

## Rutherford and Bohr\*

As every physicist knows, our present knowledge of the structure of matter, of atoms and nuclei, is built on the foundations laid by Rutherford and Niels Bohr early this century. Each of them was a great physicist in his own right, but their contributions to physics, particularly in the important period from 1912 to perhaps 1920 owe much to their mutual interaction – we are dealing with a ‘two-body problem’, to use current jargon. Later their paths separated; Rutherford’s primary interest was the atomic nucleus, and once he had identified its role, he left the further exploration of the atom to others, while Bohr concentrated on the theory of the atom, and for its study developed the quantum theory. This was the ‘old’ quantum theory, spectacularly successful in explaining the hydrogen spectrum, but lacking logical consistency in mixing classical and quantum concepts. He maintained an interest in the nucleus, but made major contributions to nuclear physics only from 1936 on.

However, even while their fields of work were separate, their relations developed into a close friendship, and in their correspondence and in their frequent meetings they continued discussing the current problems of physics – and of the world. In this lecture, devoted to the memory of Rutherford, it is fitting to try giving an impression of this friendship between two very different personalities, as it emerges from their correspondence.

They were indeed very different in many ways. Rutherford was outspoken, outgoing and direct. He liked his physics and his experiments simple and described his work in simple and concise language. He had a loud and booming voice, and it is said that in the days when counting circuits tended to be sensitive to noise, the performance of his collaborators’ equipment went wrong whenever he entered their research rooms. He made decisions easily and firmly, and once a matter was decided, did not give it any further

thought. He could be rude, and even unreasonable on occasion, but when he had cooled off would put matters right with a handsome apology.

Bohr spoke in a quiet voice, hardly above a whisper, and listeners had trouble understanding him in any language. In what he said or wrote he was always conscious of the many limitations and conditions that restrict the validity of any statement – as he liked to express it, ‘Truth and clarity are complementary’. He was always concerned about hurting any person’s feelings, and he found great difficulty in making his plans, and only too often changed them almost as soon as they had been arranged.

Yet with these great differences, there were also many important similarities. Both were capable of enormous enthusiasm for a promising idea in physics. Both were not deflected by unimportant details, but could give painstaking attention to detail when it mattered. Both regarded mathematics as an important tool in formulating and applying the laws of physics, but never as an end in itself. Rutherford was fond of making disparaging remarks about theoreticians who were too attached to formal mathematics, so much so that he is sometimes believed to have been opposed to theory altogether. (His respect for Bohr, his friendship with R. H. Fowler, and his enthusiastic reception of Gamow’s results, disprove this story.) Bohr was too polite for such remarks, but restricted himself to the minimum amount of mathematics in his own work. Both were untidy lecturers, but would fascinate and stimulate the audiences.

These similarities no doubt caused their mutual respect and patience with each other’s idiosyncrasies.

Niels Bohr arrived in Cambridge in 1911 to work with J. J. Thomson. He was a shy but enthusiastic young man of 26, who had written his Ph D thesis on the electron theory of metals, and expected ‘J. J.’, the discoverer of the electron, and the author of some papers on electrons in metals, to be interested in his work. In this hope he was disappointed. Thomson probably never read his thesis, and certainly was not inter-

ested in discussing it, but suggested some rather routine research work, which Bohr tackled without enthusiasm.

Then during a weekend visit to friends in Manchester he met Rutherford, the physics professor in Manchester, who was then 39. They met again when Rutherford visited Cambridge. In these encounters they took an immediate liking to each other, and Bohr asked to be allowed to work under Rutherford. He stayed in Manchester from March to July 1912.

The first problem which Rutherford suggested to him was the slowing-down of charged particles in passing through matter. This was a very important problem, because the main method for determining particle energies was then using their range. A study by Darwin had assumed the electrons of the medium to be free, and Bohr soon decided that this was an unwarranted approximation. He therefore had to make assumptions about the forces acting on the electrons. At first he took the forces to be harmonic, a model then favoured because of the existence of sharp spectral lines.

