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LECTURE FOUR  
CHANCE

I have spoken of mathematics and works of art as capable of validation but not of verification and said that an apophatic theology would lead us to include religious worship in the same class. It has indeed become apparent that even in classical physics we must regard verification as relying somewhat on validation.

Keeping this wider significance of our subject in mind let us now turn to a more detailed analysis of validation as distinct from verification in science. For it is by demonstrating the part which it plays in science that we may hope above all to establish confidence in the process of validation. The more so, since the opinion that science allows us to accept only what is verifiable, is the chief obstacle to the firm establishment of intellectual systems that are valid though not verifiable.

The avowed purpose of the exact science is to establish intellectual control over experience in the terms of precise rules which can be formally set out and subjected to critical reflection including strict empirical tests. Could this be fully achieved, all truth and all error could henceforth be ascribed to this exact theory of the universe, while its upholders would be relieved of the responsibility of making any judgments of truth or falsity. Classical physics approaches this ideal to the extent to which the element of personal judgment involved in the process of measurement can be neglected. A mathematical space-time map of planetary motion predicts the position of the planets in numbers from which we may compute the readings on the scales of a telescope fixed on the planet at any particular time. But such computation has no precise meaning. There exists no strict rule by which the numbers to be accepted as measured quantities for the purpose of a physical theory can

be computed from readings made on an instrument and consequently we cannot compute either the exact readings on an instrument from the measurable numbers predicted by a theory. Any correlation between a measured number introduced into an exact theory and the corresponding instrument readings, rests on an estimate of observational errors which cannot be definitively prescribed by rule. This indeterminacy is due in the first place to the statistical fluctuations of observational errors, but apart from these fluctuations we have also the possibility of systematic errors. For even the most strictly mechanised procedure leaves something to personal skill in the exercise of which an individual bias may enter. We should remember always the famous case of the Astronomer Royal Maskeleyne who dismissed his assistant Kinnebrook for persistently recording the passage of stars more than half a second later than he, his superior. Maskeleyne did not realise that an equally watchful observer may register systematically different times by the method employed by him; it was only Bessel's realisation of this possibility which 20 years later resolved the discrepancy and belatedly rehabilitated Kinnebrook. Despite its reduction through the use of the transit micrometer, this type of error is still present in modern astronomy. "This personal equation" - writes H. N. Russell in 1945 - "is an extremely troublesome error, because it varies with the observer's physical condition and also with the nature and brightness of the object. Faint stars are almost always observed too late, in comparison with bright ones; this gives rise to the so-called Magnitude equation." <sup>1</sup> We must allow accordingly that some trace of a hidden personal bias may always affect the result of a series

1. H. N. Russell, R. S. Dugan and J. Q. Stewart. "Astronomy I. The Solar System" Princeton, 1945, p. 63.

of readings.

Moreover, deviations between an exact theory and an exact measurement do not in themselves, invalidate the theory, for they may always be classed as anomalies. The perturbations in the planetary motions that were observed during the century after Newton's death and could not be explained by the mutual interaction of the planets were rightly set aside at the time as anomalies, in the hope that something may eventually turn up to account for them without impairing - or essentially impairing - Newtonian gravitation. Indeed, on reflection there will always be apparent some conceivable scruples which we had set aside in the process of verifying an exact theory and which we shall continue to set aside even after having thus faced them.

Yet the theories of classical mathematical physics differ from all other chapters of science by the fact that events are conceivable which would strictly falsify them. It is conceivable for example that a sun with a planet circling around it are both so far removed from other outside bodies as to render negligible any perturbations caused by the latter. We may then have a theory which predicts the path of the planet exactly and this prediction would be exactly falsified, by the fact that the planet failed to take up a predicted position at a predicted time. A finite deviation, however slight, would entail a complete refutation of the theory, in the same sense in which the slightest finite deviation from a theorem of geometry would invalidate that theorem.

