CHAPTER III

THE MEASUREMENT OF PROBABILITIES

1. I HAVE spoken of probability as being concerned with *degrees* of rational belief. This phrase implies that it is in some sense quantitative and perhaps capable of measurement. The theory of probable arguments must be much occupied, therefore, with *comparisons* of the respective weights which attach to different arguments. With this question we will now concern ourselves.

It has been assumed hitherto as a matter of course that probability is, in the full and literal sense of the word, measurable. I shall have to limit, not extend, the popular doctrine. But, keeping my own theories in the background for the moment, I will begin by discussing some existing opinions on the subject.

2. It has been sometimes supposed that a numerical comparison between the degrees of any pair of probabilities is not only conceivable but is actually within our power. Bentham, for instance, in his *Rationale of Judicial Evidence*,[1] proposed a scale on which witnesses might mark the degree of their certainty; and others have suggested seriously a 'barometer of probability.'[2]

That such comparison is *theoretically possible*, whether or not we are actually competent in every case to make the comparison, has been the generally accepted opinion. The following quotation[3] puts this point of view very well:

[1]Book i. chap vi. (referred to by Venn).

[2]The reader may be reminded of Gibbon's proposal that:—"A Theological Barometer might be formed, of which the Cardinal (Baronius) and our countryman, Dr. Middleton, should constitute the opposite and remote extremities, as the former sunk to the lowest degree of credulity, which was compatible with learning, and the latter rose to the highest pitch of scepticism, in any wise consistent with Religion."

[3]W. F. Donkin, *Phil. Mag.*, 1851. He is replying to an article by J. D. Forbes (*Phil. Mag.*, Aug. 1849) which had cast doubt upon this opinion.

"I do not see on what ground it can be doubted that every definite state of belief concerning a proposed hypothesis is in itself capable of being represented by a numerical expression, however difficult or impracticable it may be to ascertain its actual value. It would be very difficult to estimate in numbers the *vis viva* of all of the particles of a human body at any instant; but no one doubts that it is capable of numerical expression. I mention this because I am not sure that Professor Forbes has distinguished the difficulty of *ascertaining numbers* in certain cases from a supposed difficulty of *expression by means of numbers*. The former difficulty is real, but merely relative to our knowledge and skill; the latter, if real, would be absolute and inherent in the subject-matter, which I conceive is not the case."

De Morgan held the same opinion on the ground that, wherever we have differences of degree, numerical comparison *must* be theoretically possible.[1] He assumes, that is to say, that all probabilities can be placed in an *order* of magnitude, and argues from this that they must be measurable. Philosophers, however, who are mathematicians, would no longer agree that, even if the premiss is sound, the conclusion follows from it. Objects can be arranged in an order, which we can reasonably call one of degree or magnitude, without its being possible to conceive a system of measurement of the differences between the individuals.

This opinion may also have been held by others, if not by De Morgan, in part because of the narrow associations which Probability has had for them. The Calculus of Probability has received far more attention than its logic, and mathematicians, under no compulsion to deal with the whole of the subject, have naturally confined their attention to those special cases, the existence of which

[1]"Whenever the terms greater and less can be applied, there twice, thrice, etc., can be conceived, though not perhaps measured by us."—"Theory of Probabilities," *Encyclopaedia Metropolitana*, p. 395. He is a little more guarded in his *Formal Logic*, pp. 174, 175; but arrives at the same conclusion so far as probability is concerned.

will be demonstrated at a later stage, where algebraical representation is possible. Probability has become associated, therefore, in the minds of theorists with those problems in which we are presented with a number of exclusive and exhaustive alternatives of equal probability; and the principles, which are readily applicable in such circumstances, have been supposed, without much further enquiry, to possess general validity.

3. It is also the case that theories of probability have been propounded and widely accepted, according to which its numerical character is necessarily involved in the definition. It is often said, for instance, that probability is the ratio of the number of "favourable cases" to the total number of "cases." If this definition is accurate, it follows that every probability can be properly represented by a number and in fact *is* a number; for a ratio is not a quantity at all. In the case also of definitions based upon statistical frequency, there must be by definition a numerical ratio corresponding to every probability. These definitions and the theories based on them will be discussed in Chapter VIII.; they are connected with fundamental differences of opinion with which it is not necessary to burden the present argument.

