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THE CONCEPT OF PROBABILITY

BY R. D. CLARKE, F.I.A.
of the Prudential Assurance Company Ltd.

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INTRODUCTION

IT is commonplace to remark that probability lies at the root of actuarial science. It is equally commonplace that there has been—and still is—considerable dispute over the definition of probability and that such dispute is usually of little consequence in practical calculations. This being so, some may wonder why I have chosen to ventilate once again the philosophical problems which encompass the concept of probability and which commonly bedevil discussions of what is meant by propositions of the form 'The probability of x is y' or 'A is more probable than B'. The solutions which may be offered to this type of problem are not likely to affect the amount of a surrender value or the contribution scale of a pension scheme. On occasion they may affect the answers to estimation problems or decisions based upon significance tests, although even here the differences between the results yielded by alternative methods of approach are commonly negligible.

Nevertheless, although the actuary can efficiently carry on his professional practice without concerning himself with the philosophy of probability, the spirit of speculative inquiry is alive in most of us, and it would betray a lack of imaginative vigour if we did not from time to time pause in our affairs to reflect on fundamentals. Accordingly, no apology is made for submitting to this Institute a paper which has little pretension to practical utility.

2. To give this paper a homely setting, I will begin by remarking that I have always been uneasily conscious of a blatant dualism in the traditional actuarial approach to the chances of death and survival. If a healthy man presents himself for life assurance, we look up his probability of dying in a table which has been constructed from observed frequency ratios. But if a proposer suffers from some physical impairment, we ask the Medical Officer for a personal opinion on the degree of extra risk involved. Thus we are continuously using both the 'frequency' and the 'degree of belief' approaches to probability in our everyday work.

Faced with this situation I have asked myself the question: 'Is the duality of approach real or apparent?' And I have gone on to ask: 'Are the respective concepts of probability as a frequency ratio and as a degree of belief so incompatible with each other as some have assumed?' The present paper is the fruit of an endeavour to answer these questions.

PROBABILITY AS DEGREE OF BELIEF

3. The assessment of an extra risk by a Medical Officer, although it involves a large element of personal judgment, is not entirely uninfluenced by observed statistical frequencies. In the background of every doctor's experience is some information, however rudimentary, that groups of people

exhibiting certain types of impairment have been subject to high rates of mortality. Let us, therefore, consider a different type of situation. Suppose that a historian is discussing a hypothesis which cannot be finally established from the available evidence, e.g. the hypothesis that the Etruscans migrated to Italy by sea from Asia Minor. In attaching a probability judgment to a hypothesis of this kind, the historian is unlikely to get much assistance from statistical frequencies. Although the Etruscan nation may be regarded as a member of a class formed out of the civilized nations of history, the proportionate frequency of such nations known to have originated in Asia Minor (if any) would matter little beside the evidence of language, culture, racial characteristics, etc. The information on which the historian arrives at his final judgment consists of a set of unique facts which do not lend themselves to statistical classification.

4. Some writers assert that no meaning can be attached to the term 'probability' when applied to a historical hypothesis. They maintain that historical hypotheses are either true or false; or if the available information is insufficient to establish truth or falsehood, the hypothesis is simply unverified. Similarly, they deny any meaning to the statement 'the probability that k is the millionth digit in the expansion of π is p'. Theoretically it is possible to compute π to a million places of decimals. If this were done the millionth digit would become known; but until it is done the millionth digit is unknown and any reference to probability is considered irrelevant.

It is difficult to see the reasoning behind this argument. The number of primes less than 10^{12} is not known. Yet by substituting $x = 10^{12}$ in the formula

$$f(x) = \int_2^x \frac{dt}{\log t},$$

we can obtain a close approximation on the basis of which a probability statement can be made. Although a particular fact may be unknown, there is usually some information available which places us in a position intermediate between certain knowledge and total ignorance.

Suppose that an electronic computer has been given the task of calculating π to an indefinitely large number of decimal places. Suppose also that the digits appear successively on a screen as they are calculated. Let there be a bookmaker taking bets on each digit before it is recorded. Does anyone doubt that if the bookmaker offers 9 to 1 against a particular digit appearing on the screen in any specified position, he should in the long run break even? And does not the fraction $\frac{1}{10}$ represent our degree of belief in all propositions of the form: 'The 47,213th digit in the expansion of π is 6'?

In reasoning thus we are, of course, making use of the information that exhaustive analyses of the sequence of digits in the expansion of π hitherto calculated have revealed no bias or pattern of any kind. The sequence is, in fact, indistinguishable from a set of random numbers.

5. Let us now return to our historical example concerning the origin of the Etruscans. Sometimes a distinction is drawn between past events, which can only be in doubt because of inadequate records, and future events, which are in doubt because they have not yet happened. But this distinction is illusory. To the spectators watching the screen upon which the electronic computer is recording the successive digits in the expansion of π, the appearance of each digit is a future event until it has actually occurred. Similarly, it is conceivable

that at some future date historical or archaeological records will be discovered which will finally settle the question of the origin of the Etruscans. But at the present time that discovery is a future event and any probability judgment is a measure of our expectation concerning it.

As a further illustration, let us suppose that on a certain Wednesday a person is contemplating the probability that a particular football team will win its match on the following Saturday. He than receives an urgent summons calling him abroad and no news reaches him from England for over a week. On the following Wednesday, the match having meanwhile been played, he is again assessing the probability that the team has won its match. But although the tense of the verb has now altered, the probability is the same as before because the available information is unchanged.

It is true, of course, that in this last illustration the state of mind of the person in question may change during the week which elapses between his first and second judgments. He may feel inclined to take an optimistic view on one occasion and a pessimistic view on the other. Similarly, a doctor may take different views of two proposers whose personal conditions may be identical; or two doctors may make different assessments of the same person. Thus, differences may arise from variations of mood, temperament or inclination; or there may be variations in the weights attached to the several components in a complex set of information.

6. It is no purpose of this paper to discuss questions of psychology, and without more ado it is proposed to assume that we are concerned with rational beings of sufficient intelligence to keep their judgment clear of emotional or temperamental variations. Thus the only subjective differences which will be recognized are those which arise from the attribution of different relative weights to the various factors likely to affect the probability of a specified event.

Although informed persons frequently differ from one another in their judgments of particular instances, there are usually broad areas of agreement among them. This situation has a parallel in the sphere of value judgments. There may be legitimate differences of opinion as to whether Rembrandt is a greater or lesser painter than Titian; but no critic questions that either is superior to Pontormo or Terborch. In discussing the faculty of judgment we are no longer in a medium of precise thought with clearly defined outlines. It is in this vague and rather blurred territory, where there are broad areas of agreement among rational beings with fringes of uncertainty where individual opinion may differ, that probability has its roots.

It is here contended that for a given event and a given set of information there exists a corresponding probability. There may be various estimates of that probability made by different persons or by the same person at different times; but a philosophy of probability has perforce to ignore these differences that exist between individual judgments in particular situations. The fact that broad areas of agreement exist makes it possible to postulate a consensus of informed opinion as a basis for setting up the concept of a 'rational degree of belief'. It is its basis in a consensus of minds, rather than in individual minds, that lifts the 'degree of belief' concept above the level of subjectivism.

THE INFORMATION COMPONENT

7. It is elementary that a probability cannot have meaning except in terms of the information on which it is based. Symbolically, $P(X)$ is meaningless; it is always necessary to write $P(X \mid I)$, i.e. the probability of event X on information I. Thus $P(X \mid I_1)$, $P(X \mid I_2)$ and $P(X \mid I_3)$ all represent the probability of event X; but they are different probabilities, since each is based on a different set of information. I_1, I_2 and I_3 may be mutually exclusive or they may contain common elements; but the amount of information to be employed in any probability judgment will depend in the first place upon what is available, and in the second place upon the decision of the investigator as to how much of the available information he will take into account.

8. Suppose it is desired to estimate the probability that rain will fall in a certain locality upon a given date. If meteorological records are available for the past hundred years, it will be possible to ascertain in how many of these years there was in fact a fall of rain on the date in question. Thus if there were 27 such years, the required probability might be assessed at ·27.

If, however, the given date is, say, three days from the present time, it would be feasible to observe the prevailing weather conditions and to search the records for past occasions when similar conditions have existed. The proportion of such occasions which were followed three days later by rain could then be ascertained and could be made the basis of a new probability statement which might well be widely different from the former statement.

There is no incompatibility between these statements, for they are based upon two different sets of information. Later on, when frequency probabilities are discussed, this will be expressed in another way by saying that the event in question has been treated as a member of two distinct sets. Yet it is of interest to note that, so long as any possibility of selection can be ignored, an insurer could do business in policies covering weather risks by adopting either line of approach for calculating his premiums. For a given risk the premium charged would differ according to the method employed; but in the long run the insurer should break even by either method. (Of course, the long-term average breaks down if persons effecting insurances only do so in seasons when weather conditions appear to be bad. Options against the insurer are too familiar a subject to need discussion here; but their existence is the reason why in voluntary insurance it is essential not to neglect any piece of relevant information in forming probability judgments.)

9. A given set of information, I, will consist of a number of separate statements. Some of these statements will represent the results of observations having a direct bearing upon the particular event of which the probability is being discussed. Others will be general propositions governing a whole class of events, of which the particular event in question is merely one instance. The distinction between these two types of statement is relevant, because it is related to the distinction between prior probability and likelihood familiar in Bayes's theorem and in problems of inverse probability.

In the example quoted earlier, i.e. the probability that the Etruscans migrated to Italy from Asia Minor at a comparatively advanced stage of culture, the evidence of archaeological remains falls into the category of observed data. Statements about such matters as the development of sea routes or the practicability of large-scale migrations of semi-civilized communities are general propositions relevant not only to the Etruscan question, but to other historical questions of like nature. It is upon these general propositions that we decide whether a particular hypothesis is plausible and deserving of further investigation. In more familiar language, they determine the prior probability.