If we now turn to quantum mechanics we find a different situation. The space-time map of atomic events offered by quantum mechanics is as precise as that of classical physics, but the references of this to the events are in terms of probabilities, and we shall see that predicted probabilities cannot be strictly contradicted by any conceivable events.

I shall simplify my argument by fixing attention on the lowest energy level of a system, say of a hydrogen atom, which may remain unchanged forever. The factor of time being eliminated, the quantum mechanics of a hydrogen atom in the ground state is expressed by a pure space map - in three dimensions - assigning a number to every point of infinite space in terms of its distance ( $r$ ) from the atomic nucleus. This number  $f(r)$  which is the amplitude of the wave function at the point in question has a fractional value and the square of this fraction defines the probability of finding the electron of the hydrogen atom at this particular point or at any other point having the same distance  $r$  from the atomic nucleus.

The decisive reason for which the prediction of a numerically definite probability of finding an electron at some particular point of space, cannot be contradicted by any conceivable event, lies in the fact that such a prediction is ambiguous. It admits that the electron may be found or not be found at the designated place on the specified occasion.

However, if there is, as I believe that there is, some meaning in assigning a numerical value to the probability of our finding an electron at a certain place on a particular occasion, this assignment must imply some restriction on this ambiguity, and if no such restriction can be derived from it the assignment of this probability in objective terms we may expect to find in it instead some guidance to our personal participation in the event to which the probability statement refers.

When I reflect on my response to events which may or may not occur and whose occurrence I believe to depend on chance, I find myself expecting them with a degree of alertness corresponding to the numerical value of their probability and, when such an event takes place I find myself surprised by it to

a greater or lesser degree, depending inversely on the numerical value of its anterior probability. I believe these reactions to be justifiable and am prepared to regard them as the proper appraisal of an event governed by definite chances, both before and after it has taken place. This personal participation of mine in the event to which a probability statement refers, I regard as the proper meaning of its probability.

It may be argued however that if the expectations derived from a statement of probability are repeatedly disappointed I will begin to suspect the correctness of this statement, which to this extent will appear falsifiable by experience. R. A. Fisher has developed this procedure systematically for the purpose of establishing statistical judgments; which consist in deciding the tenability of a null-hypothesis the negation of which is a significant statement.

Take Darwin's experiments on the influence of a cross fertilisation as against self fertilisation on the height of plants, which R. A. Fisher describes as a standard example for the application of this procedure. 15 plants of each kind were measured and 15 pairs formed at random from which 15 differences in height were obtained. The differences being denoted by  $X$ , their mean is  $\bar{X}$ . The magnitude  $S(X-\bar{X})^2 = 19,945$  which is the sum of the square of the deviations from the mean, being computed, this is divided first by 14 and then by 15, yielding the result 94.976. The square root of this which is 9.746 is then taken to be the standard error  $\sigma$  of the 15 observations. The decisive statistical test consists in comparing the observed mean difference  $\bar{X}$  with the standard error  $\sigma$  of this difference. We form the ratio  $\bar{X}/\sigma = t$  which in this particular case is 2.148 and then consult a table which tells us what the probability is for  $t$  to have any particular

value for an experiment based on 14 independent discrepancies. As a result we note that the value 2.145 is exceeded by chance in exactly five per cent of such random trials.

The next step of the argument then appears to bring a statement of probability to the test of experience. The above computation refers to what is the null-hypothesis in this case, namely that the differences in the heights of self-fertilised and cross fertilised plants are accidental. It tells us that on this assumption our actually observed sample had a probability of occurring that was less than 5%. That is a statement concerning the probability of a past event which entitles us to be surprised by the observed result to the same extent as we would be if we drew a black ball from a sack alleged to contain only 5 black balls in a 100 otherwise indistinguishable balls. Now suppose that we had ourselves placed the balls, 85% of them white and 5% of them black into the sack, and then having shaken them up, we drew out a black ball. We should be very surprised, yet remain unshaken in our belief that the bag contained the balls we had put into it. Not so, however, for our null-hypothesis. In this case R. A. Fisher suggests (and I am prepared to follow him) that we must abandon the assumption that cross-fertilisation as against self-fertilisation has no effect on the height of plants for the probability of Darwin's results when computed on these grounds turns out to be so small that it renders this assumption untenable. Indeed we may accept Fisher's recommendation of a standard procedure for disproving a null-hypothesis, based on the exclusion of probabilities that are less than 5%, though it is already apparent that this procedure can apply only to hypothetical assumptions that are considered to be of a likelihood comparable to that of the ineffectiveness of cross-fertilisation as against self fertilisation and not to probability statements, such as refer for example to the

