Recollections of Lord Rutherford

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(Delivered and MS received 17 May 1966)

I am greatly honoured by the invitation of the President of the Royal Society to speak to you on my reminiscences of Lord Rutherford. But this is a very difficult task and I accepted it after long deliberation. At first sight I thought that to speak about the scientific achievements of so great a scientist as Rutherford would be easy. The greater the achievements of a scientist the more exactly and briefly can they be described. Rutherford created the modern study of radioactivity; he was the first to understand that it is the spontaneous disintegration of the atoms of radioactive elements. He was the first to produce the artificial disintegration of the nucleus and finally he was the first to discover that the atom has a planetary system. Each of these achievements is sufficient to make a man a great physicist. But nowadays these achievements and their fundamental values are well known not only to research students but even to schoolboys. Equally we all know the very simple and beautiful classical experiments by means of which Rutherford made his great discoveries. To come from the Soviet Union to speak about all this to the Fellows of the Royal Society would scarcely be appropriate.

You are all well aware that from research into radioactivity there grew up an independent science which is now called nuclear physics, and of all the papers published on physical research one fifth relate to the investigation of nuclear phenomena. Both nuclear energy and the use of artificial radioactivity in science and technology are developing quickly and simultaneously. All these fields absorb the main bulk of the monetary resources spent on science and which now reaches the sum of thousands of millions of pounds, dollars and roubles. And all this for the last 30 years grew out of one modest domain of physics which in the old days was called radioactivity and the father of which is justly called Rutherford. To speak of the development of nuclear technology and physics which came from the work of Rutherford and his school is very interesting and very instructive. But I am sure that such Fellows of the Royal Society as our President Professor Blackett, Sir James Chadwick, Sir John Cockcroft, Sir Charles Ellis and Sir Mark Oliphant who in the old days were the most active members of Rutherford’s school and who themselves in this domain have made fundamental discoveries and researches are certainly more qualified than I to speak on these matters.

The only way in which I can satisfy the interest of the Fellows of the Royal Society is to speak of Rutherford the man, of how I remember him during my 13 years’ work in the Cavendish Laboratory, of how he worked, how he trained us young scientists and also of his relations with the scientific world. My task is therefore to draw you a portrait of a great scientist and of a great man. Frankly, this is the job of a writer and not of a scientist. If I have now decided to do so, this is
mainly for the following reasons. When I look back and see myself as a young man coming to England in 1921 and starting work in the Cavendish Laboratory and, after 13 years, growing into a scientist, I feel that these years of my work were the happiest, and for all that I have been able to achieve I feel immensely grateful for the attention and kindness which Rutherford showed me, not only as a teacher but as a very kind and sympathetic man for whom I have a sincere affection and with whom I eventually became great friends.

However imperfect my recollections of Rutherford may be, this is the only way in which I can express my deep gratitude to this great and remarkable man.

As is well known, Rutherford was not only a great scientist but also a great teacher. I can recall no other scientist contemporary to Rutherford in whose laboratory so many outstanding physicists were trained. The history of science tells us that an outstanding scientist is not necessarily a great man, but a great teacher must be a great man. Therefore my task is even more difficult: I must give you a portrait not only of a scientist but of a man. I will attempt to make my portrait of Rutherford as alive as possible and for this purpose I shall illustrate my talk with episodes which I most vividly remember. From my many recollections I shall select the ones which characterize different sides of Rutherford's nature. I hope this will help you to reconstruct a lifelike picture of Rutherford in your imagination from all these fragments.

I would like to begin my recollections with a small episode which happened in 1930 in the Cavendish Laboratory. At that time a small conference was being held in Cambridge to commemorate the centenary of the birth of Maxwell, the first director of the Cavendish Laboratory. He was succeeded by Rayleigh, J. J. Thomson and Rutherford, four great physicists of the last and present century. After the official part of the meeting in which some of Maxwell's pupils talked of their reminiscences, Rutherford asked me how I liked the speeches. I answered that they were very interesting, but I was surprised that all the speakers spoke only of the positive side of Maxwell's work and personality and made a 'sugary extract' (sic) of him and I said that I would like to see Maxwell presented as a living figure with all his human traits and faults which of course every man possesses however great his genius. Rutherford as usual laughed and said that he charged me after his death to tell future generations what he was really like. Rutherford was joking and I was laughing too. And now when I try to fulfil his behest and I imagine Rutherford as I have to present him before you, I see that time has absorbed all his minor human imperfections and I can only see a great man with an astounding brain and great human qualities. How well I now understand Maxwell's pupils who spoke about him in Cambridge.