10. Some reflexion shows that the dividing line between information determining prior probabilities and information constituting observed data is not clearly defined. If from a study of Hittite archaeology it is discovered that an exodus from Asia Minor occurred at a period consistent with the foundation of the Etruscan settlements, does this affect the 'prior probability' of the hypothesis stated above or is it additional evidence to be added to the discoveries of Etruscan archaeology? The answer to this question is far from self-evident; in fact, it becomes difficult to sustain any rigid division of the relevant information involved in a probability relationship between two watertight categories labelled 'general' and 'particular'.

PRIOR PROBABILITIES

11. At this point it will be helpful to pay attention to some elementary problems in inverse probability. Consider the following example which formerly used to be found in text-books:

A bag contains three balls of unknown colour. One is drawn and found to be white. What is the probability that all three balls are white?

To solve this, Bayes's theorem is needed. Initially there are four possible hypotheses, namely:

H_0: none of the balls is white.
H_1: one ball, and only one, is white.
H_2: two balls, and only two, are white.
H_3: all three balls are white.

It is necessary to have some preliminary information (A, say) on which to base the prior probabilities $P(H_0 \mid A)$, etc. The likelihoods for the observed event, X (i.e. the drawing of a white ball), are, of course,

$$P(X \mid H_0A)=0, \quad P(X \mid H_1A)=\tfrac{1}{3}, \quad P(X \mid H_2A)=\tfrac{2}{3}, \quad P(X \mid H_3A)=1.$$

Thus the required probability will be

$$P(H_3 \mid A, X)=\{P(H_3 \mid A).P(X \mid H_3, A)\}\Big/\sum_{i=0}^{3}\{P(H_i \mid A).P(X \mid H_i, A)\}$$

$$=P(H_3 \mid A)/\{\tfrac{1}{3}.P(H_1 \mid A)+\tfrac{2}{3}.P(H_2 \mid A)+P(H_3 \mid A)\}.$$

As is well known, the old text-books did not give the prior probabilities. The student was expected to assume that

$$P(H_0 \mid A)=P(H_1 \mid A)=P(H_2 \mid A)=P(H_3 \mid A),$$

and thus obtained the simple though dubious answer

$$P(H_3 \mid X)=\tfrac{1}{2}.$$

12. Let us amplify the example cited in the preceding paragraph as follows:

A large sack contains black and white balls in equal proportions. Three balls are drawn from the sack and are placed in a bag without being examined. A ball is drawn from the bag and is found to be white. What is the probability that all three balls in the bag are white?

We now have information on which to calculate the prior probabilities. Thus, assuming that the number of balls in the sack is indefinitely large,

$$P(H_0 \mid A) = \tfrac{1}{8}, \quad P(H_1 \mid A) = \tfrac{3}{8}, \quad P(H_2 \mid A) = \tfrac{3}{8}, \quad P(H_3 \mid A) = \tfrac{1}{8}$$

and

$$P(H_3 \mid A, X) = (\tfrac{1}{8}.1)/(\tfrac{1}{8}.0 + \tfrac{3}{8}.\tfrac{1}{3} + \tfrac{3}{8}.\tfrac{2}{3} + \tfrac{1}{8}.1)$$
$$= \tfrac{1}{4}.$$

This trivial and elementary example has been set out so fully for a particular reason, namely, to emphasize how the 'prior probability' and the 'likelihood' are conventional names for two terms in a mathematical formula. To regard them as two different kinds of probability can only lead to confusion. The three sentences:

(*a*) a sack contains black and white balls in equal proportions,
(*b*) three balls are drawn from the sack and placed unexamined in a bag,
(*c*) a ball is drawn from the bag and is found to be white,

are all parts of the total information. (*a*) and (*b*) in conjunction give the prior probabilities; (*b*) and (*c*) in conjunction give the likelihoods. There is no justification for separating (*a*) and (*c*) into different categories of information as though prior probability had some intuitive property, whereas likelihood was derived from empirical data.

13. Much of the disputation over inverse probability has occurred because of the frequent absence of the prior probability terms in applications of Bayes's theorem. When the prior probabilities can be estimated, obscurity recedes and the probability of a general hypothesis is seen to be a relationship which in its essential nature is no different from the probability of a particular event. When we base inferences about a population upon results observed in a sample, we are not indulging in a form of reasoning different from that employed in making predictions about a sample based on knowledge of the population. In both cases we are expressing a degree of belief regarding an unknown circumstance.

The principle which emerges from previous paragraphs is that the total information involved in a probability relationship forms a unity. Sometimes that information may embrace a complete field of knowledge. In the Etruscan problem cited earlier, an appreciable fraction of the total known facts of anthropology, ethnology and archaeology may be relevant to assessing the probability of the hypothesis formulated. Indeed, with certain scientific hypotheses it may be necessary to introduce the whole corpus of scientific knowledge into the information term in the probability relationship. But all such information is ultimately empirical, having been derived from the accumulated observations and experiments of the centuries.

PROBABILITY, NUMBER AND FREQUENCY

14. Hitherto no attempt has been made to discriminate between probabilities to which numerical values can be attached and those which do not admit of precise quantification.

The probability that the Etruscans migrated to Italy from Asia Minor can only be put into numerical terms when some piece of information comes to hand which either finally proves or finally disproves the hypothesis. The probability is then either 1 or 0 as the case may be. Until this final position is reached, the probability fluctuates between the two extremes. When a new piece of information favourable to the hypothesis is discovered, the probability moves a little nearer to 1. Similarly, an unfavourable item causes it to move nearer to 0. But nobody is likely to suggest that a definite quantity can be assigned to the probability in any of these intermediate stages.

Nevertheless, not all quantitative relationships are impossible. In the situation just cited, suppose that I is the available information before the new discovery and that i is the new item favourable to the hypothesis. Then we may not unreasonably write

$$P(H \mid I) < P(H \mid I+i).$$

Similarly, if H_1 is the hypothesis that the Etruscans came from Asia Minor, and H_2 is the hypothesis that they came from Saskatchewan, we may write with equal justification

$$P(H_1 \mid I) > P(H_2 \mid I).$$

It is the contention of the present paper that the complementary relations 'more probable than' and 'less probable than' together constitute the most primitive concept in the theory of probability. When a person says 'It will probably rain this afternoon' he means that it is more likely to rain than not. Thus if I represents the prevailing conditions at the time when the statement is made, and if

$$H_0 = \text{'It will not rain this afternoon'},$$
$$H_1 = \text{'It will rain this afternoon'},$$

the statement cited may be expressed

$$P(H_1 \mid I) > P(H_0 \mid I).$$

Since H_0 and H_1 are mutually exclusive and one of them must be true, it follows that

$$P(H_1 \mid I) + P(H_0 \mid I) = 1,$$
$$P(H_1 \mid I) > \tfrac{1}{2},$$
$$P(H_0 \mid I) < \tfrac{1}{2}.$$

This is the first stage in the progress towards numerical evaluation of probabilities.

15. Groups of probabilities are often encountered which can be arranged in rank order although they do not admit of numerical evaluation. Thus a medical officer may arrange a group of n lives of equal age in order of fitness and their respective probabilities of dying may then be placed in a chain of inequalities thus:

$$P(x_1 \mid j_1) < P(x_2 \mid j_2) < P(x_3 \mid j_3) < \ldots < P(x_n \mid j_n),$$

where x_r represents the proposition that the rth life will die within one year and j_r is the evidence obtained from a medical examination of that life.

If the n lives should all be in sufficiently good health to be accepted at normal rates of premium, they should constitute a random sample of normal entrants aged x, and the rate of mortality, q_x, based upon recent experience of lives falling in this class should be appropriate to the group under consideration. From this it might seem that the probabilities of dying within one year for all n entrants are equal to q_x. But such a conclusion has the appearance of inconsistency with the chain of inequalities set out above. A group of probabilities cannot, it would seem, be both equal and unequal at the same time.

The answer to this paradox is not far to seek. The information term in the first set of probabilities $P(x_1 \mid j_1)$, etc., included the fullest available details concerning the state of health of the individual. In the substitution of q_x for these probabilities, some of the information is neglected. The knowledge that each life is free from major impairment liable to surcharge is retained; but the finer details of the personal condition of each individual are discarded.

16. It follows from the last paragraph that, for probabilities to be numerically evaluated, some simplification of the elementary probability situations is necessary. This simplification takes the form of neglecting information, i.e. of a process of abstraction. Such processes of abstraction, involving neglect of individual features in order that a complex entity may be replaced by a schematic model, are common in scientific inquiry. Indeed, they are a natural condition of the grouping and classification which are essential to the description of multiple data in conceptual terms. By selecting certain common elements, and ignoring points of difference, a collection of individuals can be regarded as a class, and thenceforward it is the class that becomes the unit of inquiry and not the individual. It is this process which is put into operation when we seek to effect the numerical evaluation of probabilities.

17. Thus the use of class frequencies to represent probabilities is a device for replacing a non-denumerable concept by one that admits of numerical description. Normally it involves the notion of 'equally likely'. All members of the class are assumed to be indistinguishable—a natural corollary of the assumptions involved in the process of classification itself. Perks ('Some observations on inverse probability including a new indifference rule', *J.I.A.* 73, 285) has suggested that 'equally likely' is a notion prior to probability. I accept this with the modification that it is prior to the *numerical evaluation* of probability. Only by inventing the notion of 'equally likely' can we achieve the necessary simplification whereby we can express probabilities by numbers.

THE PROBABILITY SET

18. A class from which frequency ratios are extracted to express probabilities is commonly referred to as a 'probability set'. It is axiomatic that all members of the probability set are 'equally likely'.

