A World of Propensities: Two New Views of Causality

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Dedicated
to the memory
of my dear wife,
Hennie

Ladies and Gentlemen,

I shall begin with some personal memories and a personal confession of faith, and only then turn to the topic of my lecture.

It was 54 years ago, in Prague in August 1934, that I first attended an International Congress of Philosophy. I found it uninspiring. But the Congress was preceded by another meeting in Prague, organized by Otto Neurath, who had kindly invited me to attend a 'Preliminary Conference' (*'Vorkonferenz'* as he called it) which he organized on behalf of the Vienna Circle.

I came to Prague with the corrected page proofs of my book, *Logik der Forschung*. It was published three months later in Vienna, and in English 25 years later as *The Logic of Scientific Discovery*. In Prague it was read by two Polish philosophers, Alfred Tarski and Janina Hosiasson-Lindenbaum, the wife of Tarski's friend and collaborator, Adolf Lindenbaum. Janina Hosiasson and her husband were murdered when, 5 years later, the Nazis invaded Poland and systematically exterminated what they described as its *'Führerschicht'*: its 'intellectual élite'. Tarski went from Prague to Vienna where he stayed for a year and where we became friends. Philosophically, it was the most important friendship of my life. For I learnt from Tarski the logical defensibility and the power of absolute and objective truth: essentially an Aristotelian theory at which, it appears, Tarski and Gödel arrived, independently at almost the same time. It was first published by Tarski in 1930, whereupon Gödel, of course, accepted Tarski's priority. It is a theory of objective truth – truth as the correspondence of a statement with the facts – and of absolute truth: if an unambiguously formulated statement is true in one language, then any correct translation of it into any other language is also true. This theory is the great bulwark against relativism and all fashions. And it allows us to speak of falsity and its elimination; of our fallibility; and of the fact that we can *learn from our errors,* from our mistakes; and of science as the search for truth. Moreover, it allows us – indeed, it requires us – to distinguish clearly between *truth* and *certainty.* I vividly remember, in spite of my bad memory, some of my conversations in Prague with Alfred Tarski and Janina Hosiasson and I vividly remember her surprise, if not horror, at my rejection of probabilistic induction, a field in which she had been working for several years. She gave me some of her papers to read, and I found them far better and far more responsibly argued than Reichenbach's theory. I decided that I must try to attend to her work with the greatest care and, if possible, find a way of reconciling her results with mine; or else to see whether her arguments could be used to refute mine. I soon found that this was not possible: and that a probabilistic theory of induction would work no better on her lines than on Reichenbach's. Reichenbach, incidentally, was also in Prague; but when Carnap tried to introduce me to him he refused to talk to me or even to shake hands. Of other participants I remember, of course, Otto Neurath, Rudolf Carnap and Philipp Frank, with all of whom I was on very friendly terms in spite of my opposition to the positivism of the Circle. Schlick, I believe, came a few days later. I can no longer remember whether Waismann and Zilsel were present.

In the course of the Preliminary Conference, Reichenbach read a paper on probabilistic induction and I replied. My reply was printed with his paper in the journal *Erkenntnis;* and it was reprinted 25 years later in the English translation of my *Logik der Forschung* (and also in its second German edition) under the title 'On the so-called "Logic of Induction" and the "Probability of Hypotheses"'.

Carnap was then, and for some years afterwards, entirely on my side, especially concerning induction (and also concerning Reichenbach's personal attitude towards me and my book); and when my book was published 3 months later, he not only wrote a most favourable review in *Erkenntnis,* which he and Reichenbach were editing together, but he defended himself and me when Reichenbach published in the same issue a long attack on me and a critical aside against Carnap's review.

Carnap and I had come, in those days, to something like an agreement on a common research programme on probability, based on my *Logik der Forschung;* we agreed to distinguish sharply between, on the one hand, probability as it is used in the probabilistic hypotheses of physics, especially of quantum theory, which satisfies the mathematical 'calculus of probabilities', and, on the other hand, the so-called probability of hypotheses, or their degree of confirmation or (as I now prefer to call it) their degree of corroboration; and we agreed *not* to assume, without strong arguments, that the degree of confirmation or corroboration of a hypothesis satisfies the calculus of probabilities, but to regard this question, in view of my arguments in *Logik,* as open – indeed, as the central problem.

This was the state of our discussion reached in 1934 and 1935. But 15 years later Carnap sent me his new big book, *Logical Foundations of Probability,* and, opening it, I found that his explicit starting point in this book was the precise opposite – the bare, unargued assumption that degree of confirmation is a probability in the sense of the probability calculus. I felt as a father must feel whose son has joined the Moonies; though, of course, they did not yet exist in those days.