However, Rutherford was then developing his ideas about the nuclear atom, and Bohr, who was impressed by Rutherford’s results, found that the assumption of Coulomb forces from a central charge gave better results for hydrogen. For his problem he had to know the orbit of the electron before the arrival of the passing particle, and in hydrogen it was natural to assume a circular orbit. What could one assume for the motion of two electrons in helium? Bohr convinced himself that there were no stable solutions to this dynamical problem, because one of the electrons could ‘fall’ into the nucleus, while the liberated energy would be carried away by the other. This difficulty reinforced a suspicion developed earlier in his studies of electrons in metals, where he showed that classical physics could not account for any magnetic properties of materials.

While continuing to work on the slowing-down problem, which retained his interest for the rest of his life, his thoughts now turned to the structure of atoms, and to ways in which the inadequate classical picture might be modi-

\*Text of the Rutherford Memorial Lecture delivered by Sir Rudolf Peierls, FRS, on 10 November 1987 at the National Physical Laboratory, New Delhi.

fied by using Planck's quantum of action. A first manuscript showing the beginnings of these ideas was written before he left Manchester.

Bohr returned to Copenhagen full of enthusiasm. His farewell letter to Rutherford said: 'Your criticisms and suggestions have made so many questions so real for me.' Further progress was slow, because of a heavy teaching load. But he kept working both on the slowing-down problem and on his new ideas. He kept in touch with Rutherford. When he worried about slow progress, Rutherford comforted: 'Don't hurry with your second paper; it is not likely that anyone else is working on this'. He also shares his thoughts on other topical problems in physics with the young Bohr. He and his pupils were discussing the new experiments of von Laue. After saying in one letter that the Laue spots can be explained by interference... nothing to do with wavelength (!), he says in the next that X-rays must be regarded as some kind of wave motion 'but I personally cannot escape from the view that the energy must be in concentrated form'. In other words, his intuition favoured the quantum theory.

Meanwhile Bohr's work developed into the well-known Bohr model of the atom, eventually published in three famous papers (called the 'Trilogy' by Rosenfeld). When the first paper was sent to Rutherford, he was impressed by the good agreement with many facts, but at once put his finger on the main contradiction in the quantum theory: How does the electron decide what frequency to emit? He added, as a minor criticism: 'I think in your endeavour to be clear, you have a tendency to make your papers much too long and a tendency to repeat your statements in different parts of the paper'. The paper could be cut down without sacrificing anything to clarity. 'Long papers have a way of frightening readers.' He added a post-script: 'I suppose I may use my judgement in cutting out any matter I consider unnecessary?'

He did not yet know Bohr well enough to realize his attitude to his writings, in which he would fight over each word as a she-bear over her cubs. Bohr had brought to England a translation of his thesis, but failed to get it published because it was considered too long. He would not shorten it. So the

English version of his important work on the electron theory of metals existed only in a few copies until it appeared in 1972 in his *Collected Works*.

Rutherford's letter crossed with one in which Bohr sent an amended and expanded version of the paper. Rutherford tried again: The additions are excellent and reasonable, but the paper is too long. Some of the discussions should be abbreviated. 'As you know it is the custom in England to put things very shortly and tersely, in contrast to the German method, where it appears to be a virtue to be as long-winded as possible.'

In April 1913 Bohr visited Manchester again to discuss his paper with Rutherford. There is no record of this discussion, but the paper emerged practically uncut; Rutherford communicated it to the *Phil. Mag.*

Rutherford evidently realized the importance of Bohr's work, and one would have expected him to mention this in letters to the many other physicists with whom he was in correspondence, but the only letter I could find was one sent on 19 April 1913 to K. Fajans, the physical chemist in Munich: 'Bohr of Copenhagen has been working at the general theory of atoms built on my model and appears to have made definite progress... He is a very capable fellow, and there will soon appear a very interesting paper by him in the *Phil. Mag.*'

In further correspondence about the remaining two papers, Bohr is evidently aware of Rutherford's critical attitude. On 13 June 1913 he sends the third paper, saying 'I hope I have better succeeded in making it short.'