Any specified event can be a member of many different probability sets. If we are told that Mars is inhabited by three races, the Lilliputians, the Brobdingnagians and the Laputans, and we are asked the probability that a particular Martian of unknown race is a Laputan, we may reply $\frac{1}{3}$. If we do so, we have formed the three races into a probability set and the result follows automatically. Later we may have access to a census of Martians and may learn the numbers of each race. We then revise the probability by forming the probability set from the whole population of Mars. But if the Martian in question has been encountered on the moon, it might be preferable to consult the passenger lists of the interplanetary travel agencies and to form our probability set from these. The situation is, in fact, in every way similar to that discussed in §8 above, where it was seen that the probability of rain on a given day could be variously estimated according to the manner in which a probability set was formed from past records of rainfall.

19. Thus the probability of a given event may have several different numerical values placed upon it, because each such value is a function not of the individual event but of a probability set of which the event in question

can be regarded as a member. Every frequency ratio which emerges from a probability set is an artificially constructed abstraction which can be used to replace the unknown—or rather unquantifiable—probability of each unique event contained in the set. And the constitution of the set will depend upon the particular items that have been abstracted from the total information available in the original probability relationship.

20. There will clearly be a considerable advantage in using the maximum amount of available information when determining the probability set. Thus if a man proposing for life assurance is aged 40, is 5 ft. 10 in. tall, weighs 15 stone, has a systolic arterial pressure of 175, lives in Southampton and follows the occupation of stevedore, it would be appropriate to form a set consisting of persons exhibiting all these several characteristics and to observe the proportion dying. This, however, is a counsel of perfection having no practical value, since the size of the class so formed would be too small to yield any useful result. The process of abstraction must be employed to build up a larger class possessing vaguer characteristics. 'Urban dweller' replaces 'living in Southampton'; 'social class II' replaces 'stevedore'; and so on. The constitution of the probability set depends entirely upon the manner in which this process of abstraction is applied, what information is neglected and what is retained. In replacing 'stevedore' by 'social class II' we are retaining our knowledge of the socio-economic status of stevedores but discarding what we know of the special occupational features of their work. If instead we replaced 'stevedore' by 'worker in the transport industry', we should be making a different selection from the available information, and in consequence we should obtain a different probability set with a different numerical value for the emerging probability.

21. The complications discussed in the preceding paragraph arise mainly from the multiplicity of characteristics which are normally attached to persons and things in their natural condition. It is, however, easy to construct artificial situations in which all members of a class are exactly similar and the property of 'equally likely' is not a conventional assumption, but is actually attained. The two sides of a coin, the six sides of a cube, the thirty-seven sectors of a roulette wheel are three examples of probability sets in which this condition holds good. Sampling experiments fall into the same category. The statement that there is a probability of ·99 that the mean of a sample of a given size will lie between certain specified limits is based upon the fact that if all possible samples of this size were examined and if their means were calculated, 99% of those means would lie between the specified limits and the remaining 1% would lie outside them. These artificially created situations, in which the principle of 'equally likely' is strictly ensured, represent the furthest point to which quantification of the concept of probability can be carried.

By isolating the probability situations which fall within this limited field of precision it is possible to build up a complete theory of probability of the frequency type. And this is in fact what many advocates of frequency theories have done, dismissing other types of probability from their attention. But a clear dividing line between probabilities that can be precisely evaluated and those which cannot be quantified at all does not exist; there is, in fact, an infinite gradation from a zone of precise quantification to a zone of vague verbal relationships, and a comprehensive treatment of probability should embrace the whole field and not be limited to a particular part of it.

22. It is worth pursuing the concept of an infinite gradation between extremes of vagueness and precision in its application to inverse probability, or the probability of hypotheses. Initially, before statistical data have been obtained from observations, the information available consists of the prior beliefs. It is from these that the prior probabilities are commonly inferred, and such prior probabilities are seldom capable of precise numerical assessment. As statistical evidence accumulates, the prior probabilities recede in importance. But the change is not discrete. If there are a number of balls of unknown colour in a bag, the initial probability that all the balls are white is a function of whatever prior information may happen to be available and will normally lie in the zone of extreme vagueness. If one ball is drawn and is found to be white we are beginning to move towards quantification, although we are still very far from precision. In fact we have gained an additional fragment of information which enables us to be slightly more confident in making a probability statement.

If the drawn ball is replaced and the experiment is repeated several times, vagueness diminishes. If 12 drawings out of 20 result in a white ball, we have a moderate degree of confidence in advancing an estimate of 6 as the probability that a ball drawn at random will be white. Increase these figures tenfold, with 120 out of 200 drawings resulting in a white ball, our confidence in the estimate of 6 is much increased. We are, in fact, moving closer to total precision.

Much, of course, depends on the size of the parent population. And this is so not only because precision is automatically linked to the sampling fraction, but also because the larger the population, the more difficult it often becomes in practice to ensure randomness. Not infrequently, statistical evidence is available which is known to contain bias. Nevertheless, probability statements about the parent population may be required, and the result is a blurring of the numerical evaluation with verbal qualifications. There is a retreat from precision towards vagueness.

QUANTIFICATION OF PROBABILITIES OTHER THAN BY FREQUENCY RATIOS

23. As indicated above in §2, this paper in some measure originated from a desire to analyse the situation in which a proposer for life assurance suffering from a medical impairment is assessed partly by reference to a standard table of probabilities derived from observed frequencies and partly upon the personal judgment of a medical practitioner. It is now proposed to examine this situation more closely.

Consider the case of a man aged 40 who is 20% over-weight. Such a man might be regarded as a member of a class formed from all men aged 40, or alternatively of a class formed from all men aged 40 who are 20% (or, say, 15–25%) over-weight. If insurance were compulsory the former approach would be quite reasonable; but with voluntary insurance, where there is an option against the insurer, the second approach should be preferred. As is well known, there are no British statistics giving rates of mortality for various classes of substandard lives. Accordingly, reference is had to the observed frequency ratios emerging from a class of standard healthy lives (from which over-weight persons will have been automatically excluded) and an adjustment is made to allow for the differences between the probabilities that a healthy life aged 40 will die within 1, 2, 3, ... years and the corresponding probabilities that an over-weight life will die within these successive intervals.

24. We thus have what may be termed a 'mixed situation' in which two methods of quantification are involved. In the first stage a class frequency is used to quantify the basic probability for a standard life; and in the second stage an act of judgment is made to give quantitative effect to a relationship of the form

$$P(x_1 \mid k_1) > P(x_2 \mid k_2).$$

Both methods are expedients for placing a numerical evaluation upon a concept which fundamentally is one of qualitative degree rather than of number. They differ in so far as in the first method information is neglected in order to form a probability set from which a precise numerical value may be extracted, while in the second method the full amount of information is retained, but the numerical evaluation now assumes a character of extreme vagueness. Thus the precision of the quantification varies inversely with the amount of information brought into account.

25. If a die is biased, the six sides of the cube are no longer 'equally likely' and at least one of the sides will turn up more frequently than one-sixth of the total number of throws. If the bias is exclusively due to a displacement of the centre of gravity from the geometrical centre of the cube, it can be measured and expressed in numerical terms. Between the amount of such displacement and the observed frequencies of throwing the various numbers marked on the sides of the die there will be a mathematical relationship, and it thus becomes possible to calculate probabilities by mathematical methods for the purpose of predicting frequencies.

This question of a relationship between a measurable intrinsic property and an external observed frequency opens up a field of speculation which deserves fuller treatment than is possible in the present paper. It was implicit in Gompertz's notion of linking the probabilities of death with an internal force of deterioration varying with age; and no doubt it would be possible to formulate analogous mathematical relations between the probabilities of death and blood pressure, weight-height relationships or other measurable physical characteristics. The existence of situations such as these, in which probabilities can be evaluated from other forms of measurement without the assistance of observed frequencies, is a cogent reason for seeking some wider definition of probability than is required by the frequency theory.

CONCLUSIONS

26. The object of this paper has been to express reasons why the frequency theory of probability is felt to be inadequate to cover the various types of probability situation which are encountered in practice. It is maintained that a comprehensive definition covering both quantifiable and non-quantifiable probabilities is desirable, and that the concept of probability should be envisaged as embracing a central zone where precise quantification is practicable surrounded by ever-widening circles of increasing vagueness.

The ability to quantify a probability depends upon the possibility of replicating the situation to which it is attached. Replication commonly involves the selection of specific items from the total amount of available information. Normally, the smaller the amount of information that is brought into account, the more precise is the numerical estimate of the associated probability. Thus numerical precision tends to vary inversely with the comprehensiveness of a probability judgment.

In considering inverse probability, or the probability of hypotheses, it is suggested that the total amount of information should be brought into the probability situation as a unity and that the distinction between prior beliefs and statistical observations is artificial. The probability of a hypothesis is no different in essence from a so-called 'direct' probability, the prior beliefs and the observational data frequently representing non-quantifiable and quantifiable elements respectively in a compound probability situation.

27. The literature of probability is so vast that it is not proposed to give a bibliography. A useful list of major works may be found at the end of 'Some notes on probability' by M. T. L. Bizley (*J.S.S.* **10**, 161). The standard works by Keynes, Von Mises and Jeffreys are widely known, and more recent works include W. Kneale's *Probability and Induction*, I. J. Good's *Probability and the Weighing of Evidence* and F. N. David's *Probability Theory for Statistical Methods.*

Among papers that have appeared in recent years, the following have been consulted:

AITKEN, A. C. (1950). Theories of probability. *T.F.A.* **19**, 229.

KENDALL, M. G. (1949). On the reconciliation of theories of probability. *Biometrika*, **36**, 101.

PERKS, W. (1947). Some observations on inverse probability including a new indifference rule. *J.I.A.* **73**, 285.

ABSTRACT OF THE DISCUSSION

Mr R. D. Clarke, introducing the paper, said that it was partly his war-time experience of having to deal with both statistical and non-statistical experience which led him to retreat from the frequency theory of probability and to adopt the ideas which he had endeavoured to express.

A short time previously, while reading Walter Bagehot's classic, *The English Constitution,* he had come across the following passage, which he thought was not irrelevant to the discussion.

