calls this the "hardest problem that can be proposed on the subject of chance" [31, p. 242]. Hume, seemingly well versed in the doctrine of chances, is at more pains than any other philosopher for a century and half, to see how chances can help induction, and concludes in the negative. And perhaps the most powerful statement ever, of the potential relations between probability and induction, was made by Richard Price.

Price is now better remembered as a moralist, but he also prepared insurance tables based on data about the city of Norwich, which were the standard for a century. Introducing Bayes' celebrated essay to the Royal Society, he writes of inferring from statistical data to a distribution of chances:

> Every judicious person will be sensible that the problem now mentioned is by no means merely a curious speculation in the doctrine of chances, but necessary to be solved in order to assure foundation for all our reasonings concerning past facts, and what is likely to be hereafter. Common sense is indeed sufficient to show us that, from the observation of what has in former instances been the consequence of a certain cause or action, one may make a judgment what is likely to be the consequence another time, and that the larger number of experiments we have to support a conclusion, so much the more reason we have to take it for granted. But it is certain that we cannot determine, at least not to any nicety, in what degree repeated experiments confirm a conclusion, without the particular discussion of the beforementioned problem; which, therefore, is necessary to be considered by any one who would give a clear account of the strength of *analogical* or *inductive* reasoning [3, pp. 371 f.].

It is a fact that Bayes did not publish his investigations. His logic was too impeccable. The bulk of his study concerns a probability model in which the physical interpretation is natural, perhaps even mandatory. "Suppose the square table or plane $ABCD$ to be so made and levelled" that there is a uniform probability distribution for places on which a ball thrown on the table can settle. This is not merely an epistemological uniformity, but a physical one.

Imagine that a ball is tossed on the table. Then a further ball is tossed repeatedly. The event M occurs if this second ball is to the right of the first. Bayes asks, what is the probability that the first ball falls in a given interval, conditional on the event M occurring p times and falling q

times? The answer is a special case of what we call Bayes' theorem in the continuum. Notice, however, that there is a completely viable physical interpretation of Bayes' result. Expressing it in terms of long run frequency, he computes the relative frequency that the first ball falls in a given interval, among experiments in which the event M occurs $p/p+q$ times.

Bayes next says—and this surely indicates that he is distinguishing physics from epistemology—that the situation with the table is isomorphic to other cases in which "we absolutely know nothing antecedently to any trials made concerning it", for, "concerning such an event I have no reason to think that, in a certain number of trials, it should happen any one possible number of times than another".

Not only did Bayes not publish, but also, Bayes' reasoning did not catch on in England, whereas it was quickly picked up in France, not only by Laplace, but also, for example, by Condorcet [11, pp. lxxxiii, and 176 ff.]. Laplace admires Bayes' ingenuity, but, having fewer philosophical scruples than Bayes, thinks the presentation *un peu embarrasée* [25, 7, p. cxlviii].

Why did Bayes' argument seem unexceptionable in France, and yet win no immediate supporters in Britain? Because in France, one explained probability in terms of possibility. Possibility is itself equivocal between *de re* and *de dicto*. By equivocating there was no manifest gap in Bayes' final argument. But in England one still worked on what Bayes himself called the doctrine of chances. The equipossibility definition was not used. There was no verbal way to paper over the gap. Bayes had to put in a scholium. Two centuries later we are still in no agreement on whether the reasoning of the scholium is correct. In France, thanks to the equipossibility tradition, Bayes' scruples were mystifying, and one could get on with the "probability of causes" in a mathematically productive way.

D'Alembert's riddle

The equipossibility account had become sufficiently standard in France that in a volume dated 1765, Diderot's *Enclopédie* offers it as the definition of mathematical probabilities. The unsigned article *Probabilité* is very judicious, and considers a large number of kinds of probability—the probability of witnesses and the like but the only numerate probability is that founded on "the equal possibility of several events", and which covers gaming and annuities. The author continues,

> I have said that this principle is to be employed when we suppose the several cases to be equally possible. and in effect it is only a supposition relative to our bounded knowledge, that we say, for example, that all the points on the die can occur equally [13, XIIII, p. 396].

Thus an epistemological understanding of probability is invited, and the equally possible cases are no longer *de re*, but *de dicto* or *in mente*. Oddly the same volume, under *Possibilité*, lists only what it calls the "metaphysical sense" of being free from contradiction [p. 169]. This would give sheer nonsense if applied to the probability article.

D'Alembert, Diderot's collaborator, was perhaps the greatest of sceptics about probability mathematics, and in the same set of volumes he challenges the whole structure. His thoughts on the matter are scattered about: Todhunter provides a good running guide and source of references [36, ch. 13].