However, I still could comfort myself with the consideration that Carnap had not given up truth in its objective and absolute sense, as defended by Tarski. Indeed, he never did.

It is this view of truth that gives Gödel's important results their non-relativistic sense. It also gives my results their non-relativistic sense; despite what many people say.

Ladies and Gentlemen, please take these remarks as an expression of my gratitude to Alfred Tarski and as a confession of faith: of my opposition to relativism and of my 54-year-long adherence to the Aristotelian theory of truth, rehabilitated by Tarski and successfully applied by him and by Gödel to some mathematical problems. And I wish to add to this confession of faith my unshaken conviction that, next to music and art, science is the greatest, most beautiful and most enlightening achievement of the human spirit. I abhor the at present so noisy intellectual fashion that tries to denigrate science, and I greatly admire the marvellous results achieved in our time by the work of biologists and biochemists and made available through medicine to sufferers all over our beautiful earth.

Admittedly, science suffers from our human fallibility, like every other human enterprise. Even if we are doing all we can in order to discover our mistakes, our results cannot be certain, and they may not even be true. But we can *learn* from our mistakes: great scientists have shown us how to turn our fallibility into objectively testable *conjectural knowledge*. They are continuing to do so at this moment.

All I have said so far has been an attempt to introduce myself to you as a valiant lover of science who has the greatest admiration for the marvellous and often true results of science without believing these results to be *certain*. The results of science remain hypotheses that may have been well *tested*, but not *established:* not *shown* to be *true*. Of course, they *may* be true. But even if they fail to be true, they are splendid hypotheses, opening the way to still better ones.

Our theories, our hypotheses, are our adventurous trials. Admittedly, most of them turn out to be errors: under the impact of our tests their falsity may be revealed. Those theories that we *cannot* refute by the severest tests, we *hope* to be true. And, indeed, they *may be true;* but new tests may still falsify them.

This method of bold, adventurous theorizing, followed by exposure to severe testing is the method of life itself as it evolves to higher forms: it is the method of trials and of the exposure and elimination of errors through tests. Just as life conquers new worlds, new lands, the ocean, the air and space, so science conquers new worlds: new lands, the ocean, the air and space. What we aim to know, to understand, is the world, the cosmos. All science is cosmology. It is an attempt to learn more about the world. About atoms, about molecules. About living organisms and about the riddles of the origin of life on earth. About the origin of thinking, of the human mind; and about the ways in which our minds work.

These are great tasks; almost impossible tasks. But scientists have made almost impossible progress in their bold attempts. I have been most fortunate indeed, throughout my life, to witness some of these attempts from a distance and others even from close quarters; and I have sometimes even participated in the adventure, in the fields of quantum physics and biology.

I now come to my central problem – causality and the change of our view of the world. Up to about 1927 physicists, with few exceptions, believed that the world was a huge and highly precise clockwork. Descartes, the great French philosopher, physicist and physiologist, described the clockwork as mechanical: all causation was push. It was the first, and the clearest, theory of causation. Later, from about 1900 on, the world was regarded as an electrical clockwork. But in both cases it was regarded as an *ideally precise* clockwork. Either the cog-wheels pushed each other, or the electromagnets attracted and repelled each other with absolute precision. There was, in this world, no room for human decisions. Our feelings that we are acting, planning, and understanding each other were illusory. Few philosophers, with the great exception of Peirce, dared to dispute this deterministic view.

But starting with Werner Heisenberg in 1927, a great change occurred in quantum physics. It became clear that minute processes made the clockwork imprecise: there were *objective indeterminacies*. Physical theory had to bring in probabilities.

It was here that I had some severe disagreement with Heisenberg and other physicists, even with my hero, Einstein. For most of them adopted the view that the probabilities had to do with our *lack of knowledge* and, therefore, with our state of mind; they adopted a *subjectivist* theory of probability. In opposition to this, I wished to adopt an *objectivist* theory. This led me to a cluster of largely mathematical problems; problems whose allurement remains with me to this very day.

The mathematical theory of probability deals with such things as throwing dice and tossing pennies, or estimating your expectation of life – perhaps for insurance purposes. How probable is it that you will live another 20 years? This has its own little mathematical problems. Thus, the probability that you will live another 20 years from today – that is that you will still be alive in 2008 – increases for most of you every day and every week as long as you survive, until it reaches the probability 1 on the 24th of August 2008. Nevertheless, the probability that you will survive for another 20 years from any of the days following today goes down and down with every day and every week you live, with every sneeze and with every cough; and unless you die by some accident, it is not unlikely that *this* probability will become close to 0 years before your actual death. Of course, you know that 0 is the lowest possible probability and 1 the highest; and that 1/2 is the probability of an event that may happen or, just as easily, not happen, such as in tossing an unbiased coin where the probability of *'heads turning up'* is equal to the probability of *'tails turning up'*, and each of these events has the probability of 1/2.