> Most men of business love a sort of twilight. They have lived all their lives in an atmosphere of probabilities and of doubt, where nothing is very clear, where there are some chances for many events, where there is much to be said for several courses, where nevertheless one course must be determinedly chosen and fixedly adhered to.

Although those words had been published in 1867 they had a modern flavour and displayed a nice apportionment of emphasis between probability and decision.

In the preparation of the paper he had received much wise and helpful advice from Mr Perks and Mr Tetley. They were in no way responsible for the views which he had put forward, but he took the opportunity of expressing his grateful appreciation to them both, not least for their kindly forbearance in various matters where their opinions differed from his.

Mr J. G. Day, in opening the discussion, said that it was no accident that, in recent years, the theory of probability had exercised some of the keenest brains of the Institute, because on that subject their teaching did not match their practice, and in fact no full or completely satisfying theory had been produced which entirely justified their work when it was concerned with the chances of life or death. He was sure, however, that there would be general agreement that their practice was sound and that there was really nothing wrong in working without a fully comprehensive theory, although it did, he believed, leave the Institute open to some criticism. The author had produced a paper which put the problem before them in a particularly clear and lucid manner.

Some might be surprised that the theory and the practical side of their work could function so smoothly without a satisfactory definition of probability; but the position had strength as well as weakness. There was a parallel in the history of geometry. It was clear that Euclid had developed his theorems with common-sense concepts of points, lines and so on, but no rigorous definitions; eventually those common-sense notions, e.g. that a point had position but no size, had been written down as the basic definitions of geometry. Those definitions, however, were never openly used in Euclid's theorems, and many of their provisions were either unnecessary or over-sufficient. Not until centuries later, when the mathematicians wished to extend geometry to take in entirely new concepts such as relativity, had the definitions been replaced. Geometers had been forced to go back to first principles and to build up a much wider theory of geometry on the minimum of basic definitions. Euclid's geometry, although useful and highly developed, was only a special case of a much more general geometry. In the same way, a probability theory was needed of much wider scope than that afforded by frequency ratios, and it followed that the definition should be much less restricted. The strength of the position was that the frequency theory was not inaccurate but merely inadequate; it might be only a special case, but it would always remain the basis of much of their work.

The author had demonstrated the inadequacies of the frequency theory with great clarity, had pointed out that there were situations 'in which probabilities can be evaluated from other forms of measurement without the assistance of observed frequencies', and had stated that 'a comprehensive treatment of probability should embrace the whole field and not be limited to a particular part of it'. Continuing to particular points, he had drawn the critical distinction that all standard lives aged x were not equal, and that only by ignoring certain information could a class be formed with the notion of 'equally likely' and consequently a numerical evaluation of probability.

He appeared to think that when a standard life was accepted the calculation was based on a 'frequency' definition of probability, and that when a man was rated up the extra information was taken into account and the estimate of probability based on a degree of belief. It was at that point, the opener said, that he parted company with the author and would like to suggest that the practice towards standard lives involved a 'degree of belief' definition of probabilities, although frequency ratios might be used, usually indirectly.

If a valuation, or premium rates, were based on A 1924–29 mortality, there was also, usually, additional information, or the hypothesis was accepted, that current mortality was lower than 1924–29 mortality. With certain reservations about duplicates and so on, the 1924–29 data might be said to be an 'irregular collective' representing the experience of lives in that period; but the experience of a life in, say, the years 1953–63 could not be said to be a member of that group.

When accepting a life aged 40, most actuaries would say with a reasonable degree of belief that q_{40} in 1953 was less than q_{40} in 1924–29. They could go further than that, for accepting a life aged 40 in 1953 involved making an assumption about q_{50} in 1963. He thought that most actuaries would agree with a reasonable degree of belief that q_{50} in 1963 would be less than q_{50} in 1953, which was itself less than q_{50} in 1924–29; but those who gave a numerical evaluation to those inequalities would certainly not agree on the exact figures.

It might be objected that using $a(f)$ or A 1924–29 at all involved basing the work on frequency ratios, but it should be remembered that those tables were graduated; and, apart from the convenience, the argument for graduation was based on probability judgments. The belief in the value for the probability of death at age 60 was affected by the values at age 59 and age 61. There was a belief that a smooth curve was more likely than one with a lot of small bumps. The operator used, therefore, not the single-frequency ratio of deaths at age 60 to exposed to risk, but a probability based both on information about deaths aged 60 relative to the exposed to risk at that age and about the ratios at other ages, together with a belief in smoothness, a knowledge of other mortality tables and a knowledge of current trends. The resulting probability was a reasonable belief based on all the relevant information and in accordance with several accepted hypotheses.

Both the use of mortality tables and their construction, therefore, implied the use, not so much of frequency ratios, as of probabilities determined by personal judgment from frequency ratios and other relevant information. It should perhaps be added that all the arguments used applied as much to classes of life insuring themselves as to individuals.

The argument which he put forward would also appear to lead to a subjective definition, because any probability theory could be said to be objective if 'it embraces some principle which establishes a rule whereby all persons in the same state of relevant knowledge will necessarily agree' (Bizley, *J.S.S.* 10, 167).

If a female aged 70 bought an annuity, some actuaries would take $a(f)$ rated down 2 years, some would rate down 1 year, and others might make other adjustments; all, with the same relevant knowledge, would not give the same reply, which suggested that the definition of probability should include subjective probabilities.

The author, however, said:

> The fact that broad areas of agreement exist makes it possible to postulate a consensus of informed opinion as a basis for setting up the concept of a 'rational degree of belief'. It is its basis in a consensus of minds, rather than in individual minds, that lifts the 'degree of belief' concept above the level of subjectivism.

It seemed from that that the author had in mind a purely objective definition of probability as a basis for actuarial theory, but his definition of 'subjective' and 'objective' might be different from that of Bizley; he might be following Dr I. J. Good, who considered that probability was objective if it 'is defined or assumed to exist independently of the views of particular people'. All the same, it seemed difficult to visualize an independent objective probability for some single event to which any three actuaries might assign different numerical values in the normal course of their work; for example, all three might quote annuity rates for the same life based on different mortality assumptions, even though their offices had had identical mortality experience. There might be no consensus of minds, but quite distinct differences of opinion. Once it was admitted that the probabilities could be non-quantifiable and that two rational and reasonable individuals could have different beliefs when they had the same information, he felt that it was necessary to include subjective probabilities in a definition of probability; he said 'include' because, of course, objective probabilities would not be excluded.

The author, by his attitude towards prior probabilities and the consideration of a probability relative to all information, and particularly by his definite statement that 'a clear dividing line between probabilities that can be precisely evaluated and those which cannot be quantified at all does not exist', had outlined clearly his attitude towards probability; but it seemed a pity, when he had dug the foundations to such a special shape, that he had not, by a definition and an outlined probability theory, shown his readers the building which he proposed to erect on those foundations. They all had their pet probability theories, and he was sure that the author, having explored the subject so thoroughly, had a fairly clear formulation of the definition and theory which he had in mind.

There was a small and purely academic point which seemed to him interesting, namely, the basis of a doctor's judgment, to which reference was made in § 3. It appeared fundamentally that a doctor's views could be based on either (*a*) a frequency knowledge of a group of similar lives, or (*b*) an *a priori* judgment. A defective life might be compared to a weighted die; and, just as after studying the structure and form of a weighted die one might estimate that the chance of a 6 was one-third and not one-sixth, so after inspecting a 'life' a doctor might be able to say that the probability of death in the first 10 years was twice the normal probability.

He had discussed the point at length with an assurance medical examiner, who had been quite definite in his view that any opinion which he gave was based on experience of similar lives; he was certain that his views were based on a frequency judgment every time. His views were based on experience, either his own, or another doctor's, or the medical profession's appreciation of the information made available to doctors by the Registrar General or other authorities. He denied that there was any condition A which he could diagnose as leading to an early death by cause B which was not covered by the medical profession's knowledge of cause B. Typical of the arguments used to justify that statement was that (*a*) if the heart was sound it was so resilient and adaptable that he could imagine no condition which would, *a priori*, lead to a weakening of it; or (*b*) if the heart was not sound, any judgment would be based on the knowledge and experience of unsound hearts. That doctor did admit freely, however, that most of his opinions would not be quantifiable, even though based on experience.

The author had presented a balanced view of the theory of probability as it affected the Institute, having discussed many of the debatable sides of the problems without becoming entangled in particular controversies or theories. It was unfortunate that there was so much confusion in the profession about the subject. The teaching for one part of the examination showed a bias towards, even if it was not actually based upon, a frequency definition; yet much of an actuary's thinking was on a 'reasonable belief' basis, and in fact their teaching included such concepts as 'hand polishing', 'scope for individual judgment', and such phrases as 'refinements are out of place' and 'conservative long-term view', which implied the use of information and non-quantifiable probabilities in addition to the frequency ratio probabilities obtained in the orthodox way.

Mr G. D. Gwilt, F.F.A., said that he had been brought up to suppose that a perfect penny, if tossed up, would land an equal number of times heads and tails. Being a busy man he had not worried much about that until he received a copy of the paper under discussion. He then got hold of an ideal penny and had a look at it. He found that it was not exactly the same on both sides, there being a head on one side and a tail on the other, but he supposed that it would not affect the matter very much, and so he did the experiment of tossing it in a particularly simple way and it came down heads every time. He thought, therefore, that there must be something wrong with the idea that a penny was an excellent example of two equally likely cases.

He then went on to ask under what conditions the penny would land an equal number of times heads and tails. He calculated that a penny tossed up about 12 in. would turn round approximately 35 times, and that only a small variation of force was required to give the coin another turn. He discovered that it was quite simple to prove that if the penny turned round an infinite number of times and if the probability distribution of force was a nice smooth curve then the probability of heads was $\frac{1}{2}$: if the number of turns was small—as it was in his experiment—there would be a bias.

The point about it was that there did not seem to be in practice any real cases of 'equally likely', and on examination it appeared that the tossing of a coin depended on a probability distribution of force. From that it followed that it was not true to say 'I can take a penny; it is the same on both sides and therefore the chances are equal that it will fall heads or tails'.