In the summer he visited Manchester again for talks with Rutherford. During that autumn the British Association meeting in Birmingham was to have a discussion on radiation, at which H. A. Lorentz would be present, and Rutherford arranged for Bohr to be invited. At first enthusiastic, Bohr writes in August that he will not be able to leave Copenhagen, and regrets he cannot come to Birmingham. But evidently he was persuaded, and in October he writes that he was very glad he did come to the meeting. This episode illustrates Bohr's frequent hesitation in making plans, and his inclination to change them at short notice, which will recur. Meanwhile Bohr's position at Copenhagen was only temporary and involved too much

teaching and lab. demonstration. Rutherford wanted him to come back to Manchester as a reader, and broached the subject in a very gentle way (20 May 1914): 'I am glad that... your position in the University of Copenhagen is now being considered... we are now advertising for a successor (to Darwins Readership) at £200. Preliminary enquiries show that not many men of stature are available. I should like to get a young fellow with some originality in him.'

In due course Bohr accepted the Readership for a year in the first place. He stayed for a second year, after Rutherford had asked the University of Copenhagen for further leave of absence for him: 'I think the experience that Dr Bohr is gaining of the work in a larger university will prove of much value to him... I should much regret the loss of his services to the university here.'

From this second, very productive Manchester period we have an assessment by Rutherford of Bohr, in reply to G. N. Lewis in California, who was considering Debye and Bohr for a temporary lectureship. After some favourable remarks about Debye, Rutherford writes: 'I regard Bohr as one of the coming men in Mathematical Physics, and I think he has a better grasp of physics than any of the Mathematical people I have come across. He is a man of great originality and, as you know, his work has already attracted wide attention, and I am confident will do so even more in the future... He is thoroughly *au courant* of all the modern physical problems, and has an extraordinarily wide knowledge of experimental, as well as of theoretical physics. He is a pleasant fellow, speaks English quite well, and is quite a clear and interesting lecturer.'

Bohr returned to Copenhagen in 1916 to take up a new professorship for which Rutherford wrote a strong letter of recommendations, ending 'Finally I would like to state that in my opinion Dr Bohr is one of the most promising and able of the younger mathematical physicists in Europe to-day. I think that any University would be fortunate who is able to acquire the services of such an original and fruitful investigator'.

The First World War made the correspondence between Manchester and Copenhagen difficult, but an exchange of letters continued, with Bohr always

Rudolf Peierls gave an Academy Lecture entitled 'Recollection of the early days of quantum mechanics – more about physicists than about physics' on 26 November 1987. Reproduced below is the introduction given by S. Ramaseshan.

Rudolf Peierls' name has become an integral part of modern physics. Every problem he has touched he has embellished, whether it be in statistical physics, condensed matter physics or nuclear physics. Each of his contributions is seminal and has set the directions for the future. Take for example, his work with Landau on the nature and long-range order in one-dimensional and two-dimensional systems. This was done decades before it was applied to liquid crystals. Or his work with Bethe on short-range order in phase transitions. This was generalized and applied much beyond its original context. The problems in quantum theory which he has worked on – indeed the panorama of problems he has taken up is really breathtaking.

There are even more significant aspects of Peierls' standing amongst his peers. His book on *Quantum Theory of Solids* is the kind that other writers of books and of papers *have* to consult to be sure that they have got everything right. The nine volumes of Landau and Lifshitz form the Bible for many theoretical physicists. To be repeatedly cited in this work is the closest one can come to canonization. Peierls has been so cited. This to me is much better than a long list of medals and honours – which also he has.

On a personal note, some of us at the Raman Research Institute consulted him on the scattering of positrons by hydrogen atoms with the formation of positronium. It was a lesson and it was a pleasure to watch the master at work, reducing the problem to its bare essentials and formulating a scheme of calculations which captured the physics of the problem.

It was a privilege to be with him, listening to his incomparable science and incomparable stories. It was fun and laughter. For example, he told me the story of the great Rutherford's letter to Neils Bohr about his painful knee which had healed. Rutherford – who was known to have driven his students wrote:

'The pain has gone. I can now kick my students with pleasure – and not with a feeling of painful duty.'

I found many stories not in his autobiography. So just as he is writing an enlarged second version of his now famous *Surprises in Theoretical Physics*, we should persuade him to write a second version of his autobiography with starred stories.

It is not too well known that Rudolf Peierls applied for a position at the Institute in 1933 or 1934 when he was 26 or 27. Raman tried to support the older Max Born – but that too was in vain. If Rudolf Peierls had come it would have been good for the Indian Institute of Science, good for Indian physics – nowever one is not too sure whether it would have been good for Rudolf Peierls. Would he have met all the physicists, he talks about?