4. If we pass from the opinions of theorists to the experience of practical men, it might perhaps be held that a presumption in favour of the numerical valuation of all probabilities can be based on the practice of underwriters and the willingness of Lloyd's to insure against practically any risk. Underwriters are actually willing, it might be urged, to name a numerical measure in every case, and to back their opinion with money. But this practice shows no more than that many probabilities are greater or less than some numerical measure, not that they themselves are numerically definite. It is sufficient for the underwriter if the premium he names *exceeds* the probable risk. But, apart from this, I doubt whether in extreme cases the process of thought, through which he goes before naming a premium, is wholly rational and determinate; or that two equally

intelligent brokers acting on the same evidence would always arrive at the same result. In the case, for instance, of insurances effected before a Budget, the figures quoted must be partly arbitrary. There is in them an element of caprice, and the broker's state of mind, when he quotes a figure, is like a bookmaker's when he names odds. Whilst he may be able to make sure of a profit, on the principles of the bookmaker, yet the individual figures that make up the book are, within certain limits, arbitrary. He may be almost certain, that is to say, that there will not be new taxes on more than one of the articles tea, sugar, and whisky; there may be an opinion abroad, reasonable or unreasonable, that the likelihood is in the order—whisky, tea, sugar; and he may, therefore be able to effect insurances for equal amounts in each at 30 per cent, 40 per cent, and 45 per cent. He has thus made sure of a profit of 15 per cent, however absurd and arbitrary his quotations may be. It is not necessary for the success of underwriting on these lines that the probabilities of these new taxes are really measurable by the figures $\frac{3}{10}$, $\frac{4}{10}$, and $\frac{45}{100}$; it is sufficient that there should be merchants willing to insure at these rates. These merchants, moreover, may be wise to insure even if the quotations are partly arbitrary; for they may run the risk of insolvency unless their possible loss is thus limited. That the transaction is in principle one of bookmaking is shown by the fact that, if there is a specially large demand for insurance against one of the possibilities, the rate rises;—the probability has not changed, but the "book" is in danger of being upset. A Presidential election in the United States supplies a more precise example. On August 23, 1912, 60 per cent was quoted at Lloyd's to pay a total loss should Dr. Woodrow Wilson be elected, 30 per cent should Mr. Taft be elected, and 20 per cent should Mr. Roosevelt be elected. A broker, who could effect insurances in equal amounts against the election of each candidate, would be certain at these rates of a profit of 10 per cent. Subsequent modifications of these terms would largely depend upon the number of applicants for each kind of policy. Is it possible to maintain that these figures in any way represent reasoned numerical estimates of probability?

In some insurances the arbitrary element seems even greater. Consider, for instance, the reinsurance rates for the *Waratah*, a vessel which disappeared in South African waters. The lapse of time made rates rise; the departure of ships in search of her made them fall; some nameless wreckage is found and they rise; it is remembered that in similar circumstances thirty years ago a vessel floated, helpless but not seriously damaged, for two months, and they fall. Can it be pretended that the figures which were quoted from day to day—75 per cent, 83 per cent, 78 per cent—were rationally determinate, or that the actual figure was not within wide limits arbitrary and due to the caprice of individuals? In fact underwriters themselves distinguish between risks which are properly insurable, either because their probability can be estimated between comparatively narrow numerical limits or because it is possible to make a "book" which covers all possibilities, and other risks which cannot be dealt with in this way and which cannot form the basis of a regular business of insurance,—although an occasional gamble may be indulged in. I believe, therefore, that the practice of underwriters weakens rather than supports the contention that all probabilities can be measured and estimated numerically.

5. Another set of practical men, the lawyers, have been more subtle in this matter than the philosophers.[1] A distinction, interesting for our present purpose, between probabilities, which can be estimated within somewhat narrow limits, and those which cannot, has arisen in a series of judicial decisions respecting damages. The following extract[2] from the *Times Law Reports* seems to me to deal

[1]Leibniz notes the subtle distinctions made by Jurisconsults between degrees of probability; and in the preface to a work, projected but unfinished, which was to have been entitled *Ad stateram juris de gradibus probationum et probabilitatum* he recommends them as models of logic in contingent questions (Couturat, *Logique de Leibniz*, p. 240).

[2]I have considerably compressed the original report (Sapwell *v.* Bass).

very clearly in a mixture of popular and legal phraseology, with the logical point at issue:

This was an action brought by a breeder of racehorses to recover damages for breach of a contract. The contract was that Cyllene, a racehorse owned by the defendant, should in the season of the year 1909 serve one of the plaintiff's brood mares. In the summer of 1908 the defendant, without the consent of the plaintiff, sold Cyllene for £30,000 to go to South America. The plaintiff claimed a sum equal to the average profit he had made through having a mare served by Cyllene during the past four years. During those four years he had had four colts which had sold at £3300. Upon that basis his loss came to 700 guineas.