There are numerous books and articles on Rutherford as a scientist. It is widely recognized that the simplicity and clarity of his thinking, his great intuition and great temperament were very characteristic of his creative ability. Studying the works of Rutherford and observing how he worked I think the basic characteristics of his thinking were great independence and hence great daring.

The basic method by which science is developing consists of experimental investigations into natural phenomena and the continuous verification of the
consistency of the results of our investigations with our theoretical conceptions. The
progress of our knowledge of nature appears in cases when we find contradictions
between theory and observation, and these contradictions, as they compel us to
develop our theories, enable us to widen our knowledge of nature. The more acute
these contradictions are, the more they lead us to further fundamental changes in
understanding the laws of nature, on the basis of which we may use nature for our
cultural development. In science, as in history, definite stages of development
demand their particular kind of genius. A definite period of development requires
men with corresponding mental abilities. In the history of the development of
physics, as in any other experimental science, the most interesting periods are
those in which we are brought to revise our fundamental scientific conceptions.

Then not only deep thinking and intuition are required from the scientist but also
a daring imagination. As an illustration I shall remind you of two well known
cases in the history of physics. They made a great impression on me personally.
The first case concerns Franklin’s creation of the study of electricity. On the basis
of this study Franklin stated that electricity has a material origin and can im­
pregnate metal and freely move in it. In his day this concept was in fundamental
contradiction to the concept of the continuous nature of matter. But Franklin’s
view was eventually accepted as it gave a simple and complete explanation of all
the electrostatic phenomena observed in his day. It is only recently, 150 years later,
after J. J. Thomson discovered the electron, that Franklin’s concepts were com­
pletely justified. But the most striking thing in this story is how it could have
happened that Franklin, who had never before done any scientific work, could, in
the course of a few years in a small remote American town when he was already a
middle-aged man, find the right way by which this most important branch of
science should be developed. And this happened in the middle of the eighteenth
century when science was developing on the level of Newton, Huygens and Euler.
How could Franklin achieve such results which were beyond the reach of pro­
fessional scientists?

The other similar case in which the fundamental concepts of electricity had to
be revised in the light of experiments is also well known. This is Faraday’s concept
on the electrical field. It is difficult to find a more revolutionary and original idea
than Faraday’s. He advanced the concept that electrodynamical processes must
be explained by the phenomena happening in the space surrounding the conductor.
I mention this case mainly because Faraday was a scientist who had no traditional
scientific education, even though at that time its level was high for an average
English scientist. I mention these two well known cases only to show that at a
particular stage of the development of science, when new fundamental concepts
have to be found, wide erudition and conventional training are not the most im­
portant characteristics of a scientist required to solve this kind of problem. It
appears that in this case imagination, very concrete thinking and, most of all,
daring are needed. Strict logical thinking which is so necessary in mathematics
hinders the imagination of a scientist when new fundamental concepts must be
found. The ability of a scientist to solve such scientific problems without showing a
logical trend of thought is usually called ‘intuition’. Possibly there is a way of
thinking which takes place in our subconscious but the laws by which it is governed are at present unknown. If I am not mistaken, even Freud, a pioneer in the study of subconscious processes, was not aware of it. But if intuition exists as a powerful creative thinking process then doubtless Franklin and Faraday mastered it thoroughly. I am sure that Rutherford mastered it too and he has rightly been called the Faraday of our time.

When at the beginning of our century Rutherford started studying radioactivity it had already been proved experimentally that these phenomena contradicted the most fundamental law of nature, the law of conservation of energy. The explanation of radioactivity which Rutherford gave, namely the disintegration of matter, at once provided not only the key to the understanding of these phenomena but also led all investigation in the right direction. The same thing happened when Rutherford created the planetary model of the atom. At first sight this model completely contradicted laws of classical electrodynamics since in its circular motion an electron was perpetually bound to lose by radiation its kinetic energy. But the experiments of scattering the $\alpha$-particles, performed by Rutherford’s pupil Marsden in 1910, definitely showed the existence of a heavy nucleus in the centre of the atom and Rutherford imagined the collision of particles so clearly that even these contradictions with the fundamental laws of electrodynamics could not prevent him from establishing the planetary structure of the atom. We know that only three years later Bohr, on the basis of the developing quantum theory of light, evolved his brilliant theory of the structure of the atom which not only justified Rutherford’s planetary model but also quantitatively explained the spectra of atomic radiation.