Mathematical probability theory, as you may know, plays an important role in quantum physics and, indeed, in all sciences. I have worked on at least seven different problems of probability theory since my introduction to the subject at university. And it was only after decades that I came to satisfactory and very simple solutions. One of these solutions was what I call *'the propensity interpretation of probability'*. I published it first only in 1956, after more than 35 years of study. This theory has further grown so that it was only in the last year that I realized its cosmological significance. I mean the fact that we live in *a world of propensities*, and that this fact makes our world both more interesting and more homely than the world as seen by earlier states of the sciences.

Now let me explain briefly *the propensity interpretation of probability*. For this purpose I will go back to the tossing of coins.

The classical theory of probability erected a powerful system upon the following definition: 'The probability of an event is the number of the favourable *possibilities* divided by the number of all the equal *possibilities.*' Thus, the classical theory was about mere *possibilities;* and the probability of the event *'tails turning up'* would be 1 divided by 2 because there are altogether two equal possibilities and only one is 'favourable' to the event 'tails'. The other possibility is *not* favourable to *'tails'*. Similarly, the possibility of throwing an even number smaller than 6 with a perfect die is 2 divided by 6 which, of course, is the same as 1/3. For there are 6 sides and therefore 6 equal possibilities and only two of these possibilities, that is the sides marked 2 and 4, are favourable to the event 'an even number smaller than 6 turning up'.

But what happens if the die is loaded or if the penny is biased? Then, according to the classical theory – say, at the time of Pascal or at the time of Laplace – we can no longer say that the six possibilities of the die, or the two possibilities of the coin, are *equal possibilities*. Accordingly, since there *are* no equal possibilities in such a case, we simply cannot speak here of probabilities in the classical numerical sense.

Of course, Pascal knew that loaded dice had been invented for cheating at gambling. In fact, everybody knew that if you insert in a wooden die a small piece of lead near, say, the face bearing the number 6, then this number will turn up less frequently than it does in throws with a fair die, and so the number on the opposite face will turn up more frequently. There are still the six possibilites; but they are now not *equal* possibilities but *loaded* or *weighted* possibilities; possibilities that may be unequal and whose inequality or different weight may be assessed, possibilities that may indeed be weighed.

It is clear that a more general theory of probability ought to include such *weighted* possibilities. It is even clear that cases of *equal* possibilities could and should be treated as special cases of weighted possibilities: obviously, equal possibilities can be regarded as weighted possibilities whose weights happen to be equal.

So the idea of weighted possibilities is fundamental for a more general theory of probability. It is needed even for a more general theory of gambling, of games of chance. But what is far more important is that it is needed for all the sciences – for physics, for biology, and for such questions as the probability of surviving a certain number of years. These cases are all very different from and more general than those cases of gambling with strictly homogeneous and symmetrically built dice or pennies or roulette wheels.

But there is no insuperable difficulty in this generalization: it is easy to see that in the absence of equal possibilities, we may still be able to say that certain possibilities and probabilities are greater, or weightier, than others, as in the case where a die is loaded.

The main problem that arises is this: Does there exist a method – or an instrument like a pair of scales – that can help us to find out the actual weight of the weighted possibilities? Does there exist a method that allows us to attribute numerical values to possibilities that are unequal?

The obvious answer is: *yes,* a statistical method; *yes,* provided we can, as in the case of dicing, repeat the situation that produces the probabilistic events in question; or provided (as in the case of sunshine or rain) the events in question repeat themselves, without our interference. Provided the number of such repetitions is sufficiently large, we can use statistics as a method of weighing the possibilities, and of measuring their weights. Or, to be a little more explicit, the greater or smaller *frequency of occurrences* may be used as a test of whether a hypothetically attributed weight is, indeed, an adequate hypothesis. To put it more crudely, we take the frequency of occurrence as measuring the weight of the corresponding possibility, so that we say that the probability of a rainy Sunday in June in Brighton equals 1/5 if and only if it has been found over many years that, *on average,* there will be rain on one out of five Sundays in June. So we use statistical averages in order to estimate the various weights of the various possibilities.

All this is, I believe, simple and straightforward. But the really important points come now.

(1) If what I have said is true – if we can measure the weight of the possibility of *'two turning up'* in throwing a certain loaded die, and find it to be only 0.15 instead of $0.1666 = 1/6$ – then there must be inherent in the structure of throws with this die (or with a sufficiently similar die) *a tendency or propensity* to realize the event *'two turning up'* that is smaller than the tendency shown by a fair die. Thus, my first point is that a tendency or propensity to realize an event is, in general, *inherent in every possibility* and in every single throw, and that we can estimate the measure of this tendency or propensity by appealing to the relative frequency of the actual realization in a large number of throws; in other words by finding out how often the event in question actually occurs.