Mr. Justice Jelf said that he was desirous, if he properly could, to find some mode of legally making the defendant compensate the plaintiff; but the question of damages presented formidable and, to his mind, insuperable difficulties. The damages, if any, recoverable here must be either the estimated loss of profit or else nominal damages. The estimate could only be based on a succession of contingencies. Thus it was assumed that (*inter alia*) Cyllene would be alive and well at the time of the intended service; that the mare sent would be well bred and not barren; that she would not slip her foal; and that the foal would be born alive and healthy. In a case of this kind he could only rely on the weighing of chances; and the law generally regarded damages which depended on the weighing of chances as too remote, and therefore irrecoverable. It was drawing the line between an estimate of damage based on probabilities, as in "Simpson *v.* L. and N.W. Railway Co." (1, Q.B.D., 274), where Cockburn, C.J., said: "To some extent, no doubt, the damage must be a matter of speculation, but that is no reason for not awarding any damages at all," and a claim for damages of a totally problematical character. He (Mr. Justice Jelf) thought the present case was well over the line. Having referred to "Mayne on Damages" (8th ed., p. 70), he pointed out that in "Watson *v.* Ambergah Railway Co." (15, Jur., 448)

Patteson, J., seemed to think that the chance of a prize might be taken into account in estimating the damages for breach of a contract to send a machine for loading barges by railway too late for a show; but Erle, J., appeared to think such damage was too remote. In his Lordship's view the chance of winning a prize was not of sufficiently ascertainable value at the time the contract was made to be within the contemplation of the parties. Further, in the present case, the contingencies were far more numerous and uncertain. He would enter judgment for the plaintiff for nominal damages, which were all he was entitled to. They would be assessed at 1s.

One other similar case may be quoted in further elucidation of the same point, and because it also illustrates another point—the importance of making clear the assumptions relative to which the probability is calculated. This case[1] arose out of an offer of a Beauty Prize[2] by the *Daily Express*. Out of 6000 photographs submitted, a number were to be selected and published in the newspaper in the following manner:

The United Kingdom was to be divided into districts and the photographs of the selected candidates living in each district were to be submitted to the readers of the paper in the district, who were to select by their votes those whom they considered the most beautiful, and a Mr. Seymour Hicks was then to make an appointment with the 50 ladies obtaining the greatest number of votes and himself select 12 of them. The plaintiff, who came out head of one of the districts, submitted that she had not been given a reasonable opportunity of keeping an appointment, that she had thereby lost the value of her chance of one of the 12 prizes, and claimed damages accordingly. The jury found that the defendant had not taken reasonable means to give the plaintiff an opportunity of presenting herself for selection, and assessed the damages, provided they were

[1]Chaplin *v.* Hicks (1911).

[2]The prize was to be a theatrical engagement and, according to the article, the probability of subsequent marriage into the peerage.

capable of assessment, at £100, the question of the possibility of assessment being postponed. This was argued before Mr. Justice Pickford, and subsequently in the Court of Appeal before Lord Justices Vaughan Williams, Fletcher Moulton, and Harwell. Two questions arose—relative to what evidence ought the probability to be calculated, and was it numerically measurable? Counsel for the defendant contended that, "if the value of the plaintiff's chance was to be considered, it must be the value as it stood at the beginning of the competition, not as it stood after she had been selected as one of the 50. As 6000 photographs had been sent in, and there was also the personal taste of the defendant as final arbiter to be considered, the value of the chance of success was really incalculable." The first contention that she ought to be considered as one of 6000 not as one of 50 was plainly preposterous and did not hoodwink the court. But the other point, the personal taste of the arbiter, presented more difficulty. In estimating the chance, ought the Court to receive and take account of evidence respecting the arbiter's preferences in types of beauty? Mr. Justice Pickford, without illuminating the question, held that the damages were capable of estimation. Lord Justice Vaughan Williams in giving judgment in the Court of Appeal argued as follows:

As he understood it, there were some 50 competitors, and there were 12 prizes of equal value, so that the average chance of success was about one in four. It was then said that the questions which might arise in the minds of the persons who had to give the decisions were so numerous that it was impossible to apply the doctrine of averages. He did not agree. Then it was said that if precision and certainty were impossible in any case it would be right to describe the damages as unassessable. He agreed that there might be damages so unassessable that the doctrine of averages was not possible of application because the figures necessary to be applied were not forthcoming. Several cases were to be found in the reports where it had been so held, but he denied the proposition that because precision and certainty had not been arrived at, the jury had no function or duty to determine the damages.... He (the Lord Justice) denied that the mere fact that you could not assess with precision and certainty relieved a wrongdoer from paying damages for his breach of duty. He would not lay down that in every case it could be left to the jury to assess the damages; there were cases where the loss was so dependent on the mere unrestricted volition of another person that it was impossible to arrive at any assessable loss from the breach. It was true that there was no market here; the right to compete was personal and could not be transferred. He could not admit that a competitor who found herself one of 50 could have gone into the market and sold her right to compete. At the same time the jury might reasonably have asked themselves the question whether, if there was a right to compete, it could have been transferred, and at what price. Under these circumstances he thought the matter was one for the jury.