The peculiar character of Rutherford’s thinking could easily be followed when talking to him on scientific topics. He liked being told about new experiments but you could easily and immediately see by his expression whether he was listening with interest or whether he was bored. You had to talk only about fundamental facts and ideas without going into the technical details in which Rutherford took no interest. I remember, when I had to bring him for approval my drawings of the impulse generator for strong magnetic fields, for politeness sake he would put them on the table before him, without noticing that they were lying upside down, and he would say to me: ‘These blueprints don’t interest me. Please state simply the principle on which this machine works.’ He grasped the basic idea of an experiment extremely quickly, in half a word. This struck me very much, especially during my first years in Cambridge, when my knowledge of English was poor and I spoke it so badly that I could only vaguely explain my ideas, yet in spite of this Rutherford caught on very quickly and always expressed very interesting opinions.

Rutherford also liked talking about his own experiments. When he was explaining something he usually made drawings. For this purpose he kept small bits of pencil in his waistcoat pocket. He held them in a peculiar way—it always seemed to me a very inconvenient one—with the tips of his fingers and thumb. He drew with a slightly shaky hand, his drawings were always simple and consisted of a few thickly drawn lines, made by pressing hard on the pencil. More often than not the point of the pencil broke and then he would take another bit from his pocket.
A number of physicists, especially theoreticians, like to discuss science and apparently the process of argument is a way of thinking. I never heard Rutherford argue about science. Usually he gave his views on the subject very briefly, with the maximum of clarity and very directly. If anybody contradicted him he listened to the argument with interest but would not answer it and then the discussion ended.

I greatly enjoyed Rutherford's lectures. I followed the course of general physics which he gave to the undergraduates as Cavendish Professor. I did not learn much physics from this course since by that time I already possessed a fair knowledge of the subject, but from Rutherford’s approach to it I learnt a great deal. Rutherford delivered his lectures with great enthusiasm. He used hardly any mathematical formulae, he used diagrams widely and accompanied his lectures with very precise but restrained gestures from which it could be seen how vividly and picturesquely Rutherford thought. I found it interesting that during the lecture he changed the topic as his thoughts, probably following some analogy, turned to a different phenomenon. This was usually connected with some new experiments made in the field of radioactivity which fascinated him and he then proceeded to speak with enthusiasm on the new subject. In this case he usually put his assistant in a difficult position by asking him to give a demonstration which was not part of the original planned version.

About the same time I also attended the lectures of J. J. Thomson in his special course on the conductivity of electricity through gas. It was interesting to notice how differently these two great scientists approached scientific problems. If Rutherford’s way of thinking was inductive, then the way of thinking of J. J. Thomson was deductive.

I think it useful when training young scientists to ask them to follow a course of lectures, even an elementary one, but delivered by an eminent scientist. Listening to these lectures they will learn something that they will never find in any textbooks. In this connexion I remember a conversation which I had with Sir Horace Lamb. He was telling me how he had attended Maxwell’s lectures. Maxwell, he said was not a brilliant lecturer; he usually came to lectures without any notes. When he was doing mathematics on the blackboard he often made mistakes and sometimes got muddled. From the way in which Maxwell tried to disentangle and correct his mistakes Lamb learned more than from any textbooks he ever read. Lamb told me that for him the most precious parts of Maxwell’s lectures were those in which he made mistakes. No doubt the mistakes of a genius are sometimes as instructive as his achievements.

When I came to Cambridge Rutherford did no more experimental work by himself; he worked chiefly either with Chadwick or with Ellis. But in both cases he took an active part in experiments. The setting up of the apparatus was done mainly by his laboratory assistant, Crow, whom he treated rather severely. But I sometimes saw how Rutherford himself, despite his slightly shaking hands, dealt quite skillfully with the finewalled glass tubes filled with radium emanation. Although Rutherford’s experiments are well known, I cannot refrain from saying a few words about them. The most attractive thing about these experiments was the clarity of setting the problem. The simplicity and directness of approach to the
solution of the problem were most remarkable. From my long experience as an experimenter I have learned that the best way of correctly evaluating the capacity of a beginner as well as of a mature scientist is by his natural inclination and ability to find a simple way of solving problems. There is an excellent saying by an unknown French author which applies perfectly to Rutherford: ‘La simplicité c’est la plus grande sagesse.’ I should also like to quote the profound saying of a Ukrainian philosopher, Gregory Skovoroda. He was by origin a peasant and lived in the second half of the eighteenth century. His writings are most interesting but probably quite unknown in England. He said ‘We must be grateful to God that He created the world in such a way that everything simple is true and everything complicated is untrue.’ Rutherford’s finest and simplest experiments concerned the phenomena of scattering by nuclear collisions. The methods of observation of scintillations by counters were worked out by Rutherford in collaboration with Geiger in 1908. Since then more than half a century has passed and this method and the Wilson chamber invented about the same time remain the fundamental methods for studying nuclear phenomena, and only the optical and resonance methods for determining nuclear moments have since been added. And up to now all nuclear physics possesses no experimental possibilities other than those used by Rutherford and his collaborators. The present development of nuclear physics is proceeding not by the invention of new experimental possibilities of investigating nuclear phenomena but thanks to the possibility of investigating nuclear collisions of a larger number of elements; and these collisions are studied in the domain of larger energies which are reached mainly by the use of powerful modern accelerators. But even now the way which leads us to the knowledge of the nucleus is still the method discovered by Rutherford, and he was the first to appreciate its fundamental value. I am referring here to the investigation of the collision of nuclei. Rutherford always liked to say ‘Smash the atom!’.