(2) So, instead of speaking of the *possibility* of an event occurring, we might speak, more precisely, of an inherent *propensity* to produce, upon repetition, a certain statistical average.

(3) Now this implies that, upon further repetition – upon repetition of the repetitions – that the statistics, in their turn, do show a tendency towards *stability*, provided all relevant conditions remain stable.

(4) Just as we explain the tendency or propensity of a magnetic needle to turn (from any initial position it may have assumed) towards the north by (a) its inner structure, (b) the invisible field of forces carried with it by our planet, and (c) friction, etc. – in short, by the invariant aspects of the physical *situation*; so we explain the tendency or propensity of a sequence of throws with a die to produce (from any starting sequence) stable statistical frequencies by (a) the inner structure of the die, (b) the invisible field of forces carried with it by our planet, and (c) friction, etc. – in short, by the invariant aspects of the physical *situation*: the field of propensities that influences every single throw.

The tendency of statistical averages to remain stable if the conditions remain stable is one of the most remarkable characteristics of our universe. It can be explained, I hold, only by the propensity theory; by the theory that there exist weighted possibilities which are *more than mere possibilities*, but tendencies or propensities to become real: tendencies or propensities to realize themselves which are inherent in all possibilities in various degrees and which are something like forces that keep the statistics stable.

This is an *objective interpretation of the theory of probability*. Propensities, it is assumed, are not mere possibilities but are physical realities. They are as real as forces, or fields of forces. And vice versa: forces are propensities. They are propensities for setting bodies in motion. Forces are propensities to accelerate, and fields of forces are propensities distributed over some region of space and perhaps changing continuously over this region (like distances from some given origin). Fields of forces are fields of propensities. They are real, they exist.

Mathematical probabilities are measures that take on numerical values from 0 to 1. 0 is usually interpreted as impossibility, 1 as certainty, 1/2 as complete indeterminacy, and values between 1/2 and 1 – say 7/10 – are interpreted as 'more probable than not'.

Physical propensities may be interpreted somewhat differently. The propensity 1 is the special case of a classical force in action: a cause when it produces an effect. If a propensity is less than 1, then this can be envisaged as the existence of competing forces pulling in various opposed directions but not yet producing or controlling a real process. And whenever the possibilities are discrete rather than continuous, these forces pull towards distinct possibilities, where no compromise possibility may exist. And zero propensities are, simply, no propensities at all, just as the number zero means 'no number'. (If I tell an author that I have read a number of his books and have to admit that the number is zero, then I was misleading him: I have read *none* of his books. Similarly, a propensity zero means *no* propensity.) For example, the propensity of getting the number 14 on the next throw with two ordinary dice is *zero:* there exists no such possibility and therefore no propensity.

Forces in the modern sense were introduced into physics and cosmology by Isaac Newton who, of course, had some predecessors who were feeling their way towards this idea, notably Johannes Kepler. The introduction of forces by Newton was a great success, even though it was opposed by those who do not like invisible or hidden or 'occult' entities in physics. Indeed, Bishop Berkeley may be said to have founded the positivist philosophy of science by attacking Newton for introducing invisible entities and 'occult qualities' into nature. And he was followed in this especially by Ernst Mach and Heinrich Hertz. But Newton's theory of forces – especially of attractive forces – had tremendous explanatory power. And it was further developed and extended, especially by Ørsted, Faraday and Maxwell, and then by Einstein, who tried to *explain* the Newtonian forces in their turn by his theory of curved spacetime.

The introduction of propensities amounts to generalizing and extending the idea of forces again. Just as the idea of forces was opposed by the positivist successors of Berkeley, of Mach, and of Hertz, so the idea of propensities is again rejected by some people as introducing into physics what Berkeley had called 'occult qualities'.

Others have accepted my theory of propensities or objective probabilities, but have (somewhat rashly, I think) tried to improve upon it. I had stressed that propensities should not be regarded as properties *inherent in an object,* such as a die or a penny, but that they should be regarded as *inherent in a situation* (of which, of course, the object was a part). I asserted that the situational aspect of the propensity theory was important, and decisively important for a realist interpretation of quantum theory.

But I was criticized by some people who asserted that the propensities of 1/2 and of 1/6 were intrinsic symmetry properties of a coin or a die, and that the propensity for surviving another year, or 20 more years, was an intrinsic property of the constitution of a man's or a woman's body and his or her state of health. And, as a strong argument, one of my critics appealed to the survival tables of the life insurance companies which, admittedly, seem to incorporate this view.