assumptions of a such high degree of likelihood as we would hold in respect of the continued presence in a bag of the black and white balls placed in the bag.

Of course, a series of results of sufficiently low probability might shake our initial assumptions even when they are most firmly held. Thus the card-guessing experiments of Rhine in the U.S. and of Saul in England have rendered untenable the null-hypothesis that in these experiments the card to be guessed had no effect on the guess of it. But in these cases the probabilities of the observed results as evaluated on the basis of the null-hypothesis had to fall very far below 5% in order to shake our belief in it. Since there is no finite limit to the confidence we may reasonably place in a null-hypothesis there is also no lower limit to the probability of events which we may assume to have occurred on the assumption of some null-hypothesis. It is clear therefore that a bare probability statement, given as such, cannot be strictly contradicted by any event, however improbable this event may appear in its light.

The contrast of probability statements to the essentially unambiguous character of classical dynamics, is sharply brought out if we assume for the moment the same idealisation of the events referred to in both cases. Suppose that all experimental errors in ascertaining these events could be reduced to zero, while the events could also be isolated from all external perturbations and could be assumed to be free of any kind of anomalies. In these circumstances the predictions of Newtonian mechanics would, as I have said already, be quite unambiguous and its laws would be strictly falsifiable by any deviation of the observed events from these predictions. But not so the laws of quantum mechanics or any other system of probability statements, such as refer for example to the odds

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in a game of chance. They are not converted into unambiguous statements by reducing observational errors to zero as well as assuming the system to be so isolated as to eliminate all extrinsic perturbations. It is not legitimate therefore to ascribe to exact statements of probability the same measure of objectivity as that possessed by statements of classical physics; the two kinds of statements can be sharply distinguished in this respect.

A consistent refusal to recognise our personal participation in an accredited reference to external objects, we lead us to deny that probability statements imply any reference to objects and suggest that they are only concerned with propositions. This interpretation of probability has indeed dominated modern theory of probability since J. M. Keynes first proposed it in his Treatise on Probability, published in 1921.

Taking for our exemplifying Darwin's investigation of the effect of cross-fertilisation as against self-fertilisation on the height of plants we would consider in this light that its result is a proposition H, "cross fertilisation enhances growth", rendered probable on the evidence that can be summed up in the proposition E, "the mean of the 15 observed differences is 2,148 times larger than the calculated standard of error of the 15 observed differences". Thus we would establish a probability relation  $P(E/H)$  between two propositions, which is a belief not about events but about a relation between propositions. Alternatively we find the result described as conveying a certain degree of belief in H based on the evidence E, which is symbolised by  $P_B(E/H)^x$ .

But this analysis does not correspond to actual or indeed any acceptable practice. Darwin's intention was to

(X) Jeffreys  $P(E/H)$ ; Good  $P_B(E/H)$ .

establish an effect of cross fertilisation on plant growth and not a relation of a proposition asserting such an effect to a proposition about observed figures. When Rhine undertook to investigate the chances of card guessing he wanted to find out whether extra sensory perception exists, and not whether there obtains a relation between the assertion of its existence and the proportion of guesses recorded.