Mr H. W. Haycocks asked whether, if the last speaker took a penny out of his pocket, held it in the palm of his hand, and asked someone to guess whether it was heads or tails, he would take evens.

Before discussing the author's paper, he wished to express his admiration for Perks's paper (*J.I.A.* **73**, 285), because he thought that the new paper showed exactly the same attitude that Perks had taken towards the problem in 1947.

It was an attitude of mind which had become much more common among philosophers and statisticians than when Perks wrote his paper, although it was the same attitude of mind that Keynes had adopted many years previously.

He found the author's paper interesting and provocative, but he did not think that it gave a complete analysis of the concept of probability. A rational belief in a proposition existed only because there was evidence for that belief. The notion of probability arose only when there were several possibilities or alternatives, and it was not known which had happened or which, in another case, would happen. It was the existence of alternatives which was common to all types of probability statement. That was the case even in a scientific model, because implicit in that model was a set of alternatives to which probability numbers were attached.

It was necessary first to analyse the situation into the possibilities before a probability judgment could be made. That applied to all the examples given by the author. Once the alternatives were stated, all the relevant evidence could be assembled, and the probability judgment was an induction based on the evidence. It was only after those steps had been taken that the probability calculus itself could be used. In so far as probability judgments were of practical use, he thought that some general presupposition about the 'world', such as the uniformity of nature, was in some sense also required. It seemed to him impossible to benefit by 'learning from experience' without such a presupposition. He thought that some such presupposition should be included in the symbol I.

In the light of those remarks, it was of interest to consider the statement that the probability that 5 was the millionth digit in the expansion of π was 1/10. Taken by itself, that statement was unduly complicated, and the average layman would not understand it. The author understood it, but he had not expressed his understanding well. The statement by itself did not explain how the author interpreted it. The millionth digit must be one of ten, because that was the numerical notation to which they had agreed. From the point of view of his normal behaviour, that was all that there was to the question; there was no reason why he should consider probabilities regarding various digits. It was only when he was asked to act on the supposition that the millionth digit was 5 that he worried about probability. If he were asked to bet, he would regard the introduction of π as irrelevant, since he was only being asked to guess one of the numbers 0 to 9. The probability statement which was given in the paper was a roundabout way of asking him to guess a hidden number, and to his mind the introduction of π was of the nature of 'eyewash' and might lead to a lot of misunderstanding. That sort of thing occurred throughout the literature on probability. An ambiguous or incomplete statement was made, and, because it could be interpreted in a number of ways, a host of pseudo-problems were raised.

Not all the examples given by the author could be brought under a simple betting scheme in an obvious way. The fact indicated that a theory of probability could not be reduced to the simple matter of stating odds. For example, there was no point in the author asking him to bet on whether the Etruscans migrated to Italy by sea from Asia Minor because there was no method of obtaining a decision. But it would be possible to bet on whether, as an historical statement, it would be accepted by a specified group of historians. In the same way, it was not generally possible to bet on whether a man was guilty of murder, but only on whether the jury would convict him, which was a very different matter.

On pp. 7 and 8 the author discussed the question of individuals belonging to different classes and so being assigned different probabilities depending on the reference to class. That had already been referred to by the opener. He did not agree with the author's interpretation, and he was not sure that he agreed with the opener's. He did not think that the author had chosen the best way of analysing the situation. It was not merely a question of simplification. The point was that they were not so much interested in the individual probabilities as in the application of the probability model, and the application did not require a precise estimate of individual probabilities. In life assurance they were interested in the expected deaths rather than the individual probabilities, i.e. they were interested in the sum of the probabilities rather than in the individual items.

It might perhaps be better to label q_x the 'expected proportion dying' rather than the 'probability of dying'. Even when setting up a formal scheme it might be better to think in terms of each individual having a different probability rather than each individual having an equal probability. The point was that in many applications, both in business life and in science, they were interested only in expected numbers and therefore need estimate only expectation. It was often the case that the only reliable estimates were estimates of expectations. That, of course, was only an aspect of the relative stability of the mean. That was not the whole story, and again the point had been mentioned by the opener. In life assurance they wanted a q_x which either over-estimated or under-estimated future mortality. The actual belief was not a belief in the probability itself, but a belief in the statement that the table on the whole would give hypothetical deaths which exceeded the actual. In modern statistical estimation that situation was allowed for by introducing a 'risk function' which expressed the relative advantages or disadvantages of using an estimate. In practical life they preferred to avoid trouble rather than to make a precise estimate of it. The taking of avoiding action was a necessity for survival. They would take avoiding action even if the probability were minute, provided the consequences would be dire, and that applied to life assurance. What it amounted to was that they were not interested in some absolute estimate, but in the estimate weighted by the utility of each possible outcome. If there was no utility or disutility the outcome would not influence them and they were not interested in the probability. There might, of course, be a formal interest.

There was another matter, namely, the assignment of probabilities to scientific hypotheses. He did not think that that served any useful purpose. In §§ 12 and 13, for instance, the author gave some simple illustrations of drawing balls from a sack and then jumped to a statement about assigning prior probabilities to a general hypothesis. A scientific hypothesis was not a proposition which was true or false; the words used about it were such words as 'plausible' or 'adequate'. A scientific model, for example, a probability model, was instrumental in allowing the passage from statements about past data to statements about other data; both sets of statements included statements about frequencies and it was those statements which were true or false, and the adequacy of the model was judged by the comparison of those statements with the actual observation. It was also worth pointing out that a scientific theory was concerned with universals and not particulars. In the model it was sufficient that the postulated probabilities produced the right expected values when large numbers were involved. Provided the observed facts were accounted for the model might as well be as simple as possible. He thought that that was the reason that scientists tended to favour the frequency definition.

Dr I. J. Good (a visitor) asked whether Mr Haycocks, if he were on the jury at a murder trial, would attempt to estimate the probability that the rest of the jury, or the whole jury, would find the man guilty, or to estimate the probability that the man was in fact guilty. He hoped that it would be the latter.

He was glad to contribute to the discussion, although he found it difficult to express his own position in the space of a few minutes. He agreed with most of what the author had said, and especially with his emphasis on the use of inequalities and with his discussion of precision *versus* comprehensiveness.

The author agreed with Jeffreys, for example, in postulating the existence of objective rational degrees of belief, sometimes known as 'credibilities'. Keynes also made that assumption when he wrote his treatise, but withdrew it in a later essay on F. P. Ramsey. It was possible to dispense with the assumption and to construct a subjective theory covering all possible applications of probability, but it was mentally healthy for a thinker to regard his degrees of belief as estimates of credibilities. Moreover, a subjective theory could always be interpreted as a multi-subjective one without any modification. The words 'you' and 'your' could be used as technical terms referring to the person or persons doing the believing. It was convenient that the plural forms of these words were the same as the singular ones.

The simplest way to avoid committing oneself to the limiting-frequency definition of probability was to use an axiomatic theory. The definition of probability was then not explicit, but was provided by the axioms together with the rules of application. The substance of the definitions by 'limiting frequency' and by 'equally probable cases' were then both available for practical purposes, as a consequence of theorems instead of definitions. In that way an axiomatic theory by-passed the criticisms which could be levelled at the classical definitions of probability.

The axioms gave rise to an abstract theory. Combined with the rules of application, that gave the theory of probability. The function of the theory of probability was to introduce a certain amount of objectivity into the body of beliefs, where a body of beliefs was defined as a set of judgments of inequalities or equalities between degrees of belief. The abstract theory was a black box into which could be plugged judgments and from which could be fed out compulsory judgments, or rather discernments. When there was no input, there could be no output. Judgments were required in every application of the theory. It was impossible to reduce the subject to formal logic alone. The theory could be used for increasing the size of bodies of belief and for detecting inconsistencies in them.

The standard form of a judgment, in that theory, was an inequality between degrees of belief. Judgments of that form were sufficient to provide a workable theory, but it was convenient to use other types of judgment as well. For example, judgments could be made of inequality between 'weights of evidence' (or 'log-factors'), between 'indices of potential surprise' and between 'expected utilities'. Those additional types of judgment were all of great importance, but he would have to be content to give references to those who were interested.* If utilities were introduced it became necessary to extend the theory of probability

* (1) *Probability and the Weighing of Evidence* (London, Griffin, 1950).

(2) *Symposium on Business Decisions under Uncertainty*. British Association, 1953. (To be published by the Liverpool University Press. Edited by Professor G. L. S. Shackle.)

(3) *Rational Decisions*, *J.R.S.S.* (Series B), **14**, p. 107 (1952).

into a theory of rational decisions or rational behaviour. That extension was performed by incorporating the 'principle of rational behaviour', i.e. the recommendation to maximize expected utilities.

The author's argument that prior probabilities were not always very prior would be less necessary if a different terminology were generally used. Following von Mises, it was possible to refer to 'initial' and 'final' probabilities. The word 'prior' sounded a little too much like '*a priori*', and encouraged people to assume that Bayes's postulate of equiprobability would necessarily be invoked. It was sometimes convenient in sequential procedures to refer to 'initial', 'intermediate' and 'final' probabilities.

With regard to the question of the probabilities of purely mathematical statements, such as that the millionth digit of π was a 7, he thought that the best answer was to modify the theory of probability itself. Instead of using the axiom A4,

> If the propositions E and F are logically equivalent, then $P(E \mid G) = P(F \mid G)$ and $P(G \mid E) = P(G \mid F)$,

there might be used the modified axiom A4′.

> If *it has been proved* that E and F are logically equivalent, then $P(E \mid G) = P(F \mid G)$....

and so on. Under the original axiom A4 the probability of a mathematical theorem was always either 0 or 1, although it might not be known which. In pure mathematical research it was important to make estimates of the probabilities of mathematical theorems in order to decide how much work was worth investing in trying to prove them. For that purpose the modified axiom A4′ would of necessity be used. Its use was necessary also if it was desired to say that a mathematical theorem, or the result of a calculation, provided information, where the amount of information in a proposition was defined as 'minus the logarithm of its initial probability' (see Chapter 6 of ref. (1) in the footnote on p. 19).