Nevertheless, the view that the propensity to survive is a property of the state of health *and not* of the situation can easily be shown to be a serious mistake. As a matter of course, the state of health is very important – an important aspect of the situation. But as anybody may fall ill or become involved in an accident, the progress of medical science – say, the invention of powerful new drugs (like antibiotics) – changes the *prospects* of everybody to survive, whether or not he or she actually gets into the position of having to take any such drug. The *situation* changes the possibilities, and thereby the propensities.

I think that this is a perfect counterexample, and that no more needs to be said. Nevertheless, the example may be a little amplified. The new invention may be expensive, at least in the beginning, which may make it clear that not only the intrinsic state of health of a person may count but also the state of his purse, or else the purse of a possible health service and, obviously, the quality of its medical men.

Incidentally, in my first publication on propensities I pointed out that the propensity of a penny to fall on a flat table with heads up is obviously modified if the table top is appropriately slotted. Similarly, one and the same *loaded* die will have different propensities if the table top is very elastic rather than of marble, or if it is covered by a layer of sand.

Of course, every experimental physicist knows how much his results depend on circumstances like temperature or the presence of moisture. But some typical experiments measure propensities fairly directly; for example, the Franck-Hertz experiment measures how the propensity of electrons to interact with gas atoms changes almost discontinuously with the rising voltage of the electrons.

The Franck-Hertz experiment, one of the classic experiments of quantum theory, studies the dependence of this interaction on an increasing voltage. As the voltage rises, the intensity of the current of electrons rises slowly and then, suddenly, falls; it rises again slowly to a still higher level and falls again suddenly. This is interpreted as the result of the single electrons reaching, step by step, the discrete excitation states of the gas atoms. Here the change of the voltage – of an external condition – is the decisive independent variable; and the changing propensities of the electrons and the atoms to interact with each other are recorded, as they depend upon the changing voltage.

For this kind of experiment – and many atomic experiments are of this kind – we need *a calculus of relative or conditional probabilities* as opposed to *a calculus of absolute probabilities* as it may suffice for, say, dicing experiments or for some statistical problems (say, life insurance tables).

A statement in the absolute calculus may be written

$$(1) \qquad p(a) = r$$

to be read: 'The probability of the event a equals r.' (Here r stands for a real number, $0 \leq r \leq 1$). This contrasts with the relative or conditional probability statement

$$(2) \qquad p(a, b) = r$$

to be read: 'The probability of the event a in the situation b (or given the conditions b) equals r'.

If we are interested in a situation that does not change (or whose changes we may neglect), then we can work with absolute probabilities or absolute propensities, having once and for all described the conditions. Thus, if you state that the probability of a (e.g. of a certain kind of radioactive atom decaying within a year) is one hundred times greater than that of b (e.g. of another kind of atom decaying), you will assume constant and stable conditions for both a and b (and not, for example, that one of these atoms is part of a crystal exposed to radiation by slow neutrons).

But in the Franck-Hertz experiment we are interested in the dependence of the propensity upon conditions that change, indeed, in a definite way (with the voltage *slowly increasing*).

One important aspect of the Franck-Hertz experiment which it shares with many other quantum experiments is that, even though the conditions change, we can *measure* the propensities because there are so many electrons involved: for statistical measurement, the large number of electrons serves extremely well to replace long sequences of repetitions. But in many kinds of events this is not the case, and the propensities cannot be measured because the relevant situation changes and cannot be repeated. This would hold, for example, for the different propensities of some of our evolutionary predecessors to give rise to chimpanzees and to ourselves. Propensities of this kind are, of course, not measurable, since the situation cannot be repeated. It is unique. Nevertheless, there is nothing to prevent us from supposing that such propensities exist, and from estimating them speculatively.

To sum up: propensities in physics are properties of *the whole physical situation* and sometimes even of the particular way in which a situation changes. And the same holds of the propensities in chemistry, in biochemistry, and in biology.

Now, in our real changing world, the situation and, with it, the possibilities, and thus the propensities, change all the time. They certainly may change if we, or any other organisms, *prefer* one possibility to another; or if we *discover* a possibility where we have not seen one before. Our very understanding of the world changes the conditions of the changing world; and so do our wishes, our preferences, our motivations, our hopes, our dreams, our phantasies, our hypotheses, our theories. Even our erroneous theories change the world, although our correct theories may, as a rule, have a more lasting influence. All this amounts to the fact that *determinism is simply mistaken:* all its traditional arguments have withered away and indeterminism and free will have become part of the physical and biological sciences.