Moreover, both these investigators claimed to have established a probability statement  $H$  and not the probability of a statement  $H$ , nor the particular degree of belief in  $H$  which would correspond to the observed evidence  $E$ . The difference between a probability statement on the one hand and the probability of a statement or the degree of belief in a statement on the other, may seem elusive but is actually quite obvious. Take the throw of a die. I say that the probability of a six to be thrown is  $1/6$ ; that is "a probability statement  $H$ ." There are six such probability statements referring to the throw such as "the probability of a one to be thrown is  $1/6$ ".... etc., all of which I jointly hold to be true. If on the other hand we are to make statements  $H$  about the throw that are not probability statements they must be of the form "a six will be thrown", "a five will be thrown", "a four will be thrown", etc. All six such statements are foolish but being equally foolish they have all exactly the same justification. These six contradictory statements are supposed to become mutually compatible and severally acceptable by being held not with certainty but with a degree of probability or belief to which we ascribe the number  $1/6$ . But obviously nobody can believe that a die will fall with each of its six sides uppermost at the same time, and no adjustment of the degree of this belief will make it acceptable. Nor is it true to say that we believe

that a die will always fall with a six on top but are rather uncertain of this, while at the same time we believe that it will always fall with a five on top but are rather uncertain of this too, and so on. It is absurd to describe our state of mind in these terms; and any attempt to do so can only be prompted by a desperate desire to avoid saying that the chance of throwing a six is  $1/6$ , which would make an ambiguous and yet significant statement about an external event. I conclude therefore that in so far as we arrive at probability statements on the lines of the statistical method illustrated by Darwin's or Rhine's investigations, or as made every day about the toss of a coin, these are statements about probable events and not probable statements about events.

The Keynesian interpretation of probability as designating the strength of beliefs held on the grounds of a given evidence E has actually much more far reaching intentions than I have so far mentioned. It aims at comprising the process of inductive inference within the calculus of probability which should supply us with a formal rule for indefinitely increasing our belief in a proposition H in view of the accumulating evidence E. If this programme could be successfully carried out it would eliminate altogether any act of personal commitment from the affirmation of empirical inferences. Since I believe this to be impossible, I shall continue now my critique of the Keynesian position with the intention of controverting this implication of it.

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Admittedly, there are certain cases in which certain observed events may lead to the affirmation of a proposition as probable. Suppose I know that the true answer to a question is either 'yes' or 'no' and that the true answer is printed on five sides of a die and the false answer on the sixth side. I may throw the die, read what is printed on it and declare that

the probability of this being the true answer is  $5/6$ -th, while the probability of it being the false answer is  $1/6$ . But it would be absurd to declare with a confidence of  $5/6$  that what came uppermost is the true answer and simultaneously to assert with a confidence of  $1/6$  that it is the false answer.

I suggest that this somewhat crude model correctly reflects the procedure of inverse probability by which it is customary to draw inferences concerning an unknown aggregate from a limited sample taken from it. Contrary to the usual parlance we can never obtain by this procedure alone, without adding any other decisions of our personal judgment, anything but statements about the probability of events, even though these events may be that such or such a proposition is true.

This is not to deny that propositions can be held with different degrees of belief. We have admitted this already in accepting probability judgments as between a null-hypothesis and its denials, which presupposes that we grant grades of anterior confidence to these alternative probability statements. We may add that this holds notoriously also for our acceptance of the unambiguous statements of classical physics, as for example of the theory of relativity. In such cases the degree of our hitherto attained assurance plays a legitimate part in determining the amount of further evidence required to satisfy us altogether in respect of the validity of a theory.

But these acts of variably graded affirmations must be envisaged within a wider perspective than the probability calculus. For this they must first be formulated as my own affirmation, by writing them in the form of asserted formulae or asserted sentences. If the formula or sentence is denoted by  $p$  my assertion of it is declared by writing down  $\vdash p$  which read "I believe  $p$ ". Herein is expressed the fiduciary quality that invariably adheres to every sincere affirmation

made in respect to any external or internal fact, whether it is an unambiguous affirmation or the affirmation of a probability.