Even now, in the process of investigating nuclear collisions, there is one great weakness: the necessity of using statistical methods in the interpretation of experimental results. Great care is required to deduce correct general laws from limited statistical data. Someone once said about statistics: ‘There are three kinds of lies: a simple lie, an impudent lie and statistics.’ In fact this was said about the application of statistics to social problems. But to some extent it is true of statistics in physics. I do not think that in any other branch of physics so many mistakes and faulty discoveries were made as in the course of the interpretation of statistical data obtained from experiments on nuclear collisions. Nearly every year new particles of resonance levels are still discovered, some of which may not exist. Rutherford was well aware what danger lies concealed in the interpretation of experimental data of statistical origin, especially when the scientist anticipates definite results. Therefore Rutherford was very careful to exclude the personal element and took the following precautions during the course of these experiments: the counting of scintillations was usually done by undergraduates who did not know the purpose of the experiment; the curves were drawn by persons who did not know what results were expected. As far as I remember, Rutherford and his pupils never made a single such mistake, while in the same line of investigation a number
of mistakes were made in other laboratories. I remember that in those days the most critical approach in the interpretation of statistical data was that of Chadwick on whose judgement Rutherford usually relied completely.

I did not work with Rutherford because my investigations were not connected with nuclear physics and therefore I did not see him working in his laboratory. But I know that up to the very end of his life the main bulk of his time was taken up by his personal scientific research. I expect he gave the same amount of attention and strength to directing the work of young research pupils working in the Cavendish Laboratory. The detailed guidance of scientific work he left to one of the senior scientific workers, usually Chadwick. But he himself always took an interest in the choice of problem for experiment and of the experimental approach. Until the research student began obtaining results Rutherford showed no marked interest in his work. He never bothered about detailed guidance.

He often came to the laboratory but only for a short time; just to make remarks like: ‘Why don’t you get a move on—when are you going to get some results?’ When I started working in the Cavendish Laboratory such remarks made a great impression on me, especially as they were made in a thundering voice and with a severe expression. But eventually I found out that such utterances were automatic, maybe customary for a New Zealand farmer who when going through the fields found it useful to stimulate the workers with a few ‘kind’ words. That it was actually so was proved by an episode which happened a few years later in the Cavendish. One day it was necessary to break a hole through a stone wall to put through a cable needed for some experiment. The work was urgent and it happened that at that time there was a building strike and it was exceptionally difficult to find a bricklayer who would consent to work. Finally a man was found and he started work but after a while he came and said that he refused to go on. When asked why, he replied that twice a gentleman had passed by him and both times had asked him when he would finally start work and get the job done. These remarks offended the workman. When asked who this gentleman was, his description showed without a doubt that it was Rutherford. When we reproachfully pointed out to Rutherford that during a strike one should be a little careful we were surprised that Rutherford denied having said anything to the bricklayer. Obviously when he likewise grumbled at us in the laboratory for our slow work, he did it unconsciously; it was a kind of conditional reflex.

The greatest quality of Rutherford as a teacher was his ability to direct research work in the right direction, then to encourage the beginner and to give just appraisal of his achievements. What he valued most in a pupil was independent thought and originality in his work. Rutherford did his utmost to develop in his pupil an individuality. I remember how in the first years of my work in the Cavendish I once said to Rutherford: You know that the work of X is pretty hopeless; don’t you think he’s wasting his time and apparatus?’ Rutherford replied that he too knew that the man was working on a hopeless problem, ‘but’, said Rutherford, ‘it is a problem of his own and even if the work cannot be accomplished it will lead him to another original research problem which will be successful’. The future showed that Rutherford was right. As I said, Rutherford would do his utmost to
develop in his pupils independence and originality of thought and as soon as a pupil showed these qualities Rutherford would pay close attention to his work. As an example of Rutherford’s ability to direct the research of his pupils I remember the story, as Rutherford told it himself, of the discovery made by Moseley. In 1912 Moseley worked with Rutherford in Manchester. He was very young and Rutherford spoke of him as one of his best pupils. When Moseley came to Manchester he at once accomplished some minor research work and then eventually he came to Rutherford and told him of three different topics he would like to investigate. One of these researches was the classical work which had made Moseley’s name so well known—the dependence of the wavelengths of Roentgen rays on the position of atoms in the periodical system. Rutherford at once advised Moseley to choose this work for his investigation. The future showed that Rutherford made the right choice, but he always pointed out that the idea of the experiment belonged to Moseley.

