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ORGANISATION EUROPÉENNE POUR LA RECHERCHE NUCLÉAIRE
CERN EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH

A TRIBUTE TO NIELS BOHR

Special Colloquium
held at CERN on 6 May 1985

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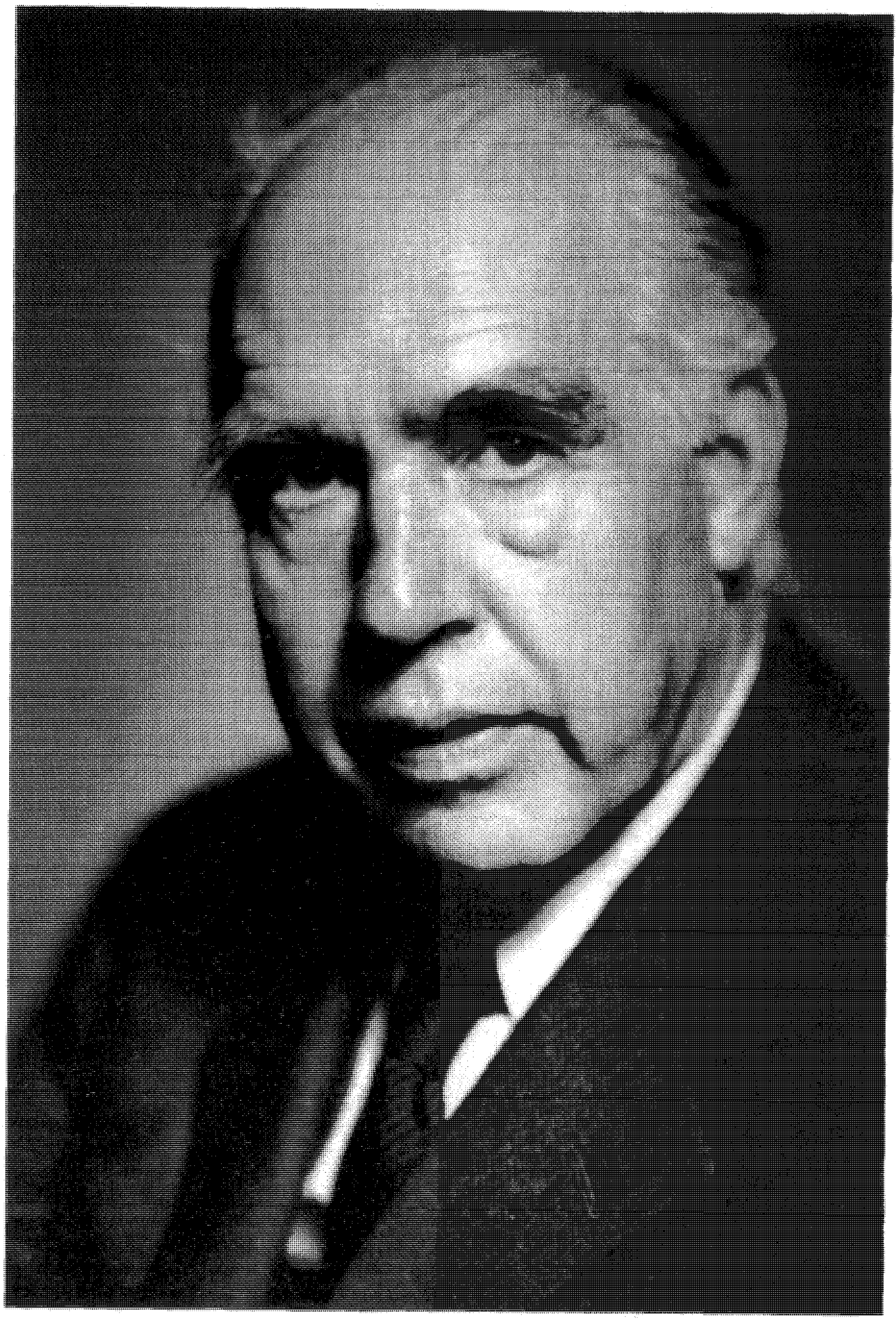
ABSTRACT

This report is organized around the contribution of A. Pais, 'Niels Bohr and the development of physics', presented at CERN on the occasion of a special colloquium in the honour of Niels Bohr.

It gives a broad survey of Bohr's life, the revolution in physics created by his quantum theory and his attempts to influence the peaceful development of nuclear energy.



Niels Bohr
1885–1962

FOREWORD

On the hundredth anniversary of his birth, physicists all over the world have been paying tribute to one of the giants of Twentieth Century Science—Niels Bohr.

On 6 May 1985, CERN paid its own special tribute. In addition to his impressive physics accomplishments, Bohr played a vital part in shaping European nuclear physics after the war and in guiding the Organization as it was coming into being.

A special colloquium surveyed both the role of Niels Bohr in the development of modern physics and his role in the creation of CERN. After Director General Herwig Schopper set the scene, Abraham Pais, Detlev W. Bronk professor at Rockefeller University, described Bohr's monumental contribution to physics, providing the first quantum picture of the atom, and his emergence as an international figure. Then former Research Director General Leon Van Hove covered Bohr's role in the events which led to the setting up of CERN in 1954, and his subsequent contribution as a key member of the Scientific Policy Committee. Symbolically, one of Bohr's last visits to CERN was on 5 February 1960, when he pushed the button at the formal inauguration of the newly completed 28 GeV Proton Synchrotron, at the time the world's most powerful particle accelerator.