Any attempt to replace the fiduciary element of an affirmation by a probability statement leads to a futile regress. When I write down "I believe planets move in elliptical orbits" I commit myself to an unambiguous assertion about planets. If I write instead: "There is a probability  $S$  that the proposition 'planets move in elliptical orbits' is true" I mention a probability statement which yet needs to be affirmed by me. This opens up a regress the aim of which recedes at every step exactly to the extent of this step.

The reverse test leads to the same conclusion. Take the formula for the wave function of the hydrogen atom in its ground state or more conveniently, the sentence "the probability of throwing a double-six is  $1/36$ ." These are obviously not expressions of the same kind as the assertion sign  $\vdash$  or its reading 'I believe'. It is clear that both the wave function of hydrogen and the sentence about throwing a double six require prefixing by the assertion sign to become the expression of an affirmation. Probability judgments like those of Darwin on cross fertilisation or of Rhine on card-guessing also require prefixing by the assertion sign if they are not to be merely mentioned but affirmed by the writer.

It is of course possible to make two different uses of the word 'probable', one in the manner which includes it in probability statements and probability judgments and another which would take the place of 'I believe...' as a reading of the assertion sign. We would have to avoid then any impersonal wording such as "it is probable...", which would still lack a personal prefix if it is to express an affirmation, and would have to use instead words like 'I consider it probable...'.  
 I have a sense of 'probable' which is not 'you' believe.

Such a phrase if understood as synonymous with "I believe with moderate assurance..." would effectively express the affirmation of a sentence or formula followed by it. It would not need to be prefixed nor allow itself to be prefixed by an assertion sign.

We might try to go even further in reconciling our new requirements with previous usages. Empirical affirmations prefixed by "I believe..." or "I consider it probable..." can be responsibly made only in due consideration of the evidence available from observation. Though the evidence will usually not be fully specificable, we may assume for the sake of argument that it was and imagine it expressed in the affirmation of E. It may then be thought that the affirmation: "I consider it probable that H is true, for it fits in with E" is equivalent to  $P(E/H)$  as used by Keynes or his successors.

There would be of course no objection to the use of these symbols in this new context, provided they were no longer used also with an entirely different meaning. Now the predominant purpose of the Keynesian school was to generalise the calculus of probability so as to include the process of empirical inference. It aimed at representing the empirical inference H, based on E, in the same formal terms by which a probability statement or probability judgment H can be derived from an observed evidence E. As a result however  $P(H/E)$  can in fact represent neither the first nor the second. I have shown before in detail that it cannot be taken to affirm the probability of chance events; but it cannot be taken either to describe states of mind since it does not refer to any particular person, who alone could have a definite state of mind and even if it did, it would still only make a psychological observation of a process of judgment and would still leave unasserted the correctness of the judgment.

X Keynes speaks of 'psychological beliefs', Jeffreys of 'rational beliefs' and Good of what 'you' believe.

Therefore, if (as I believe) every affirmation contains an ultimate fiduciary element which cannot be replaced by the operations of any strict rules but can be expressed only for a definite person by the declaration of his own personal commitment, the symbols  $P(W/E)$  could stand for such a declaration only if the theory in which they had hitherto played a part were first completely abandoned.

This logical argument will gain in scope by relating it to psychological observations made on the expectations induced in animals and men by exposing them to a variable series of events. Experiments by Humphreys have shown that persons will acquire the habit of blinking when a light is shown, both if the showing is invariably followed by a puff of air blown into the eye or if the puff is administered only on frequent occasions, at random. But the expectations involved in the two habits were shown to be different when the administration of the puff of air was definitely discontinued. Subjects trained according to the first method rapidly lost the habit of blinking while those trained according to the second persisted in it through a larger number of tests. A vivid illustration of this effect can be given in terms of a statistical guessing experiment in which a signal light was followed by a second light, either invariably or in 50% of the cases, at random. At the completion of the training the subjects of the first experiment were guessing the occurrence of the second light rightly with 100% frequency, while those of experiment the second were guessing at chance level, i.e. about 50% right. The curves show that after the showing of the second light was definitely discontinued, subjects of group 1) soon ceased to expect that it would turn up again while those of group 2) at first increased their percentage of positive guesses and then comparatively slowly ceased to expect it altogether.