The attitude of the Lord Justice is clear. The plaintiff had evidently suffered damage, and justice required that she should be compensated. But it was equally evident, that, relative to the completest information available and account being taken of the arbiter's personal taste, the probability could be by no means estimated with numerical precision. Further, it was impossible to say how much weight ought to be attached to the fact that the plaintiff had been *head* of her district (there were *fewer* than 50 districts); yet it was plain that it made her chance *better* than the chances of those of the 50 left in, who were not head of their districts. Let rough justice be done, therefore. Let the case be simplified by ignoring some part of the evidence. The "doctrine of averages" is then applicable, or, in other words, the plaintiff's loss may be assessed at twelve-fiftieths of the value of the prize.[1]

6. How does the matter stand, then? Whether or not

[1]The jury in assessing the damages at £100, however, cannot have argued so subtly as this; for the average value of a prize (I have omitted the details bearing on their value) could not have been fairly estimated so high as £400.

such a thing is theoretically conceivable, no exercise of the practical judgment is possible, by which a numerical value can actually be given to the probability of every argument. So far from our being able to measure them, it is not even clear that we are always able to place them in an order of magnitude. Nor has any theoretical rule for their evaluation ever been suggested.

The doubt, in view of these facts, whether any two probabilities are in every case even theoretically capable of comparison in terms of numbers, has not, however, received serious consideration. There seems to me to be exceedingly strong reasons for entertaining the doubt. Let us examine a few more instances.

7. Consider an induction or a generalisation. It is usually held that each additional instance increases the generalisation's probability. A conclusion, which is based on three experiments in which the unessential conditions are varied, is more trustworthy than if it were based on two. But what reason or principle can be adduced for attributing a numerical measure to the increase?[1]

Or, to take another class of instances, we may sometimes have some reason for supposing that one object belongs to a certain category if it has points of similarity to other known members of the category (*e.g.* if we are considering whether a certain picture should be ascribed to a certain painter), and the greater the similarity the greater the probability of our conclusion. But we cannot in these cases *measure* the increase; we can say that the presence of certain

[1]It is true that Laplace and others (even amongst contemporary writers) have believed that the probability of an induction is measurable by means of a formula known as the *rule of succession*, according to which the probability of an induction based on n instances is $\frac{n+1}{n+2}$. Those who have been convinced by the reasoning employed to establish this rule must be asked to postpone judgment until it has been examined in Chapter XXX. But we may point out here the absurdity of supposing that the odds are 2 to 1 in favour of a generalisation based on a single instance—a conclusion which this formula would seem to justify.

peculiar marks in a picture increases the probability that the artist of whom those marks are known to be characteristic painted it, but we cannot say that the presence of these marks makes it two or three or any other number of times more probable than it would have been without them. We can say that one thing is more like a second object than it is like a third; but there will very seldom be any meaning in saying that it is twice as like. Probability is, so far as measurement is concerned, closely analogous to similarity.[1]

Or consider the ordinary circumstances of life. We are out for a walk—what is the probability that we shall reach home alive? Has this always a numerical measure? If a thunderstorm bursts upon us, the probability is less than it was before; but is it changed by some definite numerical amount? There might, of course, be data which would make these probabilities numerically comparable; it might be argued that a knowledge of the statistics of death by lightning would make such a comparison possible. But if such information is not included within the knowledge to which the probability is referred, this fact is not relevant to the probability actually in question and cannot affect its value. In some cases, moreover, where general statistics are available, the numerical probability which might be derived from them is inapplicable because of the presence of additional knowledge with regard to the particular case. Gibbon calculated his prospects

[1]There are very few writers on probability who have explicitly admitted that probabilities, though in some sense quantitative, may be incapable of numerical comparison. Edgeworth, "Philosophy of Chance" (*Mind*, 1884, p. 225), admitted that "there may well be important quantitative, although not numerical, estimates" of probabilities. Goldschmidt (*Wahrscheinlichkeitsrechnung*, p. 43) may also be cited as holding a somewhat similar opinion. He maintains that a lack of comparability in the grounds often stands in the way of the measurability of the probable in ordinary usage, and that there are not necessarily good reasons for measuring the value of one argument against that of another. On the other hand, a numerical statement for the degree of the probable, although generally impossible, is not in itself contradictory to the notion; and of three statements, relating to the same circumstances, we can well say that one is more probable than another, and that one is the most probable of the three.

of life from the volumes of vital statistics and the calculations of actuaries. But if a doctor had been called to his assistance the nice precision of these calculations would have become useless; Gibbon's prospects would have been better or worse than before, but he would no longer have been able to calculate to within a day or week the period for which he then possessed an even chance of survival.

In these instances we can, perhaps, arrange the probabilities in an order of magnitude and assert that the new datum strengthens or weakens the argument, although there is no basis for an estimate *how much* stronger or weaker the new argument is than the old. But in another class of instances is it even possible to arrange the probabilities in an *order* of magnitude, or to say that one is the greater and the other less?