Rutherford was very particular to give credit for the exact authorship of any idea. He always did this in his lectures as well as in his published works. If anybody in the laboratory forgot to mention the author of the idea Rutherford always corrected him. He was also very particular not to give a beginner technically difficult research work. He reckoned that, even if a man was able, he needed some success to begin with. Otherwise he might be disappointed in his abilities, which could be disastrous for his future. Any success of a young research worker must be duly appreciated and must be duly acknowledged.

Once, in one of our outspoken talks, he told me that the most important thing a teacher must learn is not to be jealous of the successes of his pupils—which is not so easily done as the teacher gets older! This profound truth made a great impression on me. No doubt the greatest quality of a good teacher should be generosity. Rutherford was undoubtedly very generous and I think this is one of the main secrets which explains why so many first-class scientists came from his laboratory. There was always an atmosphere of freedom and efficiency there.

Rutherford well understood the importance that his pupils had for him. It was not merely that young research students increased the scientific productivity of the laboratory, but, as he said, ‘My pupils keep me young’. This is very true, since pupils do not permit a teacher to lag behind new achievements in science. How often do we notice that when a scientist is ageing he starts opposing new ideas and underestimates the significance of new trends in science. Rutherford, with great ease and generosity, always accepted new ideas in physics like wave quantum mechanics, while a number of distinguished scientists of his generation were sceptical of the same ideas. Such conservatism is characteristic of scientists who work by themselves without having pupils to be directed and encouraged.

Rutherford was very sociable and loved talking to the scientists who came to visit him and the Cavendish Laboratory. Usually there were many such visitors. His attitude to other people’s work was kind and considerate. In conversation Rutherford was very lively; he was fond of jokes and often made them himself. He laughed easily, his laughter was sincere, loud and infectious. His face was very expressive: you could see at once what mood he was in, good or bad, or whenever
Rutherford in the Cavendish laboratory for high magnetic fields.

A meeting of the governing board of the Magnetic Laboratory: Rutherford (right), Cockcroft (left), Kapitza (centre).

(Facing p. 136)
Two views of Rutherford and Cockcroft standing in front of the high power impulse generator for producing high magnetic fields, taken about 1930.
he was worried by anything. You always knew he was in a good temper when he
good-naturedly teased the person he was talking to. The more he teased him, the
more he liked him. This was particularly noticeable when he talked to Bohr or to
Langevin to both of whom he was especially attached. His kindest jokes often
concealed a deeper sense. I remember one occasion when he brought Professor
Robert Millikan to my room in the laboratory. Rutherford said to me, ‘Let me
introduce you to Millikan; no doubt you know who he is. Show him your installa­
tion to produce strong magnetic fields and tell him about your experiments. But
I doubt whether he will let you speak as he himself will tell you about his own
experiments!’ There followed loud laughter in which Millikan joined with rather
less enthusiasm. Rutherford then left us, and I soon found out that his prophecy
was correct.

I shall not describe the way in which Rutherford read his papers. I always liked
them very much as regards both their content and their exposition. He attached
great importance to the way in which his papers were presented and evidently
prepared them very carefully. He taught me how to read papers to the Royal
Society, and one of his instructions I still remember very clearly: ‘Don’t show too
many slides. When it is dark in the lecture room some of the audience take the
opportunity to leave!’

Rutherford’s interests were not limited narrowly to physics; they were much
wider. He was well read, he liked books on geography and history and liked to
discuss what he had read. He absorbed all knowledge enthusiastically and always
extracted the essentials.

Later on, when I became a Fellow of Trinity and used to accompany him home
after dinner on Sundays, we often discussed politics. On the first day I started
work in the Cavendish I was surprised to hear him saying to me that in no circum­
stances would he tolerate my making Communist propaganda in his laboratory.
At this time this remark came quite unexpectedly. It not only surprised me, but
also shocked me and to a certain extent even offended me. Undoubtedly it was a
consequence of the current atmosphere of acute political struggle and was con­
nected with the propaganda which existed in those days, only four years after the
Russian revolution. Before coming to England, I was so absorbed by my research
work in Russia that I was completely unaware of what was happening in Western
Europe and could not appreciate the scale of the bitter political controversy which
then existed. Later on when my first experimental research was published I pre­
sented Rutherford with a reprint and I made an inscription on it that this work
was proof that I had come to his laboratory to do scientific work and not to make
Communist propaganda. He got extremely angry with this inscription, swore and
gave me the reprint back. I had foreseen this and I had another reprint in reserve
with an extremely appropriate inscription with which I immediately presented
him. Obviously Rutherford appreciated my foresight and the incident closed.
Rutherford had a characteristically hot temper but cooled down just as quickly.
Eventually we had many conversations on political questions; we were especially
concerned about the growth of fascism in Europe. Rutherford was an optimist and
thought that all would soon be over. We now know that this was not the case.
Rutherford, like most scientists who work in the exact sciences, had progressive political views. I involved Rutherford in some political activity on two occasions.