This report contains only the contribution of A. Pais: 'Niels Bohr and the development of physics'. First, however, a few photographs are collected in order to show Bohr taking part in two of the principal events which marked the birth and the early years of the Organization.

I would like to thank B. Southworth for his help in the preparation of this report.

M. Jacob,
Geneva, October 1985

A tribute to Niels Bohr on the hundredth anniversary of his birth

Programme of the
Special CERN Colloquium
6 May 1985

Opening address
H. Schopper - CERN

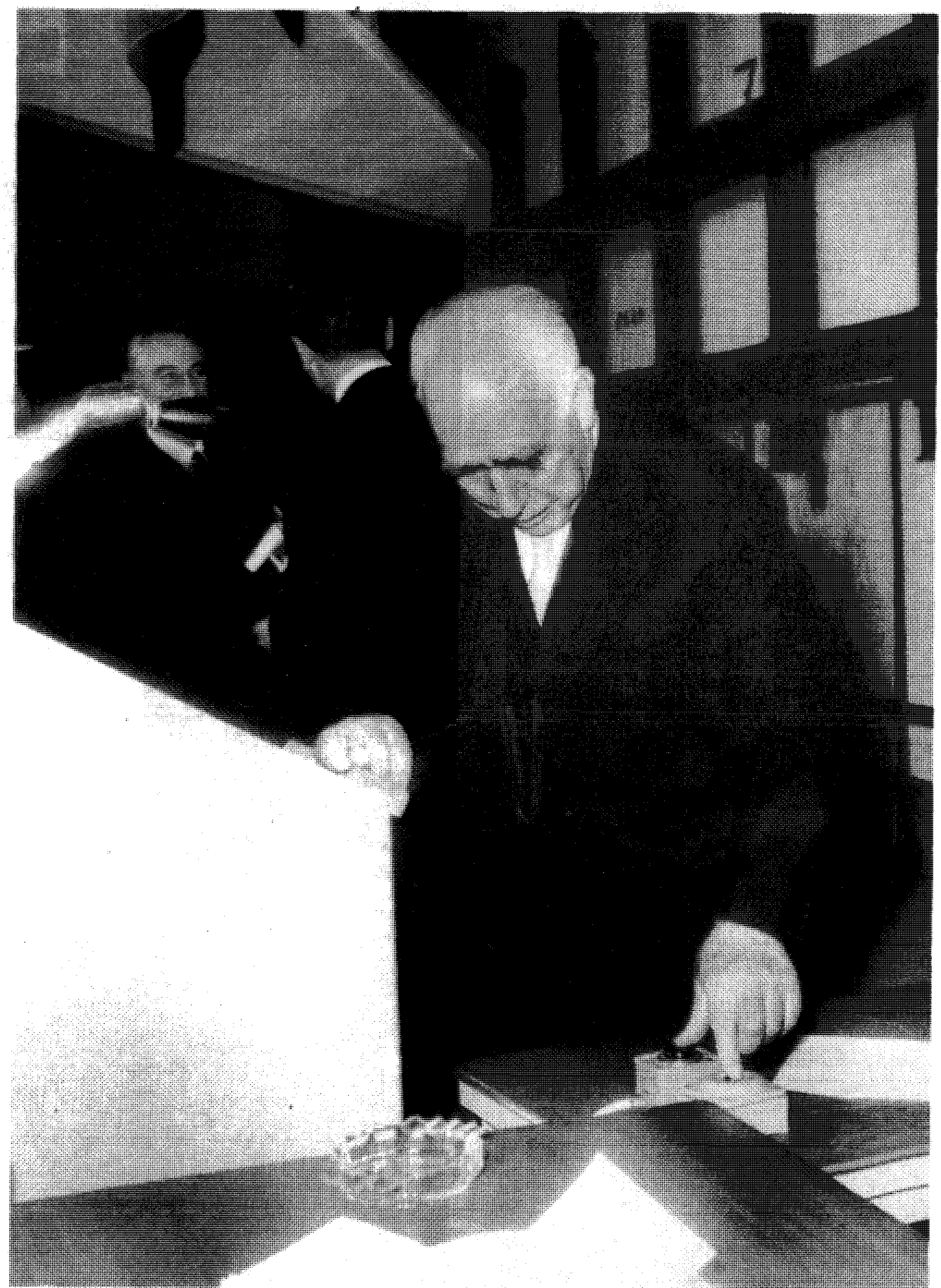
Niels Bohr and the development of physics
A. Pais - Rockefeller University

Niels Bohr and the creation of CERN
L. Van Hove - CERN

Editor M. Jacob, CERN

In February 1952, a meeting was held in Geneva to establish the provisional organization which became known as CERN (Conseil Européen pour la Recherche Nucléaire). At the meeting were, left to right, Albert Picot (a Swiss diplomat and a key advocate for bringing the CERN laboratory to Geneva), Niels Bohr, and the distinguished Swiss physicist Paul Scherrer.

Many distinguished personalities came to CERN in February 1960 for the inauguration of the Proton Synchrotron, which had come into operation a few months previously. The photograph shows Niels Bohr, Edwin McMillan and Robert Oppenheimer together for a press conference before the ceremony began.