Mr W. Perks found himself in substantial sympathy with the author's ideas on probability. He thought that the paper was beautifully written, but it was also deceptively simple, so that it was necessary to read it with extreme care to avoid being misled about what it actually meant. He said that with some feeling, because he had been himself misled several times, only to find on careful reading that after all he agreed with the author.

He was sure that the author was right in wanting to get rid of dualism from probability theory. It seemed to arise because of confusion between theory and application and because of a misguided desire to base probability on frequency. The place for frequency was in the applications; it was undesirable at the foundation, because it limited the scope of the theory, and that meant that other arbitrary principles had to be created in the applications. In actual applications, as had been pointed out by Jeffreys, the frequentists never did without the idea of degree of belief. The plain fact seemed to be that a rigorous frequency theory was impossible. The definition of probability as a mathematical limit of relative frequency was self-contradictory. A definition as a limit 'in probability' involved either circularity or it depended on 'degree of belief'.

To avoid that reliance on degree of belief, some text-books, for example Johnson and Tetley, used some such vague phrases as 'about 99% of cases'. The author of the paper under discussion skated over that difficulty in § 21. The only way to avoid that vagueness and to retain frequency, in the speaker's view, would be to retreat into an infinite regress. On the other hand, the frequentists were able to point out that a degree of belief theory never got away from degree of belief. A reply to that had been given by Dr Good, and it was, in effect, that a pure axiomatic theory did not really define probability at all, or even prescribe its sphere of applicability; it merely specified the mutually consistent properties which the symbols were to have and then deduced the theorems. Application and interpretation were not possible until some simple probabilities were specified or implicitly assumed, such as, for example, that all the faces of a die were equally likely to be uppermost.

In applying Bernoulli's theorem, as they often instinctively did in large sample work, they could usually interpret a chance-in-a-million belief as a once-in-a-blue-moon frequency. It was one thing to do that at the application stage and quite another to try to base a theory upon it.

He was sure that the author was right when he emphasized in § 12 of the paper that 'prior probability' and 'likelihood' should not be regarded as two different kinds of probability, but it should be recognized that in practical applications their interpretations and justifications might be essentially different. The author's second example of the application of Bayes's theorem in § 12 was, he thought, quite unhelpful, because the prior probabilities were there specified by a sampling process and had an immediate frequency interpretation. That case would be accepted as valid by all the probability schools, and it was therefore quite unsuitable as a basis of argument about dualism or unity of information.

While it might be right to talk about the unity of information in a general sort of way, he doubted whether it was ever helpful in the applications. The author himself abandoned the idea as soon as he started to quantify. By abstracting some of the information he implicitly distinguished the rest of the information by a belief that it was irrelevant to his probabilities, and that was surely the justification for the process of abstraction. The author then proceeded, in §§ 16 and 17, to introduce the notion of 'equally likely'. He, the speaker, believed that an advance of the greatest importance had been made by Dr Good in his well-known book when he introduced the notation $P_\beta(X|H)$ for the probability of X on data H and given the body of prior beliefs β. No probabilities could ever be deduced unless the prior beliefs were first specified. That was obvious in games of chance, where they always specified what was equally likely. The paradoxes in geometrical probability had been resolved only when it had been recognized that it was necessary to specify what was equally likely. In a sampling experiment they always specified the distribution model and the sampling process; i.e. they specified the prior beliefs. It was not surprising that in the application of Bayes's theorem it was also necessary to specify the prior beliefs. He could not himself think that it was helpful to confuse those beliefs with the brute facts in some metaphysical unity of information.

It was instructive to analyse the situation in an application of Bayes's theorem to a binomial experiment, assuming that there was no prior information. The given facts were then that there had been m successes out of n trials. The body of beliefs included three separate beliefs. The first was that the drawings were random and independent, and that was justified by the care taken in the actual sampling. The second was that the probability of success at the first trial was precisely one-half. That was justified on the logical ground that there was no information one way or the other about the possibility of success at the first trial. It rested on the basis of a fundamental economy principle, never discriminate without good reason, a principle which went beyond Occam's razor, and which had yielded enormous dividends in science. The probability set to which that case belonged was not the set of drawings in the particular experiment, but the set of cases where the logical abstraction was a pair of alternatives with no information favouring one rather than the other. In that way its probability could be given a frequency interpretation.

It was the third belief required for the application of Bayes's theorem that was the difficult one; that a particular probability distribution expressed the maximum degree of symmetrical uncertainty to be attached to one half as the prior estimate of the value of the binomial parameter. Without the specification of that belief it would not be possible to proceed with the application of Bayes's theorem. The justification of the actual form chosen lay in its minimal effect on the posterior results, in the universality of its range of application, in the logically consistent results which it produced and in the avoidance of unacceptable results. Its justification was essentially pragmatic, as was only right and proper for the king-pin in a formal process of inductive reasoning.

In § 17 of the paper he was glad to see that the author agreed with him about 'equally likely' being a notion prior to probability. He thought it desirable to stress that the whole of his own paper in 1947 had been concerned with the numerical evaluation of probability. Although he did not think that it was important, he believed that the primitive concept of 'equally likely' was more fundamental than the concepts 'more likely' and 'less likely'. Jeffreys used all three in his first axiom. In practical applications inequalities were often all that was known, but at the level of the foundation of the theory he thought that it would be possible to define the inequalities in terms of 'equally likely', whereas any attempt to define 'equally likely' in terms of 'more likely' and 'less likely' would require a sophisticated limiting process.

The author's use of the word 'precision' needed careful interpretation. In the application of Bayes's theorem to a binomial experiment with no prior information, the probability of a success at the first trial was precisely one half, in the author's sense of the word 'precisely', but in its more usual sense, the precision of one half as an estimate of the binomial parameter was essentially at a minimum. That meant that it was possible for the probability of success at the first drawing to be absolutely 'precise' whilst the estimate of the proportion of balls in the bag was completely vague. It was only in that way, so far as he could see, that the references in §§ 22 and 26 to vagueness and precision and to the amount of information could be reconciled.

There was scope for confusion also between the 'probability set', or, as he preferred to call it, the 'reference class', and the sequence of trials by drawing. It would be easy to jump to the conclusion that the author had not escaped that confusion, but a careful reading of the second paragraph of § 26 showed that the author did not identify probability with the results of replication; he merely made the possibility of replication a condition of quantification of probability. The difficulty was in the specification of the form of replication, and that was often misunderstood. It might be exact replication of the logical features abstracted from the practical situation under consideration. However, he was not sure that he accepted that particular conclusion of the author's.

Mr A. W. Joseph said that it was almost an axiom of probability that the simplest problems gave rise to the greatest doubt, difficulty and discussion, and that had been why he welcomed an old friend in §§ 11 and 12. In the primitive form of the problem all that was known was that one ball out of three in a bag was white. That led to vague and imprecise feelings about the colours of the other two, based perhaps not only on the known colour of the one ball, but also on the knowledge that white balls were not usually made singly, and that where one white ball came from others might usually be found. The problem was then narrowed by the disclosure that the three balls were drawn from a large sack containing black and white balls in equal proportions. By the use of prior probabilities and Bayes's theorem the probability that all three balls were white was calculated to be 1/4. Since the drawn ball was known to be white, that meant that the probability that the remaining two balls were both white was 1/4. That result could actually be deduced from the single hypothesis that the two balls were drawn at random from a large sack containing black and white balls in equal proportions, so that the interesting fact emerged that when the additional information about the source of the three balls was forthcoming, the information concerning the colour of the one drawn ball was not used in any way at all in determining the probable colour of the other two. The information about the drawn ball was a red herring, an irrelevancy whose only importance was to determine with certainty the colour of that drawn ball. Any vague impression about the colour of the other two, deducible from the colour of the drawn ball, would have been absorbed in the more precise information about the balls in the large sack.

It was that failure to assess properly the interplay of various pieces of information which was at the bottom of some gambling superstitions. If in roulette red had turned up 20 times—in itself an improbable but possible event—then, if the wheel was true, the previous coups were irrelevant, and the chance that red would turn up at the next spin of the wheel was 18/37. If there was a possibility that the wheel was biased, then the previous coups were not irrelevant, and the chance that red would turn up on the next spin was greater than 18/37, but exactly what it was would be almost impossible to say. Curiously enough, the most usual superstition after 20 red numbers had turned up was that the next coup was likely to be black, for which he could not see any logical justification of any kind.

Mr R. C. B. Lane confessed that he had not done much work on the subject under discussion since he first learnt his probability; but he had been much encouraged by the simplicity which had been praised in the paper, though discouraged by some of the discussion. However, he would take his chance.

He looked back to his earlier days, when he knew nothing about the theory of probability but had some idea about the chance of this and the chance of that happening. He remembered that at school he knew that they had a good chance or a poor chance of winning the boat race, or that they might win a cricket match. He had a sort of idea—he supposed that it was a degree of belief—that an event was likely or unlikely, as the case might be. In those days he thought that probability extended from 0 to infinity, and he had no idea of probability from 0 to 1 only. Later he had learnt his theory of probability based on frequency ratios, and he had had to get used to the idea that probabilities would be limited to the range between 0 and 1. It gave him no particular difficulty at the time or since.

On going a little beyond that certain difficulties were met and situations arose where it was not easy, and might even be impossible, to find frequencies of which to take a ratio; and that, he thought, was the sole cause of the difficulties which gave rise to papers and discussions such as those with which they were concerned that evening. If they could always have frequencies of which to take the ratio and which they could measure, there would be no need for any of those higher flights of theory. He admitted the difficulty, and he admitted that, having got the concept of probability in their minds, and having to deal with a practical probability in such circumstances, there was a certain value in going further, in generalizing inductively and in saying that even where a probability could not be measured in terms of the ratio of two frequencies it might yet be proper to say that there was a probability, and to assign a probability number similar to and obeying the same sort of laws as their old friends the frequency ratios. The paper explained nicely and intelligibly how that sort of thing cropped up, but he thought that it was a great pity that, when the protagonists of the degree-of-belief theory got to that stage, they turned round immediately and tried to deny the foundation of that theory, which he thought was historically, and he believed was still logically, a frequency theory.