The expectations induced in group 1) appear similar to those affirmed by classical physics. Arising from a confrontation of the subject with an unambiguous correlation of sign and event these expectations are sharply disappointed the moment the correlation is discontinued and they are quickly abandoned in consequence. By contrast, the expectations induced in group 2) appear similar to those of quantum mechanics or any other probability statement such as refers, e.g. to the spin of a coin. They are not easily disappointed by any turn in events, though they are gradually weakened and eventually extinguished altogether when they can be upheld only by considering the events which have actually occurred as having been extremely improbable.

We can relate these psychological observations to our logical analysis of empirical inference by endorsing the process which they describe as a rational mode of behaviour on the part of the subjects. Having acknowledged that the observed subjects were forming justifiable expectations and abandoning them later on reasonable grounds we may try to enlarge this acknowledgement by analysing their performance in further detail.

We will note then in the first place that both kinds of expectation were held by the subjects, with variable degrees of confidence at various stages of their experience and that their confidence was finally reduced to zero by a series of consistent disappointments. We note that the fiduciary element contained both in an unambiguous affirmation and in the affirmation of a probability may grow from a vague hunch to a sense of unshakeable certitude and sink again down to a mere lingering trace of suspicion.

I shall also acknowledge it as reasonable to make either kind of affirmation and to entertain the corresponding expectations the more confidently, the more consistently they

are borne out by experience and to allow our confidence to ebb away and gradually to vanish altogether if experience continues to conflict with them or can be reconciled with them only on the assumption that the events that have occurred were exceedingly improbable.

Such guidance as I offer here to my personal judgment in drawing inductive inferences may sound similar to the attempt that I have repudiated of deducing empirical inferences from a calculus of probability, as propositions possessing the highest grade of probability. Yet this guidance is sharply distinct in principle from these or any other attempts to define the procedure of empirical inference in terms of formal rules. For what I am formulating here is merely a set of maxims, similar to rules of an art, the correct application of which is an integral part of the art. I am not trying to unburden myself of the ultimate responsibility for drawing inferences from experience by substituting the operations of an impersonal machinery for the act of my personal judgment. The maxims by which I guide my judgment are spoken within this commitment and the meaning implied in the terms used in these maxims is part of this commitment. They are not meant to be understood as I understand them by anyone who does not share this commitment. Another person may accept my maxims for the guidance of his empirical judgment and yet come to different conclusions in respect to any particular case by applying its terms differently to the facts of the case. And if he is stubbornly sceptical of any empirical inferences, he could easily show that my maxims are altogether ambiguous, as they evidently must be to anyone reflecting on them critically.

The range of maxims which may justify our inferences from experience is wide and varied. At this stage I only wish to illustrate the part played by the conception of chance in

framing these maxims, and for this I shall first have to define randomness in terms of chance.

Take the series of heads and tails formed by the consecutive throws of an unbiased penny. In the theory of probability such a series is called an 'irregular collective' of which it is asserted that as the number of its members is increased indefinitely the ratio of heads to tails will approach ever more closely to unity. What is called 'irregular' here is usually called 'random': it consists in our example in the complete independence of every consecutive throw of the penny from any previous one. More generally, the randomness of a series asserts that it is quite impossible to predict in any way, even as a matter of probability, any following member of the series from the knowledge of its previous members.

A belief in the total disconnectedness of two events may be regarded as the limit to which a belief in their connectedness may decline. When reduced to zero strength by repeated disappointments, my belief in either an unambiguous or in a statistical relationship of two collocated events, will amount to a belief in their utter disconnectedness. They are believed then to occur independently of each other or at random in respect to each other; their occurrences being irrelevant to each other.