8. Consider three sets of experiments, each directed towards establishing a generalisation. The first set is more numerous; in the second set the irrelevant conditions have been more carefully varied; in the third case the generalisation in view is wider in scope than in the others. Which of these generalisations is on such evidence the most probable? There is, surely, no answer; there is neither equality nor inequality between them. We cannot always weigh the analogy against the induction, or the scope of the generalisation against the bulk of the evidence in support of it. If we have *more* grounds than before, comparison is possible; but, if the grounds in the two cases are quite different, even a comparison of more and less, let alone numerical measurement, may be impossible.

This leads up to a contention, which I have heard supported, that, although not all measurements and not all comparisons of probability are within our power, yet we can say in the case of every argument whether it is *more* or *less* likely than not. Is our expectation of rain, when we start out for a walk, always *more* likely than not, or *less* likely than not, or *as* likely as not? I am prepared to argue that on some occasions *none* of these alternatives hold, and that it will be an arbitrary matter to decide for or against the umbrella. If the barometer is high, but the clouds are black, it is not always rational that one should prevail over the other in our minds, or even that we should balance them,—though it will be rational to allow caprice to determine us and to waste no time on the debate.

9. Some cases, therefore, there certainly are in which no rational basis has been discovered for numerical comparison. It is not the case here that the method of calculation, prescribed by theory, is beyond our powers or too laborious for actual application. *No* method of calculation, however impracticable, has been suggested. Nor have we any *prima facie* indications of the existence of a common unit to which the magnitudes of all probabilities are naturally referrible. A degree of probability is not composed of some homogeneous material, and is not apparently divisible into parts of like character with one another. An assertion, that the magnitude of a given probability is in a numerical ratio to the magnitude of every other, seems, therefore, unless it is based on one of the current *definitions* of probability, with which I shall deal separately in later chapters, to be altogether devoid of the kind of support, which can usually be supplied in the case of quantities of which the mensurability is not open to denial. It will be worth while, however, to pursue the argument a little further.

10. There appear to be four alternatives. Either in some cases there is no probability at all; or probabilities do not all belong to a single set of magnitudes measurable in terms of a common unit; or these measures always exist, but in many cases are, and *must remain*, unknown; or probabilities do belong to such a set and their measures are *capable* of being determined by us, although we are not always able so to determine them in practice.

11. Laplace and his followers excluded the first two alternatives. They argued that every conclusion has its place in the numerical range of probabilities from 0 to 1, *if only we knew it*, and they developed their theory of *unknown* probabilities.

In dealing with this contention, we must be clear as to what we mean by saying that a probability is *unknown*. Do we mean unknown through lack of skill in arguing from given evidence, or unknown through lack of evidence? The first is alone admissible, for new evidence would give us a new probability, not a fuller knowledge of the old one; we have not discovered the probability of a statement on given evidence, by determining its probability in relation to quite different evidence. We must not allow the theory of unknown probabilities to gain plausibility from the second sense. A relation of probability does not yield us, as a rule, information of much value, unless it invests the conclusion with a probability which lies between narrow numerical limits. In ordinary practice, therefore, we do not always regard ourselves as *knowing* the probability of a conclusion, unless we can estimate it numerically. We are apt, that is to say, to restrict the use of the expression *probable* to these numerical examples, and to allege in other cases that the probability is unknown. We might say, for example, that we do not know, when we go on a railway journey, the probability of death in a railway accident, unless we are told the statistics of accidents in former years; or that we do not know our chances in a lottery, unless we are told the number of the tickets. But it must be clear upon reflection that if we use the term in this sense,—which is no doubt a perfectly legitimate sense,—we ought to say that in the case of some arguments a relation of probability does not exist, and not that it is unknown. For it is not *this* probability that we have discovered, when the accession of new evidence makes it possible to frame a numerical estimate.

Possibly this theory of unknown probabilities may also gain strength from our practice of estimating arguments, which, as I maintain, have *no* numerical value, by reference to those that have. We frame two ideal arguments, that is to say, in which the general character of the evidence largely resembles what is actually within our knowledge, but which is so constituted as to yield a numerical value, and we judge that the probability of the actual argument lies between these two. Since our standards, therefore, are referred to numerical measures in many cases where actual measurement is impossible, and since the probability lies *between* two numerical measures, we come to believe that it must also, if only we knew it, possess such a measure itself.

12. To say, then, that a probability is unknown ought to mean that it is unknown to us through our lack of skill in arguing from given evidence. The evidence justifies a certain degree of knowledge, but the weakness of our reasoning power prevents our knowing what this degree is. At the best, in such cases, we only know *vaguely* with what degree of probability the premisses invest the conclusion. That probabilities can be unknown in this sense or known with less distinctness than the argument justifies, is clearly the case. We can through stupidity fail to make any estimate of a probability at all, just as we may through the same cause estimate a probability wrongly. As soon as we distinguish between the degree of belief which it is rational to entertain and the degree of belief actually entertained, we have in effect admitted that the true probability is *not* known to everybody.