The theory of motives determining our actions, and the theory that these motives in their turn are motivated or caused or determined by earlier motives, etc., seems, indeed, to be motivated – motivated by the wish to establish the ideology of determinism in human concerns. But with the introduction of propensities, the ideology of determinism evaporates. Past situations, whether physical or psychological or mixed, do not determine the future situation. Rather, they determine changing *propensities that influence future situations without determining them in a unique way.* And all our experiences – including our wishes and our efforts – may contribute to the propensities, sometimes more and sometimes less, as the case may be. (In spite of the instability of the weather, my wishes do not contribute to 'sunshine tomorrow'. But they can contribute a lot to my catching the flight from London to San Francisco.)

In all these cases the propensity theory allows us to work with an *objective* theory of probability. Quite apart from the fact that we do not *know* the future, the future is *objectively not fixed.* The future is *open: objectively open.* Only the past is fixed; it has been actualized and so it has gone. The present can be described as the continuing process of the actualization of propensities; or, more metaphorically, of the freezing or the crystallization of propensities. While the propensities actualize or *realize* themselves, they are continuing processes. When they have realized themselves, then *they are no longer real processes.* They freeze and so become past – and unreal. Changing propensities are objective processes, and they have nothing to do with our lack of knowledge; even though our lack of knowledge is, of course, very great, and even though a particular lapse may, of course, be an important part of the changing situation.

Propensities, like Newtonian attractive forces, are invisible and, like them, they can act: they are *actual,* they are *real.* We therefore are compelled to attribute a kind of reality to mere possibilities, especially to weighted possibilities, and especially to those that are as yet unrealized and whose fate will only be decided in the course of time, and perhaps only in the distant future.

This view of propensities allows us to see in a new light the processes that constitute our world: the world process. The world is no longer a causal machine – it can now be seen as a world of propensities, as an unfolding process of realizing possibilities and of unfolding new possibilities.

This is very clear in the physical world where new elements, new atomic nuclei, are produced under extreme physical conditions of temperature and pressure: elements that survive only if they are not too unstable. And with the new nuclei, with the new elements, new possibilities are created, possibilities that previously simply did not exist. In the end, we ourselves become possible.

The world of physics is, we have known for some time, indeterministic. It was long regarded to be deterministic. And then, after quantum indeterminism was accepted, indeterminism was usually regarded as affecting only the tiniest bodies, such as radioactive atoms, and only a very little. But this, it turned out, was a mistake. We now know that not only tiny particles are affected but also the probability of chemical reactions, and thus, of classical mass effects. It has now become clear, especially through the findings of the Japanese chemist, Kenichi Fukui, that unoccupied frontier orbitals play an important part in chemical reactions. But these are just unrealized possibilities – presumably empty de Broglie waves. In any case, they are propensities, similar to attractive forces.

Let us have a quick look at chemical evolution. Especially in the evolution of biochemistry, it is widely appreciated that every new compound creates new possibilities for further new compounds to synthesize: possibilities which previously did not exist. The possibility space (the space of non-zero possibilities) is growing. (Incidentally, *all* spaces are possibility spaces.)

And behind this growth there seems to be hidden something like a natural law that can be stated as follows: All non-zero possibilities, even those to which only a tiny non-zero propensity is attached, will realize themselves in time, provided they have time to do so; that is to say, provided the conditions repeat themselves sufficiently often or remain constant over a sufficiently long period of time. This law amounts to stating that there is a kind of *horror vacui* in the various possibility spaces (perhaps a kind of horror of empty de Broglie waves, so that the propensities are like *active* attractive forces).

Just like a newly synthesized chemical compound, whose creation in turn creates new possibilities for new compounds to synthesize, so all new propensities always create new possibilities. And new possibilities tend to realize themselves in order to create again new possibilities. Our world of propensities is inherently creative.

These tendencies and propensities have led to the emergence of life. And they have led to the great unfolding of life, to the evolution of life. And the evolution of life has led to better conditions for life on earth and thus to new possibilities and propensitites; and to new forms of life that differ widely from the old forms and from each other. All this means that possibilities – possibilities that have not yet realized themselves – have a kind of reality. The numerical propensities that attach to the possibilities can be interpreted as a measure of this status of a not yet fully realized reality – a reality in the making. And in so far as these possibilities can, and partly will, realize themselves in time, the open future is, in some way, already present, with its many competing possibilities, almost as a promise, as a temptation, as a lure. The future is, in this way, *actively* present at every moment.

The old world picture that puts before us a mechanism operating with pushes, or with more abstract causes that are all in the past – the past kicking us and driving us with kicks into the future, the past that is *gone* – is no longer adequate in our indeterministic world of propensities. Causation is just a special case of propensity: the case of a propensity equal to 1, a *determining* demand, or force, for realization. It is not the kicks from the back, from the past, that *impel* us but the attraction, the lure of the future and its competing possibilities, that *attract* us, that *entice* us. This is what keeps life – and, indeed, the world – unfolding. (Remember that Newtonian forces too are attractive forces!)