The formal act of inauguration of the CERN Proton Synchrotron. Niels Bohr presses a button to release a bottle of champagne against a shielding block to launch the accelerator on its voyage in physics.

Following the death of Niels Bohr in 1962, a ceremony as a tribute to his contributions to physics and to CERN was held at the laboratory. Victor Weisskopf, then CERN Director General, unveiled a bust in his honour.

NIELS BOHR AND THE DEVELOPMENT OF PHYSICS

A. Pais

On 24 October 1957, Robert Oppenheimer and I took an early train to Washington. We were on our way to the Great Hall of the National Academy of Sciences, where, that afternoon, President Eisenhower was to present the first Atoms for Peace Award to Niels Bohr. It was a festive event. I recall greeting Supreme Court Justice Felix Frankfurter before the start of the proceedings. We briefly chatted about how bittersweet a moment in Bohr's life this occasion had to be. (More about that later.) Then the meeting was called to order. James Killian, president of MIT read the citation from which I quote:

> 'You have explored the structure of the atom and unlocked many of Nature's other secrets. You have given men the basis for greater contributions to the practical uses of this knowledge. At your Institute at Copenhagen, which has served as an intellectual and spiritual center for scientists, you have given scholars from all parts of the world an opportunity to extend man's knowledge of nuclear phenomena. These scholars have taken from your Institute not only enlarged scientific understanding but also a humane spirit of active concern for the proper utilization of scientific knowledge.
>
> 'In your public pronouncements and through your world contacts, you have exerted great moral force in behalf of the utilization of atomic energy for peaceful purposes.
>
> 'In your profession, in your teaching, in your public life, you have shown that the domain of science and the domain of the humanities are in reality of single realm.'

These words eloquently describe that combination of qualities we find in Bohr and only in Bohr: creator of science, teacher of science, and spokesman not only for science *per se* but also for science as a potential source for the common good.

As a creator he is one of the three men without whom the birth of that uniquely twentieth century mode of thought, quantum physics, is unthinkable.

The three are, in order of appearance: Planck, the reluctant revolutionary, discoverer of the quantum theory who did not at once understand that his quantum law meant the end of an era now called classical.

Einstein, discoverer of the quantum of light, the photon, who at once realized that classical physics had reached its limits, a situation with which he never could make peace.

And Bohr, founder of the quantum theory of the structure of matter, also immediately aware that his theory violated sacred classical concepts, but who at once embarked on the search for links between the old and the new, achieved with a considerable measure of success in his correspondence principle.

How different their personalities were.

Planck, in many ways the conventional university professor, teaching his courses, delivering his Ph.D's.

Einstein, rarely lonely, mostly alone, who did not really care for teaching classes, and who never delivered a Ph.D.

And Bohr, always in need of other physicists, especially young ones, to help him clarify his own thoughts, always generous in helping them clarify theirs, not a teacher of courses nor a supervisor of Ph.D.'s but forever giving inspiration and guidance to postdoctoral and senior research.

I first met Bohr when I came to Copenhagen in 1946 as the first of the post-war postdoctoral crop from abroad. Some months later Bohr asked me whether I would be interested in working together with him day by day for the coming months. I was thrilled and accepted. The next morning I went to Carlsberg. The first thing Bohr said to me was that it would only then be profitable to work with him if I understood that he was a dilettante. The only way I knew to react to this unexpected statement was with a polite smile of disbelief. But evidently Bohr was serious. He explained how he had to approach every new question from a starting point of total ignorance. It is perhaps better to say that Bohr's main strength lay in his formidable intuition and insight, not at all in erudition. I thought of his remarks of that morning some years later, when I sat next to him during a colloquium in Princeton. The subject was nuclear isomers. As the speaker went on, Bohr got more and more restless and kept whispering to me that it was all wrong. Finally he could not contain himself and wanted to make an objection. But after having half raised himself he sat down again, looked at me with unhappy bewilderment and asked: 'What is an isomer?'

These little stories may help to convey what it was like to work with Bohr. We learned from him to take nothing for granted and, to use one of his favourite phrases, to be prepared for a surprise every new day. The substance of the work was solid and serious, the style lighthearted and highly informal. One day a visitor, unacquainted with the Copenhagen spirit, remarked to Bohr: 'It seems that at your Institute nobody takes anything seriously.' To which Bohr replied: 'That is true and even applies to what you just said.'

Niels Henrik David Bohr was born in Copenhagen on 7 October 1885, the son of Christian Bohr, an internationally known physiologist, and of Ellen Adler who hailed from a wealthy Danish-Jewish banker family. He had a two-year-younger brother Harald, who was to become a mathematician of great eminence.

Niels was a good pupil in school, though apparently not a driven personality. He was good at handcrafts and sports. He was always a physical man, loving to ski and sail, and, in his youth, to play soccer. Both brothers were good at that; in fact Harald played halfback in the Danish Olympic team that won the silver medal in 1908.

Bohr entered the University of Copenhagen in 1903. In 1906 he won a gold medal of the Royal Danish Academy of Science for a theoretical and experimental investigation of ripples on vibrating liquid jets, work which led to two papers in Royal Society journals. In May 1911, his thesis *Studies on the electron theory of metals* earned him the Ph.D. degree. This very thorough work is based on Lorentz' electron theory. Already then Bohr was aware of the limitations of the classical description. To these difficulties he added one he himself had discovered: 'It does not seem possible, at the present stage of development of the electron theory, to explain the magnetic properties of bodies from this theory.' I have not found in the thesis any reference to the question of spectra.