The first of these was connected with Langevin. In his younger days Rutherford had worked with Langevin in the same room at the Cavendish. A deep friendship developed between them. Indeed it was practically impossible not to be friendly with a man of such brilliant intelligence and exceptional moral qualities. In Paris my friends, pupils of Langevin, were greatly shocked that Langevin, undoubtedly the best French physicist, had not been elected to the French Academy as a result of his left wing political views. Langevin had taken part in a number of progressive organizations, had been the founder of the League of the Rights of Man (ligue des droits d'hommes) and had fought anti-semitism in the Dreyfus case. I told Rutherford of the difficulties Langevin had encountered in France and asked him whether a man who held such leftist views as Langevin could be a Foreign Member of the Royal Society. Rutherford said something I could not quite follow, then started to tell me what a really good man Langevin was, and then recalled that during the war Langevin had been very active in inventing supersonic beams propagated in water by which he had established communication between England and France across the Channel. At this point the conversation ended. I learnt later that at the next election in 1928 Langevin was elected a Foreign Member of the Royal Society and this was much earlier than his election to the French Academy.

The second example occurred much later, when Hitler started to come into power. We were very anxious about the fate of such distinguished physicists as Stern, Frank, Born and a number of others in the conditions of active and increasing anti-semitism in Germany. About this time Zillard came to England and we were faced with the question of how to get these scientists out of Germany without raising suspicion. I spoke to Rutherford and he was very willing to help, writing personal letters to these scientists, and inviting them to come to lecture in Cambridge.

Rutherford took an interest in a great variety of people, but he particularly liked people with strong personalities. When Rutherford was elected President of the Royal Society and often had to attend dinner parties with distinguished politicians, businessmen and statesmen, he was fond of telling stories afterwards about the conversations he had had with them and always gave descriptions of them. I specially remember that Churchill made a great impression on Rutherford. His description of Churchill was, like all his descriptions, short and clear, and in due course I found out that it was quite correct. I well remember that Churchill in those days already regarded Hitler as a real danger to peace and called him 'a man riding a tiger'. Possibly this conversation somewhat altered Rutherford's optimistic view of the future. Rutherford's interest in understanding human psychology and his kindness to others was undoubtedly felt by them. This explains why Rutherford's excessively direct way of speaking which was sometimes not very tactful, was completely compensated for by his kindness and cordiality.

Of course Rutherford's correct evaluation of people and his understanding of them was due to the fact that he was a subtle psychologist. People interested him and he had the faculty of understanding them. His assessments of people were
always very outspoken and direct. As in his scientific work, his description of a man was always brief and very accurate. I was always convinced that his descriptions were correct. Possibly his approach to people was also a subconscious process and could be called intuitive.

I should like to illustrate his interest in psychology with the following two episodes. In Cambridge there was a small but progressive theatre which produced Chekov's play 'Uncle Vanya'. Rutherford went to this play and was greatly taken with it. As in all Chekov's works, it deals with a psychological problem complicated by the fact that all the people in the play are highly intellectual and therefore their acceptance of life is very complex. In the play a certain retired professor comes to live on the estate of his wife. Uncle Vanya, who manages the estate, has devoted his whole life to supporting the professor. Soon Uncle Vanya finds that the professor is a fake celebrity, scholastic and pedantic in his work. Against a background of complex psychological situations Uncle Vanya fires a pistol at the professor but misses him. I remember how vividly, clearly and simply Rutherford told me this plot and his sympathy was completely on Uncle Vanya's side. The fact that Rutherford was so attracted by the play shows that he undoubtedly enjoyed disentangling complicated psychological cases of this kind.

A great impression was made on me by the following case which demonstrates Rutherford's skill in handling complicated psychological problems. I think enough time has now passed and I can tell you about this case which involved the then well known physicist, Paul Ehrenfest.