Any epistemological set of equally possible cases must, in the end, rely on the principle of indifference. D'Alembert produced the first persistent challenge to this principle. He has had an undeservedly bad press. Peter and Paul play with a coin. Peter wins if the coin falls heads on the first or second toss. Otherwise he loses. To bolster D'Alembert's case, let us throw the coin into a furnace as soon as the game is over, so that if Peter wins at the first toss, there cannot be a second toss. Then there are just three possible "simple" outcomes. Is not 2/3 the probability of Peter winning?

Were d'Alembert writing for the old proclivity theory or the English doctrine of chances, his answer would have to be: No, the probability of Peter winning is 3/4. For in such a theory it would be taken for granted as a physical property of the coin, that on the first toss, H can be made as easily as T; likewise in the second toss, if made. The multiplication laws of the doctrine of chances give us 3/4.

Even on a purely epistemological interpretation one can dispute the figure 2/3. If this is a fair coin, then the physics give us more reason to expect that Peter will win, than Paul, and so d'Alembert's three cases are not equally possible *de dicto*. D'Alembert in the end concedes this. But he does not grant that there is anything sacrosanct about the figure 3/4. He is inclined to say—how seriously one is not quite sure—that the probability of Peter's winning is an incommensurable between 2/3 and 3/4, and he suspects it is closer to 2/3 than 3/4.

D'Alembert is not as foolish as he has seemed. Remember that probability is supposed to be defined in terms of equally possible cases, and these are supposed to be cases such that we are equally undetermined about their occurrence or not. Given that we are not equally undetermined about d'Alembert's three cases, what, in a purely epistemological theory, entitles us to a probability fraction of exactly 3/4? Answers can be given. Indeed one can answer along the lines of my reconstruction of Leibniz, given above in my final paragraph on that philosopher. But I am inclined to say that d'Alembert was never adequately answered. Laplace did indeed present the calculation that leads to 3/4 [25, 7, p. xii]. But his answer may seem to beg the question. Of course d'Alembert knew about that calculation. His question is: given the kind of foundation that Laplace professes, why *that* calculation?

Some hesitant distinctions

Laplace's first great papers appeared in 1774. By this time, two spheres of interest had arisen. On the one hand there was the theory of errors, in which probability theory aims at a best estimate from discordant measurements. One's focus of attention is the set of the objective physical characteristics of the measuring device, as revealed by a distribution of error. On the other hand, there are recurring dilettante proposals for assessing the credibility of witnesses. The theory of errors begins with probability in its physical interpretation, while the theory of witnesses falls under the epistemological interpretation. These diverging interests made more plain that two concepts of probability were at stake.

In his seminal contributions to error theory, Lambert at first uses some established terminology. Thus in 1760 he is saying that when equal positive and negative errors are possible, they will occur equally frequently [25, p. 131]. But he soon evolves a whole new terminology. He invents a word for the epistemic concept of probability: *Zuverlässigkeit* [23]. He needs this because on the one hand he is concerned with what he calls the true value of the quantity under observation. He relies on a frequency distribution of error. He uses that in turn to infer the "reliability" of the estimate of the true value.

One finds a similar tendency in Lagrange. *Facilité* is perhaps his favoured word for the objective, physical interpretation. He is concerned with the facility of errors. But from this he wants to infer what he calls the probability that a true value lies in a given interval. *Probabilité*, far from being a synonym for *facilité*, has become an antonym!

Perhaps characteristically it is the lesser minds of the period that take the conceptual matters seriously instead of ploughing on with the mathematics. William Emerson furnishes a curious example. He distinguishes mathematical probability from some more general idea of probability. Of the former he gives a good frequentist account:

> Although it is impossible to determine with certainty how an event shall happen yet it may be determined mathematical, what is the likelihood or degree of probability there is for its happening or failing; and that is all that is intended by a calculation, except that there be made an infinite number of repetitions, and then one with another will always bring it to the same thing as the calculation makes it. [14, p. 1].

For this "calculation", "it is supposed that all chances are equal, or made with equal facility" (p. 3)— But then,

The probability or improbability of an event is the judgment we form of it by comparing the number of chances there are for its happening, with the number of chances for its failing.

Thus Emerson, though operating in the standard English doctrine of chances that speaks only of a physical interpretation, is groping for the idea of probability as "judgment" or credibility.

I think Condorcet is the first to render this groping explicit. In work published in 1785, we have the purely mathematical sense (he calls it) in which probability is defined in terms of "equally possible combinations" [11, p. v.]. This is illustrated by a die such that each face "*puisse arriver également*".