In October 1911 Bohr went to Cambridge, hoping to work with J.J. Thomson. As I know from Bohr, his first meeting with J.J. went about as follows. Bohr entered, opened J.J.'s book *Conduction of electricity through gases* on a certain page, pointed to a formula concerning the diamagnetism of conduction electrons, and politely said: 'This is wrong'. Some of the subsequent encounters went similarly, until J.J. preferred to make a detour rather than meet Bohr. Rosenfeld later told me a rather similar story. Shortly before his death, Bohr said: 'I considered Cambridge as the centre of physics, and Thomson as a most wonderful man. It was a disappointment to learn that Thomson was not interested to learn that his calculations were not correct. That was also my fault. I had no great knowledge of English, and therefore I did not know how to express myself... . The whole thing was very interesting in Cambridge, but it was absolutely useless' [1]. The lack of contact was especially disappointing to Bohr because, then and later, he looked up to J.J. as a great man. He kept busy in Cambridge, however, attending lectures, writing a short paper on the electron theory of metals, and reading *The Pickwick Papers* (a book he always remained fond of) in order to improve his English.

In March 1912 Bohr moved to Manchester for a three months' stay at Rutherford's laboratory. Only a few months before Bohr's arrival Rutherford had discovered that an atom has a nucleus of small dimensions in which nearly all the atomic mass is concentrated. Inspired by Rutherford's discovery Bohr was able, in 1913, back in Copenhagen meanwhile, to decode the Balmer formula for the spectrum of hydrogen and thereby to unlock the secret of the structure of atoms.

It was a triumphant moment in the history of thought. Awareness of spectra predates recorded history, since primitive man must have worshipped the rainbow. Aristotle had produced a theory for this phenomenon, elaborated in learned discourse during medieval times [2]. First Descartes then Newton had grasped the true origins of the rainbow effect. Newton, experimenting with his prisms, had decomposed the sun's light into a continuous spectrum of colours and so had founded spectrum analysis. Newton had also wondered about the dynamical origin of spectra:

> 'Do not all fix'd Bodies, when heated beyond a certain degree, emit Light and shine; and is not this Emission perform'd by the vibrating motions of their parts?' [3]

It was the twenty-eight-year-old Bohr who in 1913 answered Newton's query. His inspiration came from Rutherford, as said, but in addition from the great experimental advances in quantitative spectroscopy which began in the 1850s. For our purpose the main event in this fascinating period [4] is Balmer's divine guess concerning the line spectrum of atomic hydrogen. Balmer, a Swiss school master from Basle, was mainly interested in biblical architecture. At some point his attention was drawn to data obtained by Angström (1868) [5] on the frequencies of four hydrogen lines, H_α, H_β, H_γ, and H_δ (now known as the first four Balmer lines). Comparison with modern results shows that Angström's values were accurate to one part in ten thousand. Nothing more, nothing less than these four lines led Balmer to conjecture that the hydrogen spectrum has infinitely many lines with frequencies ν_{mn} ($n = 1, 2, 3, \ldots, m > n$ and integer) given by (in modern notation)

$$\nu_{mn} = R[(1/n^2) - (1/m^2)]\,, \qquad R = 3.2916 \times 10^{15}\,\mathrm{s}^{-1}\,.$$

That is the formula published in 1885 by the sixty-year-old Balmer in the first physics paper [6] he ever wrote. Thus in 1885 the constant R (later called the Rydberg) was known to one part in ten thousand—to the great benefit of Bohr. (Fits of many more lines to the formula followed rapidly.) I once asked Bohr what people thought of this formula and of spectra in general between 1885 and 1913 when he, Bohr, saw what it meant. I do not remember exactly what Bohr replied but I can guess that it must have been something similar to what he said some years later.

> 'One thought [spectra are] marvellous, but it is not possible to make progress there. Just as if you have the wing of a butterfly then certainly it is very regular with the colours and so on, but nobody thought that one could get the basis of biology from the colouring of the wing of a butterfly [7].

What was Bohr like in 1913? According to Richard Courant: 'Somewhat introvert, saintly, extremely friendly, yet shy' [8]. According to the Danish physicist Rud Nielsen: 'He was very friendly, an incessant worker, and seemed always in a hurry. Serenity and pipe smoking came later' [9].

At that time Bohr was also a newlywed. In the summer of 1912 he married Margrethe Norlund. It was a superb marriage, as I know especially well since during the summer of 1946 I lived with the Bohr family in their country house in Tisvilde, one of the happiest periods in my life.

Now to Bohr's paper [10] on the hydrogen atom, dated 5 April 1913. At its beginning, Bohr refers, for the first time I believe, to the fact that, according to the classical theory, 'the electron will no longer describe stationary orbits' but will fall inward to the nucleus due to energy loss by radiation. Then he plunges into the quantum theory. His first postulate: An atom has a state of lowest energy (he calls it the permanent state, we the ground state) which *by assumption* does not radiate, one of the most audacious hypotheses ever introduced in physics. His second postulate: Higher 'stationary states' of an atom will turn into lower ones such that the energy difference E is emitted in the form of a light quantum with frequency f given by $E = hf$ (h is Planck's constant).