He did not think that they could ever get away from that; he could not do so himself, and he did not believe that anybody else ever had. Earlier in the discussion it had been said rather challengingly that the 'frequency theory' people never got away from the degree of belief. There might be truth in that, but the converse was certainly true: the 'degree of belief' people had not got away from their frequency ratios. He thought that there were two or three reasons for saying that. He had never, for example, seen a theory of probability which extended from 0 beyond 1, and he could not see where that 1 came from unless it was in a frequency ratio. It would be natural and reasonable to assign infinity as the measure of certainty and he was sure that the man in the street knew what 'infinitely likely' meant, even if a body of actuaries and philosophers did not.

Originally the law of combinations of probabilities started on a frequency theory. Any higher-flown theory of probability was bound to come back to frequency theory. Perhaps there were higher-flown theories which did not give the same laws of combination as the frequency theory but they were not heard of and were of no practical value. Moreover, whenever the devotee of a higher theory of probability came back to a practical problem he had to measure a probability, or he did not get anywhere; and the speaker did not think that any probability which was measured, as distinct from estimated or guessed, was measured except in terms of the ratio of two frequencies. That was true of actuarial practice and of statistical and scientific applications. Sometimes it was not possible to get a frequency array; enough cases could not be got in the time available. Then it was necessary, as the author said, to fall back on an inductive argument, on other information, on a belief in a certain high degree of correlation between, say, high blood pressure and high mortality, and so on. All sorts of things helped in assigning a probability number, but they did not ever finally get rid of the belief that what they were trying to estimate was what the frequency ratio would be if they had enough cases—enough stevedores with high blood pressure living in Southampton—and enough time to do the experiment and measure the relevant frequency ratios. He did not believe that they got beyond that point.

He sometimes wondered whether, having built up their notion firmly on a rough, layman's idea of probability, and having gone through the stage of frequency ratios, and having been floated right off into the air with ideas of probability numbers quite unrelated to, and supposed to be quite divorced from, frequency ratios, they ever came down again to the ground; but he was sure that when they did they would still be standing on a frequency ratio.

Mr R. S. Skinner, F.F.A., began his remarks, as a Fellow of the Faculty who had been in London for nearly 5 years, by thanking the Institute for the welcome and the hospitality which he had always enjoyed when he had been able to come to its meetings.

He confessed that as the discussion had progressed he had become more and more depressed. He had always found difficulty, he said, in combining or trying to combine the discussion of any hypothesis, such as that mentioned by the author about the Etruscans, which must be either true or false, and degrees of credibility, with the normal probabilities with which actuaries had to deal. As the meeting progressed he had begun to feel that he must be extremely stupid, and he had to confess that, because of various activities, he had been unable to give the subject the rigorous thought which it demanded.

Mr Lane's contribution to the discussion, therefore, had come as a great relief to him. He remembered hearing Professor Whittaker describe how geometricians had constructed various geometries and made certain assumptions and acted on certain axioms, producing beautifully logical theories which no one outside the realms of certain mathematical studies ever heard of. He thought that much the same might apply in the case under discussion. Those theories had no application to the actual facts of life. Eventually, of course, Euclid's theory of geometry had been extended by Einstein and others to a form which did appear to correspond to the facts of life as known in modern times, but when it came to measuring a carpet for a room ordinary measuring instruments and the theories of Euclid were still used.

There had been efforts by various distinguished thinkers which had produced theories which endeavoured to bring under one cloak, so to speak, all the different problems, degrees of credibility, hypotheses which could only be either true or false, and the other problems which came directly back to frequency ratios, a process in keeping with the general trend towards unification in modern science; and yet, when actuaries went back to their work, all that mattered was that they should be able to produce reasonable working probabilities. Sometimes, as had been said, it might be impossible to deduce a suitable frequency ratio and then it was necessary to substitute for it, by relation to other data or otherwise, some figure which was believed to be reasonable, but essentially it was frequency ratios which were dealt with.

Dr N. L. Johnson said that he too had been cheered up a little by the last two speakers. He had read the paper with a certain amount of fear and trembling, lest the purity of his frequentist faith should be imperilled by reading heretical writings. However, after he had been through it he had been reassured, because it seemed to him that the whole of the points raised in the paper could be interpreted by regarding probability as frequency. The paper seemed to be mainly concerned with methods whereby that frequency might be estimated.

Various methods were put forward. One of the points made was that sometimes—he himself would say always—it was not possible to say precisely what the frequency was. It was however, just that knowledge of the frequency which was desired. He was grateful to the author for emphasizing the fact that the concept of frequency implied some reference class and, as a necessary corollary, that it was possible to have different probabilities for a given event by considering it as a member of different reference classes. Those reference classes were more familiar to students under the name of 'series of trials', and one event could be regarded as belonging to more than one series of trials. The frequency with which events of a given type occurred would not necessarily be the same in different reference classes.

That was an aspect of the paper which he wished to emphasize again, namely, that when all was said and done there remained an estimate of the frequency of a certain type of event in a certain reference class. As one deduction from that, it was not possible to consider the probability (other than 0 or 1) of a single isolated event. Taking the author's example of the number which was some digit of π, that was, the author stated, a single event, and a probability of 1/10 was suggested, but unfortunately the author did not give the reference class of which this event was to be considered a member.

It might well be that some theory could be constructed relating to events where there was no particular frequency concerned, which would provide guidance for action; but, if that were so, he saw no reason for regarding it as part of probability theory. As had been pointed out by a previous speaker, the origins of probability theory were concerned with estimating frequencies. There was, for example, a very early book by Cardano on games of chance, written about 1524, in which it was remarked that if there were something wrong with a die it was necessary to test it to see how often the various faces turned up. He was definitely concerned there with frequency.

While there was no desire on the speaker's part to deny the possibility of developing the ability of the human mind to deal with problems wherein it could not assign frequencies, the science of probability as it had been known was not concerned with such cases, and if that new science were to be developed it ought to be given another name.

Mr H. Tetley, in closing the discussion, said that anyone who had attempted to keep in touch with probability theory and its modern developments could not fail to have often echoed the words of Ecclesiastes:

Of making many books there is no end; and much study is a weariness of the flesh.

Not only were there many new theories or new presentations of old theories, but the ideas involved were often extremely subtle and took the reader into rarefied atmospheres where the speaker, at any rate, could not hope to follow. Probability was becoming, in fact, the preserve of the academically-trained man or woman who could devote a great deal of time to the study of the literature, which was growing at an amazing rate, and to a study of the extremely abstruse arguments involved.

That was not to say that actuaries should ignore so fundamental a subject, but rather that they could, he suggested, render their best service to the development of probability theory by concentrating more on its practical applications, in the hope that they could point out ways in which the various theories seemed to lead to results which were mutually incompatible or which it was difficult to accept.

The author had proceeded somewhat on those lines, although it was inevitable that he should fail, in the 12 short pages of what was an extremely lucidly written paper, to do full justice to some of the theories which, after all, needed an entire volume for their development.

Personally, he could not accept all the author's arguments, although he admired the extremely clear way in which they had been set forth. He could not, for instance, accept the view which the author expressed on the first page of the paper, that there was 'a blatant dualism in the traditional actuarial approach to the chances of death and survival'. From a fairly extensive experience of medical officers, he would say that their assessment of the state of health of a proposer was fundamentally based on relative frequencies—a point made by a previous speaker—although the medical officer might subconsciously be adding or combining several simple probabilities. A doctor often said 'In my experience this type of trouble does not become serious until about age 60, but it usually kills them off by 70 or 75.' That was fundamentally a frequency approach. If the doctor did his job properly, he drew on his own experience and the experience of others whose work he had read or knew of in some other way, in order to assess out of several hundred such proposers how many were likely to survive, say, 10, 15, 20 or 25 years. Some offices invited their medical officers to present their assessment of the case somewhat in that form. To meet the argument that a doctor could not expect to have seen many cases exactly like the one he was examining, with a particular combination of impairments, he would reply that there the doctor was, subconsciously perhaps, arriving at a compound probability, having first assessed the simple individual items.

Although he confessed that he was open to conviction, his chief difference with the author arose over what the author called a probability, but what he himself would often call an estimate of a probability. For instance, provided that the problem was sufficiently clearly defined—and that in itself was a difficult piece of work—many actuaries would arrive at different estimates of the probability that a healthy man of 27 would die in a year. In several probability theories, each of those would be a probability. He found it difficult to accept that view, and would prefer to say that they were different estimates of one probability and not different probabilities. He was, as he had said, open to conviction on that, but he felt that he was on stronger ground when dealing with the point raised by the opener and also mentioned by Dr Good. The opener had put it that in practice they often, in calculating premium rates or annuity prices, deliberately adopted probabilities which were not based on observations or other knowledge; in other words, they introduced a bias or a loading, a safety margin, and incidentally at the same time they made use of other knowledge, such as q_x at neighbouring ages.

When they introduced a definite bias for safety, he could not accept the view that the resulting figure was a probability, or even an estimate of probability; it was simply a figure which was going to be incorporated in some arithmetical calculations and which would eventually produce financial figures. It was adopted in the teeth of the evidence and involved a deliberate departure from knowledge of the data; to the extent that it departed from that knowledge it was not, in his view, either a probability or even an estimate of a probability.

Whether they agreed with the author or not, however, they would all agree that he had that evening shaken them out of their complacency, and it was to be hoped that that would lead others, like himself, to think again and seriously about beliefs which they had cherished for a long time, and with which they had been most comfortable and happy. The interesting discussion which had taken place would no doubt continue for a considerable time after the meeting had dispersed.