But this admission must not be allowed to carry us too far. Probability is, *vide* Chapter II. (§ 12), relative in a sense to the principles of *human* reason. The degree of probability, which it is rational for *us* to entertain, does not presume perfect logical insight, and is relative in part to the secondary propositions which we in fact know; and it is not dependent upon whether more perfect logical insight is or is not conceivable. It is the degree of probability to which those logical processes lead, of which our minds are capable; or, in the language of Chapter II., which those secondary propositions justify, which we in fact know. If we do not take this view of probability, if we do not limit it in this way and make it, to this extent, relative to human powers, we are altogether adrift in the unknown; for we cannot ever know what degree of probability would be justified by the perception of logical relations which we are, and must always be, incapable of comprehending.

13. Those who have maintained that, where we cannot assign

a numerical probability, this is not because there is none, but simply because we do not know it, have really meant, I feel sure, that with some addition to our knowledge a numerical value would be assignable, that is to say that our conclusions would have a numerical probability relative to *slightly different* premisses. Unless, therefore, the reader clings to the opinion that, in every one of the instances I have cited in the earlier paragraphs of this chapter, it is theoretically possible on *that* evidence to assign a numerical value to the probability, we are left with the first two of the alternatives of § 10, which were as follows: either in some cases there is no probability at all; or probabilities do not all belong to a single set of magnitudes measurable in terms of a common unit. It would be difficult to maintain that there is *no* logical relation whatever between our premiss and our conclusion in those cases where we cannot assign a numerical value to the probability; and if this is so, it is really a question of whether the logical relation has characteristics, other than mensurability, of a kind to justify us in calling it a probability-relation. Which of the two we favour is, therefore, partly a matter of definition. We might, that is to say, pick out from probabilities (in the widest sense) a set, if there is one, all of which are measurable in terms of a common unit, and call the members of this set, and them only, probabilities (in the narrow sense). To restrict the term 'probability' in this way would be, I think, very inconvenient. For it is possible, as I shall show, to find *several* sets, the members of each of which are measurable in terms of a unit common to all the members of that set; so that it would be in some degree arbitrary[1] which we chose. Further, the distinction between probabilities, which would be thus measurable and those which would not, is not fundamental.

At any rate I aim here at dealing with probability in its widest sense, and am averse to confining its scope to a limited type of argument. If the opinion that not all probabilities can be measured

[1]Not altogether; for it would be natural to select the set to which the relation of certainty belongs.

seems paradoxical, it may be due to this divergence from a usage which the reader may expect. Common usage, even if it involves, as a rule, a flavour of numerical measurement, does not *consistently* exclude those probabilities which are incapable of it. The confused attempts, which have been made, to deal with numerically indeterminate probabilities under the title of unknown probabilities, show how difficult it is to confine the discussion within the intended limits, if the original definition is too narrow.

14. I maintain, then, in what follows, that there are some pairs of probabilities between the members of which *no* comparison of magnitude is possible; that we can say, nevertheless, of some pairs of relations of probability that the one is greater and the other less, although it is not possible to measure the difference between them; and that in a very special type of case, to be dealt with later, a meaning can be given to a *numerical* comparison of magnitude. I think that the results of observation, of which examples have been given earlier in this chapter, are consistent with this account.

By saying that not all probabilities are measurable, I mean that it is not possible to say of every pair of conclusions, about which we have some knowledge, that the degree of our rational belief in one bears any numerical relation to the degree of our rational belief in the other; and by saying that not all probabilities are comparable in respect of more and less, I mean that it is not always possible to say that the degree of our rational belief in one conclusion is either equal to, greater than, or less than the degree of our belief in another.

We must now examine a philosophical theory of the quantitative properties of probability, which would explain and justify the conclusions, which reflection discovers, if the preceding discussion is correct, in the practice of ordinary argument. We must bear in mind that our theory must apply to all probabilities and not to a limited class only, and that, as we do not adopt a definition of probability which presupposes its numerical mensurability, we cannot directly argue from differences in degree to a numerical measurement of these

differences. The problem is subtle and difficult, and the following solution is, therefore, proposed with hesitation; but I am strongly convinced that something resembling the conclusion here set forth is true.

15. The so-called magnitudes or degrees of knowledge or probability, in virtue of which one is greater and another less, really arise out of an *order* in which it is possible to place them. Certainty, impossibility, and a probability, which has an intermediate value, for example, constitute an ordered series in which the probability lies *between* certainty and impossibility. In the same way there may exist a second probability which lies *between* certainty and the first probability. When, therefore, we say that one probability is greater than another, this precisely means that the degree of our rational belief in the first case lies *between* certainty and the degree of our rational belief in the second case.