Rudolf Peierls tells us how Zuchichi told him: 'You are getting on in years. You must have known many physicists that we only read about. Could you not tell us about them?'

This is how the delightful book *The Bird of Passage* was begun.

in the position of the pupil writing to his teacher. Reporting on his work in November 1916 he says he has been 'remembering your often repeated advice as to the greater use of working out special problems than making philosophy'.

In December 1917 Rutherford writes that in spite of many war-time duties he occasionally finds some time for research. 'I have got I think results that will ultimately prove of great importance. I wish you were here to talk matters over with.' He describes the early experiments on nuclear disintegration by alpha particle bombardment. This was the beginning of his famous experiments which showed that alpha particles could knock protons from many

nuclei, thus causing a transmutation of one chemical element into another. 'Regard this as private.'

As soon as the war was over, Rutherford set about getting a chair for Bohr created in Manchester, but wanted to make sure that Bohr would be inclined to take such an offer seriously. The chair would be independent, but 'on account of my grey hair, I would probably continue to be director... I wish I had you here to discuss the meaning of some of my results'. It is hard to get firm proofs. 'Counting scintillations is hard on old eyes' (He was then 46).

To Bohr this letter 'besides being the source of the greatest pleasure to me, has at the same time been the object of

sorrowful considerations'. He feels under an obligation to stay in Copenhagen, and work for the development of physics in Denmark, after the university had done for him what they could. 'At the same time I feel very strongly that the result will never be the same as if I could work with you.'

Rutherford pleaded with him to keep an open mind until they could talk things over, but the problem soon was made academic by Rutherford moving to Cambridge. Their correspondence now settles down to a pattern of exchanges about developments in physics and local news. (Rutherford in January 1920 reports that Aston talked about isotopes and J. J. Thomson said he did not believe the result about chlorine.

'You can imagine that I enjoyed myself thoroughly between the two, but I have little doubt that Aston is quite correct.')

After some exchange of letters it is arranged that Rutherford will visit Copenhagen and be present at the formal opening of Bohr's new Institute. Some delays in the correspondence (Rutherford: 'You are as bad a correspondent as I am, and have not the excuse of age!').

Bohr is eager for the visit. 'You can hardly imagine with what expectation and pleasure your visit and lectures are looked forward to by all Danish Scientists, and my wife and I only regret that Lady Rutherford will not be coming, too.' Rutherford did come in September 1920, but the new Institute was not yet finished.

The teacher-pupil relation is symbolized in these letters by Rutherford quite early changing from 'Dear Dr. Bohr' to 'Dear Bohr' and often 'My dear Bohr', while Bohr kept to a respectful 'Dear Professor Rutherford'. In 1920 he even changed to 'dear Sir Ernest' (Rutherford was knighted in 1914). The address 'Dear Rutherford' appears first only in 1929.

A visit by Bohr to Cambridge was planned for April, 1921, after the Solvay conference. There was much correspondence about dates, arrangements for lectures, etc., spiced with Rutherford's remarks about his work - 'My work on knocking H out of atoms goes on apace. Several atoms succumb each week'. Bohr still sent his papers for Rutherford to communicate to the journals, and often to deal with the proofs. 'I have made a few minor corrections in English... and would have liked to make more, but I know of old how difficult it is to reconstruct the meaning to your satisfaction'. Bohr's note disagreed with a theory by Langmuir, and Rutherford added: 'I always felt that Langmuir's ideas were much too descriptive for the year of our Lord 1921. They might have served in 1911. See you in Brussels (at the Solvay conference)'.

But Bohr could not go to Brussels or to England, as he was suffering from overwork and was ordered complete rest. Rutherford expresses his regrets. 'I am afraid you are too unselfish and give too much of your time and energy to help other people.' this was in March.

In May Rutherford enquired about Bohr's health. 'I trust that Mrs Bohr is in proper control of you and will prevent you doing any serious work for the next three months... I would feel inclined to go for a holiday in the open air to a place where you never see a scientific man and where you do nothing but eat and read light novels.'

In August Bohr thinks he might be able to come to Cambridge in October or November, but he would like to wait a fortnight to confirm this. 'Being sure that you will understand that this form of the answer is dictated by the wish not to disappoint you once more by late alterations.'