Condorcet asserts that the "mathematical probabilities" computed thus are merely definitional equivalances to the equipossible distributions with which one started. (One is reminded of the recent criticism by Ayer and others of Carnap's inductive logic: if probability statements are related to the data as logically necessary truths, how then can they serve as a guide to action?). There is, says Condorcet, a "more extended sense" of probability. He does not so much go on to define a new sense of probability, as to introduce a new concept, the *motif de croire*. He argues that the grounds for belief are in proportion to mathematical probabilities derived from equipossible cases. Thus in Condorcet's opaque discussion, probability, in the strict sense, appears to have to do with the physical interpretation, whereas there is to be a more general sense, best called "grounds for belief", which fits the epistemological interpretation.

Condorcet's description of statistical inference is confused, but in a rather engaging way. He is less concerned with inferring the probability of statistical hypotheses, than with guessing what will come next. Thus if we do not know the probability distribution for a chance set-up, but do have some results of trials on it, what is the probability of getting outcome S on the next trial? Condorcet's analysis is Bayesian. He says that we conclude not with the "true probability" of S, but with the "mean probability" [p. lxxxvi].

In a subject prone to conceptual difficulty, I have encountered no phrase less felicitous than "*probabilité moyenne*". The "mean" must be suggested by our starting with a uniform prior distribution, and then averaging. Evidently the "true probability" of which Condorcet speaks is an objective, physical, unknown, while the mean probability is an epistemological measure of credibility, of *motif de croire*. Elsewhere, Condorcet abandons his attempt to give us distinctions, and in Bayesian analysis speaks regularly of the "probability of a probability" (p. 180). Laplace is much better when, in the same context, he speaks without fussing of the "probability of a possibility".

Laplace

It would be impertinent to analyse the work of Laplace in my few remaining pages. We can only briefly note that although he canonised the epistemological terminology of "equally possible cases", that is not a dominant feature of his early work. Indeed in his first paper, of 1774, he says innocently that probability is defined in terms of a ratio among cases, so long as the cases are equally probable! [25, **8**, p. 10]. In the third paper, of 1776, this becomes, "if we see no reason why one case should happen more than the other" [p. 146]. The word "*possibilité*" does not occur in the definition although it does occur soon after (e.g. p. 149) to denote something like physical probability.

A simple problem well illustrates Laplace's thought at this time. Suppose we have a biased coin, but no information about the direction of bias. Then in one toss *H* and *T* are equally credible. But in two tosses the four outcomes are not equally credible. If (unknown to us) the bias is for *H*, then *HH* is the most probable outcome; if the bias is for *T*, *TT* is the most probable. In our ignorance, *HH* and *TT* are more credible than *HT* and *TH*. Laplace was obviously very pleased with this observation, for he makes it repeatedly, and boasts that no one ever thought of it before.

I said that *H* and *T* are equally credible, while *HH* is more credible than *HT*. Laplace says of *H* and *T*:

> One regards two events as equally probable when one can see no reason that would make one more probable than the other, because, even though there is an unequal possibility between them, we know not which way, and this uncertainty makes us look on each as if it were as probable as the other. (p. 61).

Notice that in a careful statement like this, Laplace does not say that *H* and *T* *are* equally probable, but that we *regard* then as equally probable. The word "possibility" is kept to indicate that so far as physics is concerned, there is an objective difference between *H* and *T*. Laplace has not yet become confidently subjective. In contrast, let us turn to the more familiar, polished, Laplace whom all of us have read.

The opening prose of Laplace's philosophical essay on probability is almost as captivating as the mathematics of Book II of the *Théorie Analytique* it served to introduce. Laplace's demon has become the byword for a physically determinate system. Because the world is determined, Laplace implies, there can be no probabilities in things. Probability fractions arise from our knowledge and from our ignorance. The theory of chances then,

consists in reducing all events of the same kind to a certain number of equally possible cases, that is to say, those such that we are equally undecided about their existence [25, 7, p. viii].

On the first page of Book II, we have,

One has seen in the Introduction that the probability of an event is the ratio of the number of cases that are favourable to it, to the number of possible cases, when there is nothing to make us believe that one case should occur rather than any other, so that these cases are, for us, equally possible [p. 181].

In the introduction "equally possible" is glossed in an epistemological way, and the Book II turn of phrase, "for us, equally possible", is consistent with that gloss.

Whenever Laplace is doing direct probabilities—deductions from probability distributions to other probability distributions—he can happily continue with the epistemological interpretation. Or, one may add, any other interpretation, or no interpretation. His results could nowadays be presented quite formally, as pure mathematics, and the interpretation is irrelevant.