I shall not discuss in detail Bohr's quantum constraint on the hydrogen orbits. Suffice it to say that it is equivalent to the one we learned in school: The orbital angular momentum L is restricted to the values

$$L = n(h/2\pi), \qquad n = 0, 1, 2, \ldots.$$

Not only does the Balmer formula follow at once, but R now appears in terms of fundamental constants

$$R = (2\pi^2 e^4 m/h^3)\, s^{-1}.$$

This prediction of R, 'inside the experimental errors in the constants entering in the expression for the theoretical value' [10] is the first triumph of quantum dynamics.

Bohr's ideas were rapidly given serious consideration, but the initial response was mixed. Success and logic appeared to have parted ways. Why should an atomic ground state not radiate? There was also the issue of causality, first raised by Rutherford even before Bohr's paper [10] on the hydrogen atom had appeared. On 6 March 1913, Bohr had sent his paper to Rutherford, requesting [11] that it be submitted to *Philosophical Magazine*. In his reply (20 March) [12] Rutherford remarked: 'There appears to me one grave difficulty in your hypothesis which I have no doubt you fully realize, namely how does an electron decide what frequency it is going to vibrate and when it passes from one stationary state to another? It seems to me that you would have to assume that the electron knows beforehand where it is going to stop.' These conflicts with causality, classical causality, characteristic for the old quantum theory, the period which begins with Planck and ends with quantum mechanics, would remain unresolved until after 1925 when that new mechanics brought clarity. Numerous other serious difficulties arose in the years 1913–1925. Just one more example: attempts to apply the Bohr theory to the spectrum of helium, the next simplest atom, led to disaster—not surprisingly, in the absence of spin and the exclusion principle.

Bohr was of course well aware of all these problems. As was written of him at the time of his death: 'The tentative character of all scientific advance was always on his mind, from the day he first proposed his hydrogen atom, stressing that it was merely a model beyond his grasp. He was sure that every advance must be bought by sacrificing some previous certainty and he was forever prepared for the next sacrifice' [13].

The days of the old quantum theory are among the most unusual in all of the history of physics. As said, paradoxes were plentiful, yet, now here, now there, additional evidence showed that there had to be something right about the Bohr theory. There were Bohr's own prediction [14] on the ratio of Rydberg constants for singly ionized helium and hydrogen, agreeing with experiment to five significant figures; Sommerfeld's theory of fine structure; the introduction of selection rules, beginning with the rule $\Delta L = \pm 1$ for electric dipole transitions, first stated by Bohr [15] and independently by Rubinowicz [16]; and Bohr's treatment of the periodic table of elements, according to which atomic electrons are arranged in shells such that the chemical properties are largely determined by the electron configuration in the outermost shell.

Bohr's most important contribution to the development of physics is his pioneering work, only briefly sketched in the foregoing, on the quantum dynamics of the atom. This is perhaps best appreciated by recalling the situation at the turn of the century as described [17] by d'Andrade:

'It is perhaps not unfair to say that for the average physicist at the time, speculations about atomic structure were something like speculations about life on Mars—very interesting for those who like this kind of thing but without much hope of support from convincing scientific evidence and without much bearing on scientific thought and development.'

It was Bohr who made atomic structure into a subject of scientific inquiry, building on the wealth of information on spectra amassed in the nineteenth and early twentieth century. He himself created many tools of the old quantum theory, most notably the correspondence principle. In 1923 Hans Kramers, Bohr's closest collaborator through most of the period under discussion, wrote [18] of this principle: 'It is difficult to explain of what it consists, because it cannot be expressed in exact quantitative laws and it is, on this account also difficult to handle.' [Roughly speaking, the principle states that for large wavelengths (slow oscillations) the theory should be in formal accord with classical mechanics and electrodynamics.] Another contemporary characterized [19] the old quantum theory like this: 'It is all mysterious, but [one] cannot deny that all the reasoning is sound, and that is is a fruitful mysticism.'

The best characterization of Bohr's activities during those years was given [20] in 1949 by the seventy-year-old Einstein: 'That this insecure and contradictory foundation was sufficient to enable a man of Bohr's unique instinct and tact to discover the major laws of the spectral lines appeared to me as a miracle—and appears to me as a miracle even today. This is the highest sphere of musicality in the sphere of thought.'

Those years of struggle in the *clair-obscure* left an indelible mark on Bohr's style, once again best expressed [21] by Einstein: 'He utters his opinions like one perpetually groping and never like one who believes to be in the possession of definite truth.' As Bohr himself often used to say, never express yourself more clearly than you think. That admonition goes a long way toward explaining why Bohr was such a divinely bad lecturer. He was bad because he spoke too softly and because he would every now and then skip an argument well thought out beforehand (as I know well, having helped him prepare lectures on a few occasions and listening to their subsequent delivery). He was divine because he would be actively struggling with the concepts under discussion even as he was delivering his lecture. Rabi has told me the following story. During the First International Conference on the Peaceful Uses of Atomic Energy, Bohr gave one of the evening lectures [22], in English, in CERN's main auditorium. As usual, simultaneous translation from a prepared text was provided. Playing with his head set Rabi noticed that one of the translations was in English. He now had the choice, either to listen to the translation and understand what Bohr was saying; or to listen to Bohr directly. He chose the latter.