In September he seems well enough but cannot come in October because he is working on a new paper, and also feels obliged to catch up on his university duties neglected while he was unwell. Meanwhile he sends another note for *Nature*, remarking hopefully '... if you should not find the language too bad I do not think I shall need a proof'.

But Rutherford does not agree: 'I am afraid your English is deteriorating with absence from this country'. He has sent the note on, but is anxious to see a full paper to understand the precise assumptions made in assigning electron numbers to the various shells. He adds domestic news: His daughter Eileen is to marry R. H. Fowler. 'The couple were with us during the vacation, and I derived much interest and amusement from their ways. You and Mrs Bohr are young enough to sympathize with them.' Once again the emphasis on age.

In another episode, Bohr reports that Jacobsen in Copenhagen has obtained results on the 'straggling' of alpha particles which differ from those obtained by Henderson in Manchester. But in the next letter he writes 'in a very shameful mood'. Jacobsen's work was hurriedly prepared and is not reliable.

In December 1921, Bohr is appointed an honorary member of the Royal Institution. Rutherford writes that as a Professor in that Institution he is delighted 'that it has seen fit to honour one of my old friends'. Bohr answers belatedly and, as usual, with excuses. He acknowledges Rutherford's letters 'and all the honour which has been shown me by English societies, which I feel I have only deserved very little, but for which I am very thankful not least on account of

this testimony of your friendship for me'. A delicate way of saying this was no doubt Rutherford's influence.

Bohr's long-delayed visit to Cambridge finally took place in April 1922. Following the usual exchange of many letters about dates, with Rutherford urging: Please let me know at once the titles and dates for the lectures. Bohr writes five weeks after the visit, apologizing for his lateness in sending thanks 'for the kindness you and Lady Rutherford showed me during my stay in your house. I feel it very difficult to express myself, but I am thinking very often of Cambridge and of your home and laboratory, and the way you looked after me in small and big things in both places'.

On the question whether Kramers, then Bohr's assistant, could be invited to spend a term in Cambridge, Rutherford says he would like to do so, but funds are short. 'I must be just before I am generous.'

To his telegram of congratulations on the Nobel Prize in late 1922, Bohr replies 'I have felt so strongly how much I owe to you, not only for your direct influence on my work and your inspiration, but also for your friendship in these 12 years since I had the great fortune to meet you in Manchester'. He is sad not having written for so long. 'I took your rebuke for only writing seldom as not the smallest sign of your friendship'. He ends 'Yours ever, Niels Bohr, who will do his best to improve in writing'.

This letter crossed with one from Rutherford, following up his telegram. It is now his turn to apologize for writing late, because he was busy. On the prize 'I knew it was merely a question of time, but there is nothing like the accomplished fact'.

Bohr's theory had predicted the existence and properties of a new element, and this was discovered by Hevesy and Coster in Copenhagen in 1922; it was called hafnium. Rutherford wrote to congratulate Hevesy on this 'admirable example of the cooperation between theory and experiment'. He adds: 'I am sure it is highly gratifying to Bohr, even if he tries to suppress it'. He knew of course that this letter would be shown to Bohr, with the gentle teasing of Bohr's modesty.

There was controversy about the discovery, with some chemists claiming

they had found the new element earlier. Rutherford sympathized with Bohr's Institute. He calls Scott, who made such a claim 'a well-known rather elderly chemist' and says the *Times* editorial on the matter 'is very wrong and silly. One need pay no attention to such irresponsible utterances'. Bohr had sent him a letter by Hevesy and Coster for *Nature*. They have now found that Scott's preparation contained no trace of hafnium, so the letter should be re-worded. Rutherford made the changes, and also made changes in one paragraph, which was poor English. 'I think it now reads very well and is certainly a document of an explosive character.' 'I quite agree... that Urbain has not a leg to stand on.'

In February, 1923 Rutherford had his first grandson, and this again induced thoughts about age. 'I ought to feel incredibly ancient now that I have turned into a grandfather, but I still have some kick left in me.' A few days later he has evidently forgotten that he wrote about this. 'Have you heard about Eileen's son? Great amusement over my transformation into a grandfather.'