Ehrenfest was born in Austria. On one of his mountaineering excursions he met a Russian woman scientist and followed her to Russia where he married her. In Russia he published a number of outstanding theoretical works on thermodynamics. Eventually he was invited to Leiden University to take the chair of theoretical physics vacated by the great Lorentz, creator of the electronic theory of metals and one of the founders of the theory of relativity. In Leiden Ehrenfest and his house became one of the world centres of theoretical physics. Ehrenfest's main quality was his precise critical mind. He was not only a very good teacher of young scientists, who were very fond of him, but his criticisms were regarded as profound and of such high quality that leading theoretical scientists like Einstein and Bohr often came to Ehrenfest to discuss their work. Ehrenfest always noticed even the smallest contradiction or mistake. His critical remarks were made very readily, with great spirit and even sharply, but always very good-naturedly. The quality of his criticism was greatly appreciated. Despite our difference in age we became friends and I often visited his very hospitable and very charming family and more than once was present at his scientific discussions.

Ehrenfest's exceptionally strong critical mind evidently acted as a restraint on his creative imagination and he did not succeed in producing scientific work which he himself would have considered of sufficiently high standard. In those days I did not know that in his acute nervous condition Ehrenfest suffered greatly when he could not in his work attain the level of the friends he criticized. I learnt about his feelings in the following manner. In the beginning of 1933 I received a long letter from him in which he described in detail his state of mental depression.
and spoke of the futility of his achievements. He had come to the conclusion that it was not worth living any more. The only way to save himself, he thought, was to leave Leiden and settle somewhere away from his friends. He asked me to help him to find a chair at some small university in Canada and to ask Rutherford, who doubtless had connexions in Canada, to assist him. I was, of course, very upset by this letter. We all liked Ehrenfest and all knew that his influence as a teacher and critic in the development of modern physics was colossal. I translated the letter from German into English and came to Rutherford, who had little personal acquaintance with Ehrenfest. I handed Rutherford the letter and told him that we were very worried about Ehrenfest’s future as, without any doubt, the letter showed that he was mentally unbalanced; perhaps, I said, this state was only temporary and everything possible should be done to help him out of his state of depression. Rutherford said I must not worry and he would handle the case himself. I do not know what Rutherford wrote to Ehrenfest but shortly afterwards I received a letter telling me that he was once again in a happy frame of mind. He said that Rutherford had explained what a great role he was playing in physics and he added that of course there was now no need for him to go to Canada. This story shows how skilfully Rutherford dealt with a very complicated psychological case, probably better than a professional psychiatrist.

A few months later, while I was on a visit to Russia, the state of depression returned to Ehrenfest and on 25 September 1933 he committed suicide.

I should now like to recall quite a different and rather amusing case characteristic of Rutherford’s attitude to the young. Once Rutherford called me into his study and I found him reading a letter and roaring with laughter. It appeared that the letter was from some Ukrainian schoolboys. They had written to say that they had organized a physics club and were proposing to continue Rutherford’s fundamental work on the study of the nucleus of the atom and ask him to be an honorary member and to send them reprints of his scientific work. In the part of the letter in which they described Rutherford’s achievements in nuclear physics, instead of using the correct term in physics they used a corresponding term which in slang has a different meaning. In this way the description of the structure of the atom acquired a property of the living organism. Its character is such that one does not speak about it in polite society, and it made Rutherford laugh heartily. I explained to Rutherford that the schoolboys were apparently not very well versed in English and the writing of the letter was mostly done with the use of a dictionary and the mistake was bona fide. Rutherford said that he appreciated this. He sent the boys a reply, thanking them for the honour of being elected a member of the club and promising to send them reprints.

Finally, I should like to discuss a question I have come across several times in descriptions of Rutherford’s activities. The question is: did Rutherford foresee the great practical consequences which would emerge from his scientific discoveries and investigations into radioactivity?

The immense reserves of energy which are hidden in matter was understood by physicists a long time ago. The development of his view took place side by side with the development of the theory of relativity. The question which was not
clear at that time was: would it eventually be possible to find technical means of making practical use of these reserves? We know now that the actual possibility of obtaining energy from nuclear collisions was becoming more and more real as nuclear phenomena were better understood. But up to the last moment it was not certain whether it would be technically possible to produce nuclear reactions with a great yield of energy. I remember only rare occasions on which I discussed this question with Rutherford and in all these conversations he expressed no interest in it. From the beginning of my acquaintance with Rutherford I noticed that he never took any interest in technical problems and I even had the impression that he was prejudiced about applied problems. Possibly this was because such problems were connected with business interests.