I now turn to *causation*. In the light of what has been said about propensities, two comments will be made on causation; comments that appear to me new.

The first is a comment on the deterministic push theory of causation. In Plato and Aristotle, movement is something that needs an explanation: it is explained by a mover. This idea is clarified and elaborated in Descartes's clockwork theory of the world. The world is a mechanical clockwork in which a cog of one cog-wheel pushes the adjacent cog of the adjacent cog-wheel. Since the wheels are perfect, there is no loss of movement. The first mover is the first cause, and *all causation is push*. Newton was still thinking on these lines and therefore made an attempt, as he hints in the *Optics*, to reduce the attractive pull to push. But in contrast to Lesage, he realized that a theory of the Lesage type would not work. So Descartes's monistic push theory of causation gave way to a *push-me-pull-you* theory: shocking at first, even to Newton himself, but still highly intuitive, even for a poet such as Pope.

Faraday and Maxwell prepared for the electrification of the world-clock. Push is no longer symmetrical with pull, and Ørsted forces play a most important additional role. But these Ørsted forces are not central and therefore they really destroy the intuitive character of the push-me-pull-you world. Physics becomes now abstract: Ørsted forces make a field theory inevitable. And so the new physics was called 'theoretical physics' or *'Theoretische Physik'*; first, I think, in Berlin, in the Helmholtz circle. It tried to describe the abstract, the hidden, the invariant structural properties of the physical world. *Cause* became that state of affairs which, *relative to an accepted theory,* was described by the initial conditions. *Effect* was that event or that state of affairs which the theory, in the presence of the initial conditions, would predict.

Owing to this deductive relationship, it is trivial that, in the presence of the theory, the probability of the effect given the cause equals 1:

$$p\ (\textit{effect, cause}) = 1$$

This, I say, is trivial. But it leads in our world of propensities to the following view. What may happen in the future – say, tomorrow at noon – is, to some extent, open. There are many possibilities trying to realize themselves, but few of them have a very high propensity, given the existing conditions. When tomorrow noon approaches, under constantly changing conditions, many of these propensities will have become zero and others very small; and some of the propensities that remain will have increased. At noon, those propensities that realize themselves will be equal to 1 in the presence of the then existing conditions. Some will have moved to 1 continuously; others will have moved to 1 in a discontinuous jump. (One can therefore still distinguish between *prima facie* causal and acausal cases.) And although we may regard the ultimate state of the conditions at noon as the cause of the ultimate realization of the propensities, there is nothing of the old Cartesian deterministic push left in this view of the world.

This is my first comment on causation in the light of the theory of propensities. But to complement the first, a second comment is needed.

In our theoretical physics, that is in our somewhat abstract description of the invariant structural properties of our world, there are what we may call natural laws of a deterministic character and others that we may call natural laws of a probabilistic character, like those described by Franck and Hertz. Let us first look at the deterministic laws – say, Kepler's laws, since they are still valid in Einstein's theory for not too excentric planetary ellipses; or, say, Bohr's wonderful 1921 theory of the periodic system.

What is the status of this kind of theory that describes the structural properties of our world?

They are hypotheses, arrived at in (often unsuccessful) attempts to solve some problems such as Kepler's great problem to find the secrets of the *'Harmony of the Universe'*, or Bohr's problem of explaining the periodic system of elements in terms of his theory of the electrons surrounding the Rutherford nuclei. That they were wonderful hypotheses, I wish to stress, in full admiration of the great achievement of these masters. Yet that they were not more than hypotheses we know from the fact that Kepler's laws were corrected by Newton and Einstein, and that Bohr's theory was corrected by the theory of isotopes.

Being hypotheses, these theories had to be tested. And it was the close agreements with the tests that gave them their great importance.

Now, how are such theories tested? Obviously by making experiments. And this means: *by creating, at will, artificial conditions that either exclude, or reduce to zero, all the interfering and disturbing propensities.*

Only the system of our planets is so well isolated from all extraneous mechanical interference that it is a unique, natural laboratory experiment. Here, only the *internal* disturbances interfere with the precision of Kepler's laws. Kepler knew nothing of these problems, for example of the insolubility of the three-body problem, and it was one of the glories of Newton's theory that he invented an approximation method of solving them. He tamed, up to a point, the disturbing propensities of the planets for interfering with each other.