An embarrassing situation arises when Bohr is offered an honorary degree by Manchester, but says he is unable to come to Manchester on the proposed day. As it happens he is also offered an honorary degree by Cambridge, and thinks he cannot get there either, but asks the advice of Rutherford, who explains that honorary degrees are not conferred in absentia, and declining the invitation to the ceremony amounts to declining the degree. Bohr, always most punctilious about etiquette, is horrified and agrees to come to Cambridge. He asks for Rutherford's help in reconciling Manchester. A kind word from Rutherford results in agreement to hold a ceremony later in the year. As far as the letters show, this seems to have been postponed again to May, when however Bohr suffered again from overwork, and had to cancel his trip. It seems the Manchester degree was not awarded, after all. But he did come to Cambridge, and sent afterwards his usual fulsome letter of thanks.

Rutherford was still trying to get Bohr for a full-time position in Cambridge, and in July 1923 the Royal Society, no doubt on his instigation, offered Bohr a research professorship. Bohr was tempted, but felt he could not

desert Copenhagen and Danish physics. A compromise solution, by which he would share his time between Copenhagen and Cambridge, was explored, but as Rutherford suspected, proved unacceptable to the Royal Society. Rutherford did not lose hope. 'If the whole matter should break down, there are possibly other more private arrangements that might be made, but the stumbling block is the lack of funds in Cambridge to finance new ventures.' He never overcame this 'stumbling block'.

When in 1924 Bohr was again ordered to rest, Rutherford wrote 'You know that it is my opinion that you work far too hard for your health, and you would do just as much good work if you took matters easily. This is the advice of a grandfather, but nevertheless good, as I have found in my own experience'.

Things continued on an even keel. Bohr was elected a Foreign Member of the Royal Society, which Rutherford welcomed, particularly as this was during his presidency. 'You will soon be getting a little wearied in having to write letters of thanks for these scientific distinctions.'

Several further visits to Cambridge include one following the Faraday Lecture in London. Rutherford has heard about Bohr's intentions for that lecture: 'I hear you are on the war path and wanting to upset the Conservation of Energy... I will wait and see before expressing an opinion, but I always feel 'there are more things in Heaven and Earth than are dreamt of in our philosophy'. His caution was justified - Bohr's views on energy conservation were not confirmed.

After a visit in 1930, and the usual letter of thanks, Rutherford writes: 'I can assure you that we were all delighted to see you, and that you need not be over modest about your lectures, which went off excellently'.

In 1931 Rutherford was elevated to the peerage about the same time that his daughter, Eileen Fowler, died after the birth of a child. Bohr wrote about the peerage 'To all scientists, and especially to your old pupils, this great distinction will stand as a symbol of the new epoch in science which you have created, and which has so deeply influenced the life of us all... Thinking of those twenty years in which you and your family have

shown us so much friendship, my wife and I send you and Lady Rutherford many thoughts of gratitude and deep sympathy in your great sorrow'.

Rutherford replies separately to the congratulations: '... good of you to write such a cordial letter about my recent transformation. As you may guess it is not the type of distinction that appeals to me very much, but I am sure it was meant as a recognition of the importance of science to the community and thus was very difficult to refuse.'

There was another letter about Eileen's death. Rutherford writes 'Just a line to thank you for your kind note of sympathy from your wife and yourself. Fowler gave some lectures at the Poincaré Institute and has now returned to (his) house. Mrs Cook - as before will look after the house and children, at any rate for the moment. Fowler has stood the strain well so far. The baby, in the charge of a Truby King nurse, is flourishing'. The matter-of-fact tone of this letter must have been hiding great grief.

In 1932, the year of momentous developments in Cambridge, Rutherford writes 'it never rains but it pours'. He comments on the discovery of the neutron, and goes on to Cockcroft and Walton's experiments on disintegration with accelerated protons. 'I am very pleased that the energy and expense in getting high potentials has been rewarded by definite and interesting results... Actually they ought to have observed the effect a year or so ago, but they did not try it in the right way.'

In September, 1932, Rutherford visited Copenhagen. He asked Bohr if a grant towards expenses could be arranged, and this was done. After the visit he sent a letter of thanks: 'I look back on my stay with you and your good wife with the greatest pleasure'. A very different style from Bohr's letters on such occasions, but not less enthusiastic.

A little later he ends a letter about physics 'Please give my love to Margareta and remembrances to your boys'. This is the first time he refers to Mrs Margrethe Bohr by a first name, though he gets it slightly wrong. But it is correct on the next occasion.