Bohr's role as teacher started shortly after the appearance of his 1913 papers on the quantum theory of the atom. In April 1916 he was appointed to the newly created chair in theoretical physics in Copenhagen. On 3 March 1921 his Institute for theoretical physics (the later Niels Bohr Institute) was formally opened. Soon physicists from far and wide came to work at his Institute, arguably the world's leading centre in theoretical physics during the twenties and thirties. The international character of the enterprise was manifest from the start. By 1930 some sixty physicists hailing from Austria, Belgium, Canada, China, Germany, Holland, Hungary, India, Japan, Norway, Poland, Roumania, Switzerland, the United Kingdom, the United States, and the USSR had spent time in Copenhagen [19].

In 1925 quantum mechanics arrived. In March 1927 Heisenberg stated his uncertainty principle. On 16 September 1927, at the Volta Meeting in Como, Bohr enunciated for the first time [23] the principle of complementarity, which embodies the physical interpretation of the uncertainty relations:

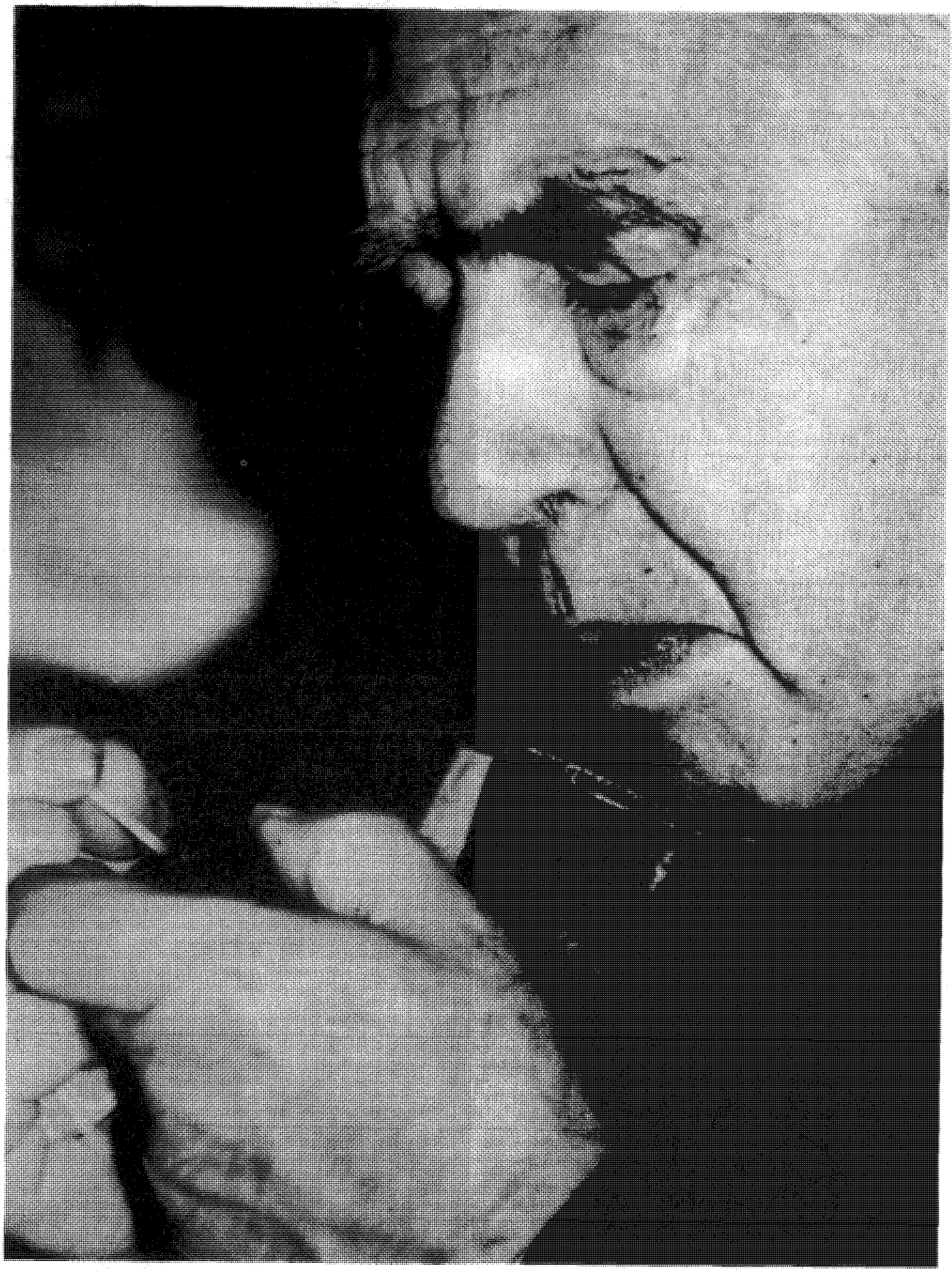
'The very nature of the quantum theory ... forces us to regard the space–time coordination and the claim of causality, the union of which characterizes the classical theories, as complementary but exclusive features of the description, symbolizing the idealization of observation and definition, respectively.'