In most laboratory experiments we have to exclude many disturbing extraneous influences such as changes of temperature or the normal moisture of the air. Or we may have to create an artificial environment of extreme temperatures – say, near to absolute zero. In this we are led entirely by our hypothetical insight into the theoretical structure of our world. And we have to learn from our experimental mistakes that lead to unsatisfactory results: results are satisfactory only if they can be repeated at will; and this happens only if we have learnt how to exclude the interfering propensities.

But what does all this show us? It shows that in the non-laboratory world, with the exception of our planetary system, no such strictly deterministic laws can be found. Admittedly, in certain cases such as the planetary movements, we can interpret events as due to the vectorial sum of forces that our theories have isolated. But in any actual event such as, say, the fall of an apple from a tree, this is not the case. Real apples are emphatically not Newtonian apples. They fall usually when the wind blows. And the whole process is initiated by a biochemical process that weakens the stem so that the often-repeated movement due to the wind, together with the Newtonian weight of the apple, leads to a snap of the stem – a process that we can analyse but cannot calculate in detail, mainly because of the probabilistic character of the biochemical processes that prevents us from predicting what will happen in a unique situation. What we might be able to calculate is the propensity of a special type of apple to fall within, say, the next hour. This *may* make it possible for us to predict that, if the weather deteriorates, it will very probably fall within the next week. There is no determinism in Newton's falling apple if we look at it realistically. And much less in many of our changing states of mind, for example in our so-called motives. Our inclination to think deterministically derives from our acts as movers, as pushers of bodies: from our Cartesianism. But today this is no longer science. It has become ideology.

All this is now supported by the new results of the mathematics of dynamic (or deterministic) chaos.

This new theory has shown that, even on the assumption of a classical (or 'deterministic') system of mechanics, we may obtain, from some special but quite simple initial conditions, motions that are 'chaotic', in the sense that they quickly become completely *unpredictable*. As a consequence, we can now easily explain such facts within classical 'deterministic' physics as the molecular chaos of every gas. We neither need to *assume* them, nor do we need to call upon quantum physics for their derivation.

This argument seems to me valid. But an interpretation that is sometimes linked with it seems to me invalid. It says that we may – or that we should – assume that our world is in reality deterministic, even where it appears to be indeterministic or chaotic; that behind an indeterministic appearance, there lies hidden a deterministic reality. This interpretation I regard as a mistake. For what has been established is that classical physics is only seemingly (or *prima facie*) deterministic; that its determinism fits only problems of a special kind, such as the Newtonian two-body problem, while it turns out to be indeterministic if problems of a wider range are taken into consideration. (This view I have upheld at least since 1950; compare my paper 'Indeterminism in Quantum Physics and in Classical Physics' (BJPS 1950) and my book, *The Open Universe* (1982) with an interpretation of some important results by Hadamard.)

To sum up, neither our physical world nor our physical theories are deterministic, even though of course many possibilities are excluded by the laws of nature and of probability: there are many zero propensities. And even non-zero propensities that are very small will not realize themselves if the situation changes before they had a chance. The fact that conditions are never quite constant may, indeed, explain why certain very low propensities seem never to realize themselves. Shaking the beaker in dicing is intended to make the throws independent of each other. But it may indeed do more: it may disturb that constancy of the physical conditions which is a mathematical condition for very low propensities to realize themselves. This may perhaps explain the claim of some experimenters that *a priori* extremely improbable runs occur in fact even less often than they should according to theory. We cannot ensure that all the probabilistically relevant conditions are really kept constant.

The future is open. It is especially obvious in the case of the evolution of life that the future always was open. It is obvious that in the evolution of life there were almost infinite possibilities. But they were largely exclusive possibilities; so most steps were exclusive choices, destroying many possibilities. As a consequence, only comparatively few propensities could realize themselves. Still, the variety of those that have realized themselves is staggering. I believe that this was a process in which both *accidents* and *preferences,* preferences of the organisms for certain possibilities, were mixed: the organisms were in search of a better world. Here the preferred possibilities were, indeed, allurements.

Looking at my own long life, I find that the main allurements which led me on and on from my 17th year were *theoretical problems*. And among these the problems of science and of probability theory loomed large. These were *preferences*. The solutions were *accidents*.

A brief closing passage from the preface to a book of mine may apply all this to the education of young scientists.

> I think that there is only one way to science – or to philosophy, for that matter: to meet a problem, to see its beauty and fall in love with it; to get married to it and to live with it happily, till death do ye part – unless you should meet another and even more fascinating problem or unless, indeed, you should obtain a solution. But even if you do obtain a solution, you may then discover, to your delight, the existence of a whole family of enchanting, though perhaps difficult, problem children, for whose welfare you may work, with a purpose, to the end of your days.

Towards an Evolutionary Theory of Knowledge