In October, 1933 they met at the Solvay conference in Brussels, and both had unfortunate experiences on the way home. Bohr had a bad cold while staying in Paris, Rutherford had some trou-

ble with his knee, and Lady Rutherford was unwell. Rutherford did not enjoy the conference. 'I have come to the conclusion that conferences on the continent are much too harassing for anyone over the age of 60, and it is going to take a good deal of persuasion to get me to go to another. There is too much of an idea of getting value for your money by turning a conference into a treadmill... I thought the conference a success, but much too big and fatiguing for elderly people like myself who believe in action other than in talk'. A little later he reports that his knee is better, 'and I hope soon to kick my students not only as a painful duty but with pleasure!'

In 1934, a great tragedy affected the Bohr family, when their eldest son Christian was drowned on a sailing trip. Presumably Rutherford wrote on this occasion, but his letter is not in the scientific archives. He wrote to Hevesy 'we were all very disturbed about the loss of Bohr's eldest son. I hope it will not have too much of a reaction on them'. And later: 'I am very sorry to hear of your impression of the effect of the tragedy on Mrs Bohr. It may only be temporary and time is a great healer'.

In 1934, Kapitza was not allowed to return to Cambridge from his summer holiday in the USSR, to everybody's concern and particularly Rutherford's. He writes to Bohr: 'Don't believe what you hear. Governments are as a whole pretty bad, but I think the USSR can give them all points on mendacity'. He keeps Bohr informed about developments in this affair.

Meanwhile Bohr again had to cancel a visit because he was overworked. Rutherford writes he hopes Bohr will soon be himself again. 'You are much

too young to be feeling tired. You must leave that to old buffers like myself – but I am glad to say I have not reached that stage yet.'

Bohr acknowledges good wishes on his 50th birthday: 'A 50th birthday is a very small occasion which is mostly apt to remind oneself that one is no longer quite young, and still has not reached any age which in itself calls for a veneration from younger ones. Just in this respect I feel every year greater admiration for the marvellous power with which you set an example to the world of untiring creative activity.'

In October 1935, Bohr seems to have had more trouble with his health. Rutherford, slightly teasing, writes he is sorry to hear of jaundice and rheumatism. 'Did you examine your jaundiced family by the spectroscope to see whether the 'D' lines stand out plainly?' It has now been decided to give up hope of Kapitza's return to Cambridge and instead to help him start his work in Moscow. This 'has meant a great deal of extra work, as I have to keep the Managing Committee, the University, the Royal Society, the DSIR and the USSR all in grand fettle. It is strange team to be driving along the road'.

At the end of 1935, Bohr made his important contribution to nuclear physics, the concept of the compound nucleus. He visited Cambridge early in 1936. Rutherford immediately saw the point of his idea and gave a very adequate summary in a letter to Max Born. But he was again impatient for quantitative detail. In a letter to Hevesy: 'I am looking forward to Bohr's paper on nuclear constitution... I gathered that he was publishing a more detailed account at once, but I suppose we shall have to wait, and he will take a good long time

to think matters over before the paper finally appears'.

The last letters, in 1937, deal with the arrangements for a meeting in India, to which Rutherford hoped Bohr would accompany them. He died before the date of the trip.

In a very moving letter to Bohr after Rutherford's death, Kapitza wrote 'From his words I always gathered that he liked you the most amongst all his pupils, and to be sincere I was always a little jealous of you. But now it is gone'.

It is not necessary to decide if Kapitza was right, or if there were others of whom Rutherford was equally fond, but from the letters it is clear that he and Bohr had become very close friends, and for each it had become very important to exchange views with the other about physics and about the world. Rutherford is reported to have said once that he could do research at the North Pole. This was in relation to a laboratory and equipment, but I do not believe he meant that his research would go on efficiently if he was isolated. There are, indeed, a few scientists who can work on their own without any need for personal contacts, but they are few, and they are always in danger of getting out of touch with the development of their subject. The Rutherford and Bohr type thrive on contacts. They are kept going by their own initiative, but they must share their knowledge and their discoveries with friends and colleagues.

There is no means of knowing whether our physics today would be any poorer if Bohr and Rutherford had not met, but they certainly felt sure that they were helped along by each other. I think therefore we should be grateful for this unique relationship.