On this theory it is easy to see why comparisons of more and less are not always possible. They exist between two probabilities, only when they and certainty all lie on the same ordered series. But if more than one distinct series of probabilities exist, then it is clear that only those, which belong to the *same* series, can be compared. If the attribute ‘greater’ as applied to one of two terms arises solely out of the relative order of the terms in a series, then comparisons of greater and less must always be possible between terms which are members of the same series, and can never be possible between two terms which are not members of the same series. Some probabilities are not comparable in respect of more and less, because there exists more than one path, so to speak, between proof and disproof, between certainty and impossibility; and neither of two probabilities, which lie on independent paths, bears to the other and to certainty the relation of ‘between’ which is necessary for quantitative comparison.

If we are comparing the probabilities of two arguments, where the conclusion is the same in both and the evidence of one exceeds the evidence of the other by the inclusion of some fact which is favourably relevant, in such a case a relation seems clearly to exist between the two in virtue of which one lies *nearer* to certainty than the other. Several types of argument can be instanced in which the existence of such a relation is equally apparent. But we cannot assume its presence in every case or in comparing in respect of more and less the probabilities of every pair of arguments.

16. Analogous instances are by no means rare, in which, by a convenient looseness, the phraseology of quantity is misapplied in the same manner as in the case of probability. The simplest example is that of colour. When we describe the colour of one object as bluer than that of another, or say that it has more green in it, we do not mean that there are quantities blue and green of which the object's colour possesses more or less; we mean that the colour has a certain position in an order of colours and that it is nearer some standard colour than is the colour with which we compare it.

Another example is afforded by the cardinal numbers. We say that the number three is greater than the number two, but we do not mean that these numbers are quantities one of which possesses a greater magnitude than the other. The one is greater than the other by reason of its position in the order of numbers; it is further distant from the origin zero. One number is greater than another if the second number lies *between* zero and the first.

But the closest analogy is that of similarity. When we say of three objects A, B, and C that B is more like A than C is, we mean, not that there is any respect in which B is in itself quantitatively greater than C, but that, if the three objects are placed in an order of similarity, B is nearer to A than C is. There are also, as in the case of probability, *different* orders of similarity. For instance, a book bound in blue morocco is more like a book bound in red morocco than if it were bound in blue calf; and a book bound in red calf is more like the book in red morocco than if it were in blue calf. But there may be no comparison between the degree of similarity which exists between books bound in red morocco and blue morocco, and

that which exists between books bound in red morocco and red calf. This illustration deserves special attention, as the analogy between orders of similarity and probability is so great that its apprehension will greatly assist that of the ideas I wish to convey. We say that one argument is more probable than another (*i.e.* nearer to certainty) in the same kind of way as we can describe one object as more like than another to a standard object of comparison.

17. Nothing has been said up to this point which bears on the question whether probabilities are ever capable of *numerical* comparison. It is true of some types of ordered series that there are measurable relations of distance between their members as well as order, and that the relation of one of its members to an 'origin' can be numerically compared with the relation of another member to the same origin. But the legitimacy of such comparisons must be matter for special enquiry in each case.

It will not be possible to explain in detail how and in what sense a meaning can sometimes be given to the numerical measurement of probabilities until Part II. is reached. But this chapter will be more complete if I indicate briefly the conclusions at which we shall arrive later. It will be shown that a process of compounding probabilities can be defined with such properties that it can be conveniently called a process of *addition*. It will sometimes be the case, therefore, that we can say that one probability C is equal to the *sum* of two other probabilities A and B, *i.e.* $C = A + B$. If in such a case A and B are equal, then we may write this $C = 2A$ and say that C is double A. Similarly if $D = C + A$, we may write $D = 3A$, and so on. We can attach a meaning, therefore, to the equation $P = n \,.\, A$, where P and A are relations of probability, and n is a number. The relation of certainty has been commonly taken as the unit of such conventional measurements. Hence if P represents certainty, we should say, in ordinary language, that the magnitude of the probability A is $\frac{1}{n}$. It will be shown also that we can define a process, applicable to probabilities, which has the properties of arithmetical multiplication. Where numerical measurement is possible, we can in consequence perform algebraical operations of considerable complexity. The attention, out of proportion to their real importance, which has been paid, on account of the opportunities of mathematical manipulation which they afford, to the limited class of numerical probabilities, seems to be a part explanation of the belief, which it is the principal object of this chapter to prove erroneous, that *all* probabilities must belong to it.