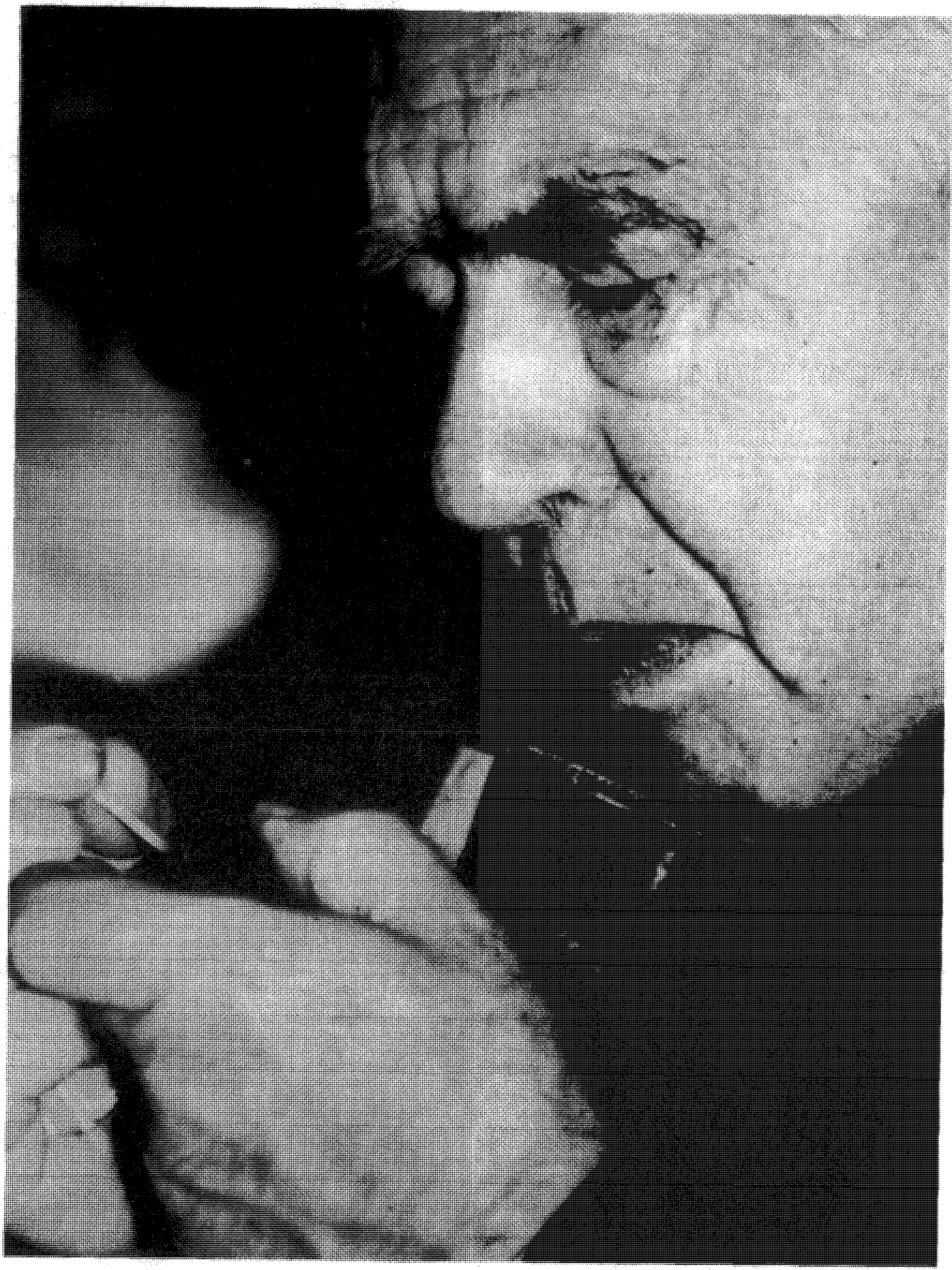
Already then he emphasized that we have to treat with extreme care our use of language in recording the results of observations that involve quantum effects. 'The hindrances met with on this path originate above all in the fact that, so to say, every word in the language refers to our ordinary perception.'

Bohr's deep concern with the role of language in the appropriate interpretation of quantum mechanics never ceased. In 1948 he put it as follows:

'Phrases often found in the physical literature, as "disturbance of phenomena by observation" or "creation of physical attributes of objects by measurements", represent a use of words like "phenomena" and "observation" as well as "attribute" and "measurement" which is hardly compatible with common usage and practical definition and, therefore, is apt to cause confusion. As a more appropriate way of expression, one may strongly advocate limitation of the use of the word phenomenon to refer exclusively to observations obtained under specified circumstances, including an account of the whole experiment' [24].

This usage of *phenomenon,* the one to which nearly all physicists now subscribe, was inacceptable to Einstein. In contrast to the view that the concept of phenomenon *irrevocably* includes the specifics of the experimental conditions





Bohr discussed his ideas with his friend Felix Frankfurter, who was so impressed that he promised to convey these ideas to President Roosevelt. After having done so, Frankfurter reported back to Bohr that the President intended to take up these issues with Prime Minister Churchill.

Bohr had also talked with Anderson and Halifax who decided that Bohr should talk directly to Churchill. Others added their voice after Bohr returned to England, in April 1944. His old friend Sir Henry Dale, President of the Royal Society, wrote to Churchill: 'I cannot avoid the conviction that science is even now approaching the realization of a project which may bring either disaster or benefit on a scale hitherto unimaginable to mankind.'