Engineers devising processes of telephony and telegraphy have raised some questions about random aggregates which have a bearing on the maxims of empirical inference. Compare the probability of guessing right a given series of  $n$  digits of the number  $\pi$  which are known to form an irregular collective with that of guessing right a given series of  $n$  coin-tosses. The first is  $\frac{1}{10^n}$  the second  $\frac{1}{2^n}$ . Both guesses would be very surprising but the first would be  $5^n$ -times more surprising than

the second. Information theory suggests that we might identify the measure of this surprise with a measure of the information that could be packed into the  $n$  digits of the random aggregate, if used in the most efficient way as a code for spelling out a message.

When a random aggregate has been thus conceived as a message its randomness is totally lost, since the deliberate process by which the consecutive members of the sequence were chosen by the sender of the message assigns, on some grounds known to the sender, to each member of the sequence its particular position in the sequence. This change from randomness to distinctiveness of the aggregate is an expression of my belief that somebody (who may have been myself) has selected its members from elements available at random in accordance with some criteria of appositeness.

We may generalise this to any configuration believed to be contrived by some person on rational grounds. We may appraise the amount of choice involved in producing this configuration by assessing the improbability of its having come into existence at random. For example, at the railway station in Abergele there is an arrangement of pebbles which forms the inscription "Come to North Wales by British Railways". The distinctiveness of this arrangement could be measured by comparing the range of configurations over which the pebbles could be distributed at random all over the station garden, with the range within which they must lie in order clearly to form the letters and words of the inscription.

Our acceptance of experienced facts and of relations between the facts may be analysed in analogy to this mode of appraisal. In a universe of random flickers there would be no

recognisable events nor relation between events. We appraise as an event something that shows coherence in space and in time while at the same time it would be conceivable that its several space time fragments might be scattered about instead as unrelated bits and flickers.

The most elementary instance of empirical inference is the recognition of solid objects standing out against a background. We speak of books, toothbrushes and dogs because such objects have parts which persist in close continuity in space and time while the same particulars are completely disconnected from other particulars which we accordingly relegate to the background. The higher the improbability of the observed covariance of the particulars having occurred by chance, the more substantially significant will their collocation appear to be. But the improbability of having found the space-time parts of a book, a toothbrush or a dog collocated accidentally in the manner observed in these objects cannot be appreciated without antecedent recognition of these objects as natural entities. For it is meaningless to assess the improbability of an accidental coincidence unless there is something non-accidental (like the text of a message known to have been deliberately formulated) with which a coincidence is conceivable.

Suppose we started from the parts of books, dogs, etc. in ignorance of the existence of natural objects and tried to compose from them entities which if formed by accident would appear to be rare coincidences, we would find open an infinite variety of choices the overwhelming majority of which would not constitute acceptable natural entities. There are for example any number of equally improbable arrangements of the space-time parts of a dog which would not be recognised as a natural object. Imagine a sound film taken of a barking dog jumping and running

about. One reel of this film would contain 15000 frames. Let us pick out and join together every  $n$ th frame of the film, say every 13th, and then again every 13th of the remainder and so on, till all the frames, except for a negligible residue, are incorporated in a new sequence. The rearranged film represents an alternative configuration of the space time parts of the barking dog which is exactly as improbable as the original, yet if you watch this film running off you hear nothing and see nothing.

The impasse can be overcome only by specifying more closely the kind of relationship which we mean by co-variance of the parts forming a natural entity and also the characteristics of its converse, the randomness of internal particulars in respect to elements of the background. Not unless we credit ourselves with the capacity for distinguishing between order and disorder can we justify our speaking of the improbability of a random aggregate constituting an ordered array. And this is the only context in which the improbability of an observed event can be properly referred to.

In the conception of order, to which I shall turn in the next lecture, we shall find a framework for our personal participation in the universe which underlies all theoretical contemplation and reveals its kinship with the whole sphere of rational mental activities.