So all goes smoothly until Book VI, on the probability of causes. Here we return to the matter of the 1774 papers. One is inferring from observed data to an unknown probability distribution. It is a distribution of causes, and that is conceived as a matter of physics. One wants to find out the true distribution. And what do we read? As more and more experimental data build up concerning simple events, then, he tells us

their true possibility is known better and better [p. 370].

True possibility! We are concerned, he says with *discovering* an as yet unknown degree of possibility. Throughout this chapter he is altogether consistent. He speaks of the probability that the possibility of an event lies in a given interval. This language occurs even in the introduction; even indeed, in the brief allusion to Bayes I mentioned above.

It seems that "probability of a possibility" occurs only when Laplace is trying to assess what we now call the inductive or epistemological or subjective probability of what we now call an objective statistical hypothesis. In these circumstances, possibility is *de re*, and is a physical characteristic of the set-up under investigation.

Thus Laplace himself is equivocal. When he needs a word to refer to an unknown physical characteristic, he picks on "possibility", using it in the old, *de re* sense. This was the language of his early papers. When he wants to emphasise the epistemological concept which finally captivated him, he uses "possibility" in what he makes clear is the *de dicto*, epistemological sense. But even in those introductory chapters, the *de dicto* equally possible cases are ones which we know to be equal because we think of them as being *de re* equally possible, that is, equal in physical characteristics. D'Alembert's riddle reminds us that we have to invoke such an identification if we are to avoid contradiction. However, if we speak ambiguously, the questions which piqued D'Alembert and the more important issues that vexed Bayes, are not noticed. They did not seriously arise for Laplace, or he and his successors would never have got us so far. Where they have got us to, is another question.

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REFERENCES

[1] ARBUTHNOT, JOHN. An argument for divine providence taken from the constant regularity of the births of both sexes. *Philosophical Transactions of the Royal Society*, **29**, 1710, pp. 186–90.

[2] ARNAULD, ANTOINE and NICOLE, PIERRE. *La Logique, ou l'Art de Penser*. Amsterdam, 1662 etc. Translated by James Dickoff and Patricia James as Arnauld. *The Art of Thinking*. New York, 1964.

[3] BAYES, THOMAS. 'An essay towards solving a problem in the doctrine of chances', communicated by Richard Price. *Philosophical Transactions of the Royal Society*, **53**, 1763, pp. 370–418. Reprinted in E. S. Pearson and M. G. Kendall, *Studies in the History of Statistics and Probability*. London, 1970.

[4] BERNOULLI, DANIEL. Specimen Theoriae Novae de Mensura Sortis. *Commentarii Acadamiae Scientiarum Imperialis Petropolitanae*, **5**, for 1730–1, published 1738. Translated as Exposition of a new theory on the measurement of risk. *Econometrica*, **22**, 1954, pp. 23–36.

[5] BERNOULLI, JACQUES. *Ars conjectandi*. Basel, 1713; for other editions see [16], note 1.

[6] BIERMANN, KURT-R. and FALK, MARGOT. G. W. Leibnitz *De incerti aestimatione*. *Forschungen und Fortschritte*, **31**, 1957, pp. 45–49.

[7] BIERMANN, KURT-R. and FALK, MARGOT. Berechnung der Sterbewahrscheinlichkeit bei J. de Witt. Ibid. **33**, 1959, pp. 168–73.

[8] BOREL, EMILE. *Éléments de la Théorie des Probabilités*, Paris, 1909. There is a translation of Borel's 1950 version by J. E. Freund, *Elements of the Theory of Probability*. Englewood Cliffs, N. J., 1965.

[9] BOUDET, P. M. Probabilité et logique de l'argumentation selon Jacques Bernoulli. *Les Études Philosophiques*, 1967, pp. 265–88.

[10] CARDANO, GERALMO. *Liber de ludo aleae*, in *Opera Omnia*, Lyons, 1663, vol. VII. Circulated maybe a century before publication. Translated by G. Gould in Oystein Ore's *Cardano, the Gambling Scholar*. Princeton, 1953; New York, 1965, pp. 183–241.

[11] CONDORCET, JAN ANTIONE NICOLA CARITAT MARQUIS DE. *Essai sur l'application de l'analyse à la probabilité des décisions rendues à la pluralité des voix*. Paris, 1785.

[12] COURNOT, A. A. *Expositions de la Théorie des Chances et des Probabilités*. Paris, 1843.

[13] DIDEROT, DENIS. *Encyclopédie, ou Dictionaire raisonne*. Paris, 1751–7.