18. We must look, then, at the quantitative characteristics of probability in the following way. Some sets of probabilities we can place in an ordered series, in which we can say of any pair that one is nearer than the other to certainty,—that the argument in one case is nearer proof than in the other, and that there is more reason for one conclusion than for the other. But we can only build up these ordered series in special cases. If we are given two distinct arguments, there is no general presumption that their two probabilities and certainty can be placed in an order. The burden of establishing the existence of such an order lies on us in each separate case. An endeavour will be made later to explain in a systematic way how and in what circumstances such orders can be established. The argument for the theory here proposed will then be strengthened. For the present it has been shown to be agreeable to common sense to suppose that an order exists in some cases and not in others.

19. Some of the principal properties of ordered series of probabilities are as follows:

(i.) Every probability lies on a path between impossibility and certainty; it is always true to say of a degree of probability, which is not identical either with impossibility or with certainty, that it lies *between* them. Thus certainty, impossibility and *any* other degree of probability form an ordered series. This is the same thing as to say that every argument amounts to proof, or disproof, or occupies an intermediate position.

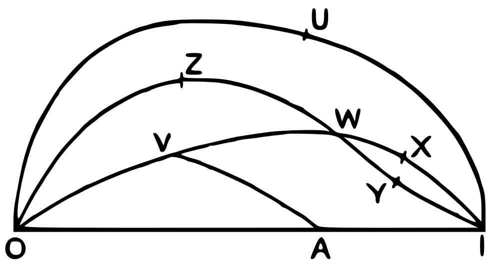
(ii.) A path or series, composed of degrees of probability, is not in general compact. It is not necessarily true, that is to say, that any pair of probabilities in the same series have a probability between them.

(iii.) The same degree of probability can lie on more than one path (*i.e.* can belong to more than one series). Hence, if B lies between A and C, and also lies between A′ and C′ it does not follow that of A and A′ either lies between the other and certainty. The fact, that the same probability can belong to more than one distinct series, has its analogy in the case of similarity.

(iv.) If ABC forms an ordered series, B lying between A and C, and BCD forms an ordered series, C lying between B and D, then ABCD forms an ordered series, B lying between A and D.

20. The different series of probabilities and their mutual relations can be most easily pictured by means of a diagram. Let us represent an ordered series by points lying upon a path, all the points on a given path belonging to the same series. It follows from (i.) that the points O and I, representing the relations of impossibility and certainty, lie on every path, and that all paths lie wholly between these points. It follows from (iii.) that the same point can lie on more than one path. It is possible, therefore, for paths to intersect and cross. It follows from (iv.) that the probability represented by a given point is greater than that represented by any other point which can be reached by passing along a path with a motion constantly towards the point of impossibility, and less than that represented by any point which can be reached by moving along a path towards the point of certainty. As there are independent paths there will be some pairs of points representing relations of probability such that we cannot reach one by moving from the other along a path always in the same direction.

These properties are illustrated in the annexed diagram, O represents impossibility, I certainty, and A a numerically measurable probability intermediate between O and I; U, V, W, X, Y, Z are non-numerical probabilities, of which, however, V is less than the numerical probability A, and is also less than W, X, and Y. X and Y are both greater than W, and greater than V, but are not comparable with one another, or with A. V and Z are both less than W, X, and Y, but are not comparable with one another; U is not quantitatively comparable with any of the probabilities V, W, X, Y, Z. Probabilities which are numerically comparable will all belong to one series, and the path of this series, which we may call the numerical path or strand, will be represented by OAI.



21. The chief results which have been reached so far are collected together below, and expressed with precision:—

(i.) There are amongst degrees of probability or rational belief various sets, each set composing an ordered series. These series are ordered by virtue of a relation of 'between.' If B is 'between' A and C, ABC form a series.

(ii.) There are two degrees of probability O and I *between* which *all* other probabilities lie. If, that is to say, A is a probability, OAI form a series. O represents impossibility and I certainty.

(iii.) If A lies between O and B, we may write this $\widehat{\mathrm{AB}}$, so that $\widehat{\mathrm{OA}}$ and $\widehat{\mathrm{AI}}$ are true for all probabilities.

(iv.) If $\widehat{\mathrm{AB}}$, the probability B is said to be greater than the probability A, and this can be expressed by $\mathrm{B} > \mathrm{A}$.

(v.) If the conclusion a bears the relation of probability P to the premiss h, or if, in other words, the hypothesis h invests the conclusion a with probability P, this may be

written $a\mathrm{P}h$. It may also be written $a/h = \mathrm{P}$.

This latter expression, which proves to be the more useful of the two for most purposes, is of fundamental importance. If $a\mathrm{P}h$ and $a'\mathrm{P}h'$, *i.e.* if the probability of a relative to h is the same as the probability of a' relative to h', this may be written $a/h = a'/h'$. The value of the symbol a/h, which represents what is called by other writers 'the probability of a,' lies in the fact that it contains explicit reference to the *data* to which the probability relates the conclusion, and avoids the numerous errors which have arisen out of the omission of this reference.