I am by training a chartered engineer and naturally I always took an interest in solving technical problems. During my stay in Cambridge I was approached several times to help in solving technical problems in industry. In these cases I used to take advice from Rutherford and he always said to me; ‘You cannot serve God and Mammon at the same time.’ Of course he was right. Once I remember Rutherford telling me about Pupin who as an able young physicist had come to Cambridge and done successful scientific work in the Cavendish Laboratory. Pupin was somewhat senior to Rutherford so they met only occasionally. Eventually Pupin turned to commercial activity in the U.S.A. and made a lot of money. Rutherford spoke disapprovingly of Pupin’s activities. So I think that Rutherford’s opinions on the practical applications of nuclear physics had no real value as they lay outside the scope of his interests and tastes.

In connexion with Rutherford’s views on industry I remember a conversation I had with him during a high table dinner at Trinity College. I do not remember how the conversation started, maybe it was under the influence of Lombroso’s book, *Genius and Madness*. I was telling my neighbour that every great scientist must be to some extent a madman. Rutherford overheard this conversation and asked me, ‘In your opinion, Kapitza, am I mad too?’

‘Yes, Professor’, I replied.

‘How will you prove it?’ he asked.

‘Very simply’, I replied. ‘Maybe you remember a few days ago you mentioned to me that you had had a letter from the U.S.A., from a big American company. (I do not remember now which one it was, possibly General Electric Co.) In this letter they offered to build you a colossal laboratory in America and to pay you a fabulous salary. You only laughed at the offer and would not consider it seriously. I think you will agree with me that from the point of view of an ordinary man you acted like a madman!’ Rutherford laughed and said that in all probability I was right.

The last time I saw Rutherford was in the autumn of 1934 when I went as usual to the Soviet Union to see my mother and my friends and unexpectedly was deprived of the possibility of returning to Cambridge. I did not hear his voice again, nor hear him laugh. For the next three years I had no laboratory to work in and was unable to continue my scientific work and the only scientist with whom I freely corresponded outside Russia was Rutherford. At least once every two months he
wrote me long letters which I greatly valued. In these letters he gave me an account of life in Cambridge, spoke about the scientific achievements of himself and his pupils, wrote about himself, made jokes, gave good advice and invariably cheered me up in my difficult position. He understood that the important thing for me was to start my scientific work which had been interrupted for several years. It is no secret that it was only due to his intervention and help that I was able to obtain the scientific installation and apparatus of the Mond Laboratory and in three years time I was able to renew my work in the domain of low-temperature physics.

I am sure that in the course of time all Rutherford's letters will be published but even so I should like here and now to quote three short extracts which require no comment.

On 21 November 1935 he wrote:

'... I am inclined to give you a little advice, even though it may not be necessary. I think it will be important for you to get down to work on the installation of the laboratory as soon as possible, and try to train your assistants to be useful. I think you will find many of your troubles will fall from you when you are hard at work again, and I am confident that your relations with the authorities will improve at once when they see that you are working wholeheartedly to get your show going. I would not worry too much about the attitude or opinions of individuals, provided they do not interfere with your work. I daresay you will think I do not understand the situation, but I am sure that chances of your happiness in the future depend on your keeping your nose down to the grindstone in the laboratory. Too much introspection is bad for anybody!...'

On 15 May 1936 he wrote:

'... This term I have been busier than I have ever been, but as you know my temper has improved during recent years, and I am not aware that anyone has suffered from it for the last few weeks!...

'... Get down to some research even though it may not be of an epoch-making kind as soon as you can and you will feel happier. The harder the work the less time you will have for other troubles. As you know, "a reasonable number of fleas is good for a dog"—but I expect you feel you have more than the average number!'

You see what short and clear and fatherly advice he gave me. The last letter is dated 9 October 1937. He wrote in great detail about his proposed journey to India. In the last part of the letter he said: '... I am glad to say that I am feeling physically pretty fit, but I wish that life was not quite so strenuous in term time...'

Ten days before his death he did not feel that it was so near.

For me the death of Rutherford meant not only the loss of a great teacher and friend; for me, as for a number of scientists, it was also the end of a whole epoch in science.

Obviously we should attribute to those years the beginning of the new period in the history of human culture which is now called the scientific-technical revolution. One of the greatest events in this revolution has been the use of atomic energy. We all know that the consequences of this revolution may be very terrible—it may destroy mankind. In 1921 Rutherford warned me not to make any Communist propaganda in his laboratory, but it now appears that just at that time he himself
together with his pupils were laying the foundations for a scientific-technical revolution.

We all hope that in the end people will have sufficient wisdom to direct this scientific revolution to the benefit of humanity.

But nevertheless the year that Rutherford died there disappeared forever the happy days of free scientific work which gave us such delight in our youth. Science has lost her freedom. Science has become a productive force. She has become rich but she has become enslaved and part of her is veiled in secrecy.

I do not know whether Rutherford would continue nowadays to joke and laugh as he used to do.