Meanwhile an event had occurred which was to complicate matters. On 28 October 1943, Kapitza had sent a letter to Bohr in Stockholm, inviting him to join scientists in the Soviet Union. The letter reached Bohr in London, from where he sent a cordial but non-committal reply [34], seeing to it that copies of both letters were transmitted to the British authorities.

On 16 May 1944 Bohr was received by Churchill whose adviser, the physicist Lord Cherwell, was also present.

Let us briefly interrupt the story in order to catch a glimpse of the status of the bomb project at that time.

On the preceding 4 February, General Groves, administrative head of the Manhattan Project, had written to the President that it was still unknown how much fissionable material was needed to produce a bomb [35].

During the following June/July the discovery of spontaneous fission of plutonium-240 caused serious reconsideration of detonation methods for plutonium bombs [36].

The point of all this is that at the time of the Bohr–Churchill meeting scientists did not yet have the means for constructing a testing device, let alone a deliverable weapon. Yet already then Bohr was urging the case for an open world.

The meeting was a disaster. Here was the War Lord, desperately preoccupied with immense responsibilities, used to incisive decisions, three weeks before D-day. There was Bohr, hard to follow as always, failing to make any impact. 'We did not speak the same language', Bohr said later [37]. It would be most inappropriate, it seems to me, to blame either man for a missed opportunity.

In June 1944 Bohr returned to the United States.

On 19 August, at Quebec, Roosevelt and Churchill signed, 'Articles of Agreement governing collaboration between the authorities of the USA and the UK in the matter of Tube Alloys.'

On 26 August Bohr met in complete privacy with Roosevelt for a meeting that lasted an hour and a half. It was for that occasion that Bohr had prepared the memorandum [32] mentioned earlier. This time Bohr got his points across and the President sounded promising [38].

On 18 September, Churchill and Roosevelt held a private meeting at the President's residence in Hyde Park, New York. An *aide-mémoire* of that encounter contains three points [39]. First, the bomb project shall be kept strictly secret for the time being. Secondly, the US and the UK shall continue their collaboration on these projects after the war. Thirdly, 'Enquiries should be made regarding the activities of Professor Bohr and steps should be taken to ensure that he is responsible for no leakage of information, particularly to the Russians.' The next day Churchill sent a note [40] to Cherwell: 'The President and I are much worried about Professor Bohr. How did he come into this business? He is a great advocate of publicity. He says he is in close correspondence with a Russian professor, an old friend of his in Russia to whom he has written about the matter and may be writing still. The Russian professor has urged him to go to Russia in order to discuss matters. What is all this about? It seems to me Bohr ought to be confined or at any rate made to see that he is very near the edge of mortal crimes. I had not visualized any of this before, though I did not like the man when you showed him to me, with his hair all over his head, at Downing Street. Let me have by return your views about this man. I do not like it at all.'

Bohr had gotten nowhere.

There is an aftermath to this tale.

Bohr spent the spring term of 1950 at the Institute for Advanced Study in Princeton. During that period he was deeply preoccupied with the preparation of an Open Letter to the United Nations, from which I have already quoted [33]. I helped him with the preparation of several drafts.

The following story dating from that period was told to me by Robert Oppenheimer.

At one point during this time Bohr called on Secretary of State Dean Acheson to discuss with him the content of his planned Letter. The meeting began at, say, two o'clock, Bohr doing the talking. At about two thirty Acheson spoke to Bohr about as follows. Professor Bohr, there are three things I must tell you at this time. First, whether I like it or not, I shall have to leave you at three for my next appointment. Secondly, I am deeply interested in your ideas. Thirdly, up till now I have not understood one word you have said. Whereupon, the story goes, Bohr got so enraged that he waxed eloquent for the remainder of the appointment.

Bohr's Letter, dated 9 June 1950, was delivered to the Secretary General of the United Nations on 12 June.

On 24 June the Korean war broke out.

On 1 November the United States exploded in the Pacific its first thermonuclear device.

Once again Bohr was defeated by overwhelming historical forces.

And that is why Frankfurter and I chatted about how bittersweet a day in Bohr's life the 24th of October 1957 had to be.

Bohr's rich and full life came to an end on 18 November 1962. He was seventy-seven years old.

As I try to find a single phrase expressing what Bohr meant to me I am reminded of what Bohr once wrote [41] of Rutherford: 'He was a second father to me.' That is just what Bohr himself was to all those who worked with him for some length of time.

In October 1937 Bohr was in Bologna, attending the Galvani Conference, when word came of Rutherford's death. Then and there Bohr made a brief speech in memory of that great man. What he said then [42] I would like, in conclusion, to apply to Bohr himself.

> 'His untiring enthusiasm and unerring zeal led him on from discovery to discovery and among these the great landmarks of his work, which will for ever bear his name, appear as naturally connected as the links in a chain.
>
> Those of us who had the good fortune to come in contact with him will always treasure the memory of his noble and generous character. In his life all honours imaginable for a man of science came to him, but yet he remained quite simple in all his ways. When I first had the privilege of working under his personal inspiration he was already a physicist of the greatest renown, but nonetheless he was then, and always remained, open to listen to what a young man had on his mind. This, together with the kind interest he took in the welfare of his pupils, was indeed the reason for the spirit of affection he created around him wherever he worked .... The thought of him will always be to us an invaluable source of encouragement and fortitude.'

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