[14] EMERSON, WILLIAM. The laws of chance. *Miscellanies*, London, 1776.

[15] GALILEO GALILEI. 'Sopra le Scoperte dei dadi', in *Opere*, Florence, 1898, **8**, pp. 591–4. Translated by E. H. Thorne in F. N. David, *Games, Gods, and Gambling*, London, 1967, pp. 192–5.

[16] HACKING, IAN. Bernoulli's Art of Conjecturing. *British Journal for the Philosophy of Science*, **22**, pp. 202–29, 1971.

[17] HACKING, IAN. The Logic of Pascal's Wager. *American Philosophical Quarterly*, early 1972.
[18] HACKING, IAN. Possibility. *Philosophical Review*, **76**, 1967, pp. 143–68, and a sequel, 'All kinds of possibility', not yet published.
[19] HACKING, IAN. The Leibniz-Carnap program for inductive logic. *Journal of Philosophy*, late 1971.
[20] HACKING, IAN. Slightly more realistic personal probability. *Philosophy of Science*, **34**, 1967, pp. 311–24.
[21] HUYGENS, CHRISTIAN. *De ratiociniis in ludo aleae*, first published by Francis Schooten in his *Exercitationum Mathematicarum*, Book V, 517–34. Amsterdam, 1657. The 1660 Dutch edition, and a French translation of it, are in the *Oeuvres*, The Hague, 1920, **14**, pp. 48–179.
[22] LAGRANGE, JOSEPH LOUIS. 'Memoire sur l'utilité de la méthode de prendre le milieu entre les resultats de plusiers observations'. In the *Mélanges* of the Societé Royale de Turin. 1770–3, published as vol. 5 of *Miscellanea Taurinensa*.
[23] LAMBERT, JOHANN HEINRICH. *Beytrage zum Gebrauche der Mathematik und deren Anwendung*, 1/B, 1765. This includes the *Theorie der Zuverlässigkeit der Beobachtungen und Versuche*.
[24] LAMBERT, JOHANN HEINRICH. *Photometria*, Angustae Vindelicorum, 1760.
[25] LAPLACE, PIERRE SIMON MARQUIS DE. *Oeuvres*. Volume 7 has the *Théorie Analytique des Probabilités*, 1812, 1814, 1820. The third edition has an *Introduction*, the *Essaie Philosophique sur les Probabilités*, which has been translated by F. W. Truscott and F. L. Emery as *A Philosophical Essay on Probabilities*, New York, 1951. Volumes 8 and 9 have the earlier papers on probability, commencing in 1774. For an overview of their titles and the relation of their contents to *Théorie Analytique*, see Todhunter [36].
[26] LEIBNIZ, G. W. *Sämtliche Schriften und Briefe*.
[27] LEIBNIZ, G. W. *Die Philosophischen Schriften*, ed. C. I. Gerhardt.
[28] LEIBNIZ, G. W. *Mathematische Schriften*, ed. C. I. Gerhardt.
[29] LEIBNIZ, G. W. *Opuscules et Fragments Inedits*, ed. Louis Couturat, Paris, 1903; Hildesheim, 1966.
[30] MISES, R. VON, *Wahrscheinlichkeit, Statistik und Wahrheit*. Vienna, 1928, 1951. Translated by Hilda Geiringer as *Probability, Statistics and Truth*. London, 1939, 1957. Page references are to the 2nd German edition. Von Mises rendered Laplace's *également possible* by *gleichmöglich*, but the English translation regrettably turns this into "equally likely".
[31] MOIVRE, ABRAHAM DE. *The Doctrine of Chances*. London, 1718, 1738, 1756: reference to the third edition, reprinted New York, 1967.
[32] PASCAL, BLAISE. 'Infini-rien' in the *Pensées*; no. 233 in Brunschvicg's numeration and no. 418 in that of Lafuma.
[33] POISSON, SIMON DENIS. *Recherches sur la Probabilités des Jugements*. Paris, 1837.
[34] POPPER, K. R. 'The propensity theory of probability', in S. Körner, *Observation and Interpretation*, London, 1957, pp. 65 ff.
[35] REICHENBACH, HANS. *The Theory of Probability*. Berkeley and Los Angeles, 1949. Translation of *Wahrscheinlichkeitslehre*. Leiden, 1935.
[36] TODHUNTER, ISAAC. *A History of the Mathematical Theory of Probability*. London, 1865; New York, 1949.
[37] WITT, JOHANN DE. *Waardye van Lyf-renten naer proportie van Losrenten*. Amsterdam, 1671. Translated by Frederick Hendriks in *The Assurance Magazine*, **2**, 1852, pp. 232–49.