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Physics and Relativity

by MAX BORN (Bad-Pyrmont)

I have been honoured by being asked to give the address on Physics and Relativity in place of NIELS BOHR who was prevented to come to Bern.

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 presumptious and quite beyond my power to summarise 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 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 aether 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, later called ABRAHAM-FÖPPL, now ABRAHAM–BECKER, was our main source of information. All this 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 preceeding 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 by-gone 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, FITZ GERALD, 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 in 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's 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 aether absolutely at rest, a kind of materialisation 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 coordinates x, y, z and the time t were simultaneously transformed into new parameters x', y', z', t' he called these 'local coordinates' and 'local time'. These transformations, for which POINCARÉ later introduced the term LORENTZ transformations, where 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 FITZ GERALD invented in 1892 the *contraction hypothesis* which was at once taken up by LORENTZ and included into 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 1^{st} 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 POINCARÉS 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, USA., and he predicted the rise of a new mechanics which will be characterised 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 1000.000 Marks for the proof of FERMAT's celebrated 'Great Theorem'. Hundreds of letters, or even just postcards, 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, 22^{nd} –

28th April 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 5 lectures dealt with purely mathematical problems; the 6th 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 the same, 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 gives you definitely the impression that he is recording LORENTZ's 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 priviledged 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 appears 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's 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 on 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 LORENTZtransformations, 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 pecularities. 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 Febr. 19 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 Erscheinungen in 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 LORENTZ-Transformation 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 transformation which later were known by his name are essential, and POINCARÉ had even penetrated deeper in these connections. Concerning myself, I knew only LORENTZ's important work of 1895 (the two papers quoted above in the German text) but not LORENTZ's later work, nor the consecutive investigations by POINCARÉ. In this sense my work of 1905 was independent. The new feature of it was the realisation of the fact that the bearing of the LORENTZ-transformations transcended their 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 makes, I think, 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 (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 current 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-FITZ GERALD 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 to challenge 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 have built and which was to carry the structure, erected by MINKOWSKI. I think it wrong to forget these other men, as it happens 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 cathod 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 to the theoreticians 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-FITZ GERALD 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 selfenergy of the rigid electron, G. HERGLOTZ, P. HERTZ, A. SOMMERFELD a. o. 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 generalising 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 selfenergy 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 in a first approximation as LORENTZ's 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 however 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, than 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 VANT 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 had 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 generalised 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 role 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 EINSTEINS 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 honey moon 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 discussion 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 on 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 this led, in combination with the quantization laws of BOHR and SOMMERFELD, 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 in hydrogen canal rays in 1938 by IVES and STILVELL, 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 life time 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 for the two optical effects 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 generalised 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 have 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 extented science which 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 by-gone 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 AMPÈRES molecular currents. He was also deeply interested in quantum theory and worried by its paradoxes.

In 1919 I became v. LAUES 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 became suddenly world famous, and his theory as well as his personality the object of fanatical controversy.

Just before the war German astronomers 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 suited 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 Nov. 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 you agree... I should think it reasonable if the Jews themselves would collect money in order to give Jewish scholars financial support and teaching opportunity outside of the universities ...'

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

'Dein ausgezeichneter Artikel in der Frankfurter Zeitung hat mich sehr gefreut. Nun aber wirst Du, gerade wie ich, wenn auch in schwächerem Maßstab, 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 (Sept. 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 touched was transformed into gold, with me everything becomes newspaper noise. Suum cuique . . .'

If you are interested in that curious period when the 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 3th, 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 Jan. 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 1926 quantum mechanics was developed I hoped of course that EINSTEIN would agree, but was disappointed. Here a quotation from one of his letters (Dec. 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 allemeinen 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 discussions and letters.

During the last period of his life in Princeton he concentrated all his powers and energies to develop 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 phenomena 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 the 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 that this is tru e, if positivism is the doctrine that the purpose of science is the description of interrelations 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 tri d to do without any empirical facts, by pure thinking. He believed in theepower of reason to guess the laws according to which God has built the wo rld. 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 publish ed 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 I have unfortunately lost; but I remember a phrase like this: 'Your thundering against 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).

I saw EINSTEIN the last time about 1930, and although our correspondence continued I feel not 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 as the Scots say: 'Neirest the heart, neirest the mouth'.