The President (Mr W. F. Gardner, C.B.E.) thought it was clear that great interest had been shown in the paper by those who were closely and technically versed in the subject with which it dealt. It might be that there were one or two members who, like himself, did not feel so well versed as they ought to be, or perhaps, speaking generously, as they used to be. They would, however, he was sure, feel with him that the paper was eminently readable and refreshing.

He had felt that the author had done himself less than justice in remarking on the first page that his paper had 'little pretension to practical utility', but he had remedied that point in his introductory remarks by the very apt quotation from Walter Bagehot which he had given.

To the practical and the administrative mind, the very scope of the author's concept of probability had an appeal. Decisions of moment inevitably depended to some extent on a cautious estimation of chances. Speaking personally, he had felt that evening that the discussion had tended to confirm his impression that those chances usually lay in the widening circles of vagueness rather than in the central zone of precise quantification. He had pleasure in proposing that a vote of thanks be accorded to the author for his beautifully expressed and evidently stimulating paper.

Mr R. D. Clarke, in reply, expressed his gratitude for the vote of thanks and for the discussion, which he had greatly enjoyed. He appreciated the adverse criticisms no less than the comments of those who were in agreement with him. It was pleasant to have that kind of discussion, when there was so much to be said on both sides. He could not reply in detail until he had been able to read the transcript of what had been said, and he would therefore leave his considered comments for the *Journal.*

Mr Clarke, in his written reply, says: I fully agree with much that Mr Day said. He pointed out that when actuaries made adjustments to the mortality rates given in a standard table they were automatically superseding frequency theory by a 'degree of belief' approach. Mr Tetley disagreed with the view that this process represents an application of probability, but I cannot share his disagreement and I do not see how the point can readily be resolved as it partly depends upon definitions. I believe that we are using probability when we make allowance for something which we think may happen in the future. Actuaries think it probable that mortality rates will continue to fall and their experience enables them to give quantitative effect to that probability judgment in the form of an adjustment which they make to values derived from a standard table.

Mr Day demurred to the suggestion that the concept of a consensus of informed opinion might form a basis for an objective theory of probability. I recognize certain objections and have no wish to run away from them. Nevertheless I believe that this approach offers a possible solution to the difficulty of establishing probability as an objective concept while defining it in terms of belief. As suggested in the paper, the notion of a consensus of minds can be applied to other problems lying outside the field of probability. It does not matter that there may be variations of opinion so long as there is a clustering around some definable position. The fact that reasonable people of trained intelligence tend to arrive at similar (though not necessarily identical) conclusions from a given body of information is the basis for positing the existence of some objective value which each individual is, to the best of his ability, endeavouring to estimate. But to develop the theory fully involves metaphysical arguments extending far beyond the subject under discussion.

On the question of the basis of a doctor's judgment, which both Mr Day and Mr Tetley took up, the exact words used in the paper are

The assessment of an extra risk by a Medical Officer, although it involves a large element of personal judgment, is not entirely uninfluenced by observed statistical frequencies.

In other words, a doctor makes a comprehensive judgment based upon a corpus of information which includes, *inter alia*, some knowledge of observed statistics. But even if there were a complete set of medico-actuarial tables of mortality, there would still be scope for a doctor to say 'this man is a bad case, so we will charge him rather more' or 'this man is really quite a good case, so we will be lenient.' There is a clear contradiction between the statement made by the opener's medical acquaintance that 'he was certain that his views were based on a frequency judgment every time' and his subsequent admission that 'most of his opinions would not be quantifiable'.

Mr Gwilt, in his experiments with a penny, was really concerned with the tossing process. It is of course a *sine qua non* of all such experiments that conditions of complete randomness must be guaranteed. In all sampling experiments great care has to be taken to avoid bias. But to do so is normally practicable and I do not feel that my central zone of precision, within which it is possible to quantify probabilities with exact numbers, is in danger of capitulation.

I agree entirely with Mr Haycocks's comments regarding the dependence of probability upon evidence. In the paper I was at pains to emphasize the importance of the information component in the probability relationship and I would certainly accept Mr Haycocks's contention that general premises, such as the uniformity of nature, should be included in that component, i.e. in the symbol I. I regard the uniformity of nature as a law inferred from general observation and not in any sense an *a priori* proposition.

I referred in the paper to probabilities concerning the nth digit in the expansion of π because it is a familiar topic in probability literature. I agree that in essence the problem is no different from that of attaching a probability to an unknown digit chosen at random. But this does not make it a 'pseudo-problem', nor can I agree that any 'eyewash' is involved.

I cannot accept Mr Haycocks's view that probability questions arise only when people are asked to act on certain suppositions. It is, of course, all-important that those who are responsible for decisions should have a full understanding of probability and its application. But probability itself is concerned with a state of incomplete knowledge and exists as a proper subject of study in its own right. It is largely because I hold this view that I differ profoundly from Mr Haycocks on the question of historical propositions. The probability that a historian, or group of historians, will accept a particular hypothesis is an entirely different thing from the probability that the same hypothesis is true or false. The former involves the training, prejudices and psychology of the historians. The latter involves the formation of a rational judgment upon the available evidence. Dr Good made this point effectively in his question regarding the functions of a juryman. I find Mr Haycocks's reference to betting on historical hypotheses somewhat irrelevant. Although the calculus of probability originated in the consideration of odds connected with games of chance, I see no reason why one should attach a probability to a statement only if one is prepared to bet on it.

I feel there is a lot to be said for Mr Haycocks's suggestion that q_x may preferably be labelled the 'expected proportion dying' rather than the 'probability of dying'. At the same time there are occasions when nothing may be known except a person's age and sex—e.g. when valuing a reversion in which there is no other information concerning the life tenant—and then q_x from a suitable table will be a proper quantification of the probability of dying, because it will then be matched to the full amount of information available.

There seems to be a misunderstanding concerning the phrase 'probability of hypotheses'. This term is nowadays used by some authorities in place of 'inverse probability' and I prefer it because it explains more graphically what is implied. I agree that the elementary example of drawing balls from a sack is over-simplified. But it is fundamental to the whole paper that the probability of hypotheses does not differ in kind from the probability of events. We are, however, once again in danger of running into confusion over definitions. I do not regard a 'model' as a 'hypothesis'; I agree, in fact, that models are introduced into science for utility purposes and that notions like probability and truth are irrelevant to them.

I am grateful to Dr Good for his observations, especially for drawing attention to von Mises's terminology of 'initial' and 'final' probabilities. I was concerned to combat the notion that prior probabilities are in some sense fundamentally distinct from posterior probabilities, as though their origin lies in intuition rather than in experience. The terms 'initial' and 'final' seem a very helpful way out which I am glad to accept.

This step may go some way towards narrowing the gulf between Mr Perks and myself in the one matter on which there appears to be a difference of opinion between us. I am quite ready to preserve a distinction between initial and final probabilities, provided that such distinction is chronological and not a difference in their essential nature. In particular, I want to get rid of any suggestion that intuition is involved in the prior beliefs. I am convinced that all prior beliefs rest ultimately on observation and experience and it is for that reason that I am unable to draw a clear dividing line and separate the information component into two watertight categories. At the same time I fully admit that in practice it is not usually difficult to make an *ad hoc* distinction between the prior information and the immediate data.

I greatly appreciated Mr Joseph's analysis of the example in §§ 11 and 12. I recognize now that this is not a satisfactory illustration of the point I wished to make. I wanted to consider a case where the prior probabilities would be known and specified. But in so doing I sacrificed generality and produced in fact, as Mr Joseph demonstrated, a problem which can be solved without involving Bayes's Theorem at all. Nevertheless it still holds good that, once it is possible to quantify prior probabilities, the distinction between inverse and direct probability vanishes.

I feel that the main difference between myself and those who, like Mr Lane, Mr Skinner and Dr Johnson, uphold the frequency theory is that I do not consider that the frequency theory is adequate to cover all the probability situations which arise. Wherever possible I should make use of frequency ratios to quantify probabilities. But I should continue to use the concept of probability in many situations for which frequency ratios are not available. I cannot help feeling that Mr Skinner in particular made a substantial concession when he admitted that, if a suitable frequency ratio were not available, actuaries would 'substitute...some figure which they believed to be reasonable'.

Dr Johnson stated his position with great clarity when he said that the science of probability as we know it is not concerned with cases in which no frequency could be assigned and that some term other than probability should be applied in situations of this type. I take the exactly opposite position and should use the term probability to denote the attitude adopted towards any proposition as to the truth of which we are in a state of incomplete knowledge. To me the situations in which probabilities can be quantified by frequency ratios are a sub-class within the wider class formed by all probability situations.

Mr Tetley underlined the distinction between a probability and an estimate of a probability. I believe that, on a given set of relevant information, there exists a probability for a specified proposition. If the probability lies within what I have called the zone of precision, it can be quantified exactly and there is no need to refer to 'estimates'. It is in cases where the probability lies outside this zone that estimates enter into the picture. As I said in the paper (§ 6):

> There may be various estimates of that probability made by different persons or by the same person at different times.

It seems to me that this statement is not discordant with what Mr Tetley said about the estimates that different actuaries might make of the probability that a healthy man of 27 would die in a year. I agree with Mr Tetley that these various estimates are not different probabilities. On the other hand, if one were to vary the information, e.g. by stating that the man in question was an Armenian or that he belonged to the Merchant Navy, then there would indeed be a different probability to be considered.

Returning to my disagreement with Mr Tetley on the subject of allowances for future changes in mortality, I believe that these are definitely the fruit of probability judgments. At the same time the introduction of a deliberate bias as a safety margin represents a probability judgment of quite another kind. Here the insurer is not asking what premium he must charge in order to have an expectation of breaking even, but what premium is necessary so that the probability of an over-all loss will not exceed some specified maximum. In other words we have entered the territory which is usually referred to as the Theory of Risk.