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# WILHELM CONRAD RÖNTGEN

## PROFESSOR OF PHYSICS

## 1845—1923

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A scientist has probably never passed so suddenly from the seclusion of serious study to world celebrity and popularity as did Röntgen when he discovered his rays at Würzburg in 1895. He had determined their presence in a few weeks of truly classical research. He was well aware that he had discovered something perfectly new: thanks to his astonishing knowledge of the literature and his brilliant memory, he could be sure of that. But he had not yet recognized the essential nature of his rays. For this reason he used the unknown "X" to denote them. After his lecture upon them before the Würzburg Physikalisch-medizinische Gesellschaft, the anatomist Kölliker gave them the name of "Röntgen rays", which however has not become current among all foreign scientists, although a doubt can hardly be entertained any more about their nature.

Wilhelm Conrad Röntgen was born at Lennep, in the Rhine province, on March 27, 1845. He was the only child of a German father and a Dutch mother. His father was an industrialist who worked at first in the Rhineland, and then in Holland. Röntgen consequently received his first education in Dutch schools, at Utrecht. He spent a happy boyhood there.

Röntgen was reticent about his private life. As far as I know, he has left no memoirs. The reader must therefore excuse me if, in compliance with the request to give the story of his career and in order to describe his character, I find myself obliged to relate various incidents in which I play a part. It was only in the last years of his life that one or two friends learned a few details concerning his youth. One of these friends, E. Wölfflin, gives us the following account, based on Röntgen's own statements: "After the first years at school had passed smoothly, he entered the Gymnasium *) at Utrecht, where later he was to see plainly the dangerous rocks in his course, on which many capable natures had already made shipwreck before him. As a result of some harmless school-boy trick, all the boys of the class were subjected to a searching cross-examination. Röntgen admitted that he was the ringleader (although actually he had not been), but could not bring himself to denounce two of his class-mates as being also guilty. The consequence was that he was obliged

*) Gymnasium corresponds roughly to the English Public School. The education is chiefly classical. The Maturität (Absolutorium) examination is held by the authorities of the Gymnasium, and at the time when Röntgen was at school, it was essential to pass this examination in order to be able to enter a university.

to leave the Gymnasium after he had already reached the highest class but one. It consoled him a little in this misfortune that his mother thoroughly approved of his behaviour at the cross-examination. A similar episode cut short the scholastic career of the Swiss poet Gottfried Keller, and was a sore point with him during the whole of his life.

"What was to be done now? His father was then informed that his son would be admitted to a so-called Privatabsolutorium, which would enable him to enter the university.

"All seemed to be going well. But on the day before the examination the teacher by whom it was being conducted and who was favourably disposed to Röntgen, was taken ill. He was replaced by a strange teacher, and Röntgen was failed. The second attempt to enter upon an academic career had thus come to nothing. There seemed to be no more hope.

"But by a curious working of destiny Röntgen now got to know a young Swiss, named Thormann, who told him that he could attend the Polytechnic at Zürich even though he had not passed his Matura. Röntgen joyfully accepted this proposal, and in 1865 went to Zürich to begin his academic studies."

On the occasion of Röntgen's 70 th birthday A. Sommerfeld writes that Röntgen also attended the school for the construction of machines at Apeldoorn in Holland. Clearly Röntgen had a special fondness for the subject of machine construction. When he was allowed to smoke, his uncle gave him a meerschaum cigar-holder and told him to colour it nicely. For this purpose his father's cigars were at his disposal. But the colouring of the holder took too long for his patience, so he constructed a sucking-pump to smoke cigars, by means of which the desired end was attained in a short time. But it may be doubted whether his father was altogether pleased with the rapid diminution of his stock of cigars!

Speaking of his time as a student at Zürich, Röntgen told me that he had never been a regular attendant at the lectures and practical courses in machine construction. The beautiful lake and the surrounding mountains were too tempting. Rowing on the lake was his special amusement, as well as mountain-climbing. The neighbouring Uetliberg could be quickly climbed, and for his ascents Röntgen generally chose the shortest way, the "Leiterli", or little ladder, as it is called, a very narrow path with primitive steps that passes through a clayey wood along the edge of wooded and very steep clay slopes. On one ascent he slipped and fell, and injured himself so seriously that he could not move, but by a lucky chance he was found and taken to the Zürich hospital. This experience made him more careful in his climbing, and later he accomplished great feats, and also gave me useful advice. He climbed Bernina, Roseg and many other of the more difficult peaks, as if it was nothing very wonderful.

In order to conclude his studies at the Zürich Polytechnic in the regular way, he entered his name for the diploma examination. His professors however had remarked a long time ago that he was not a constant attendant at their lectures and



practical courses. Röntgen told me that one of them said to him quite bluntly that he would never pass the examination. Röntgen thereupon determined to make up the ground he had lost and to prove to his professors that he did deserve the diploma; and so he gained his diploma as mechanical engineer.

Sommerfeld goes on to say: "At Zürich he was fascinated by the conceptual clarity and the conscientiousness of Clausius' lectures on the mechanical theory of heat. To this day Röntgen feels vividly that questions of technical application have a value as great as those of pure science, and he is always attracted by the plastic method of working of the technician, both in the written word and in figures. But his mind was so set upon extreme accuracy, upon determining the last decimal place, in practical work, and upon a corresponding exactitude in theoretical treatment, that he could not find satisfaction in the necessarily more rough methods of technical work. He therefore graduated as doctor of philosophy (June 22, 1869), and became assistant to Kundt. Like so many other German physicists, he cherishes his memory with gratitude."

Röntgen also told me that after obtaining his diploma he again rowed industriously on the lake of Zürich, and went on shorter and longer mountaineering expeditions. One day Kundt met him, and asked him what he was working at now. "Nothing", was the gist of Röntgen's answer, whereupon Kundt asked him to do experimental work in his laboratory, which Röntgen did. At that time Schneebeli and another young physicist (F. Exner?) were already working in the laboratory. Röntgen related to me the following story about them. These two physicists once made a remarkable discovery. They were working with sensitive magnetic needles, with a magnetometer. It is known that owing to disturbances through the magnetism of the earth these needles are liable to violent oscillations which may even become a rapid trembling. The physicists observed such disturbances, but curiously enough they were much greater than had ever been seen before, and — which was the most remarkable feature — they occurred only at noon, between 12 and 1. Clearly they had made a great discovery! Day after day the observations were strictly registered. On one occasion one of the two observers was delayed, and it was not till after 12 that he approached the laboratory building. What did he see? A workman, sitting on the window-sill and plying his knife and fork, was eating his midday meal. Within the laboratory, in the immediate vicinity of the same window, the shutters of which were closed to darken the room, stood the magnetometer of the two discoverers, and its needle reacted to the movements of the workman's knife and fork. The cause of the mysterious discovery was found. This experience was not published, although it was very instructive for budding physicists and certainly also for Röntgen. It made him careful in the announcement of new scientific finds.

In the Obere Zäune at Zürich was the restaurant known as Zum Grünen Glas (The Green Glass), much esteemed by students. It was kept by a German, named Ludwig, who had had a university education and at the time of revolution of 1848 had fled from Germany with Kinkel and others, and sought refuge in Zürich. In

his spare time Ludwig translated their dissertations for students who did not know enough Latin, the use of this classical tongue being then still compulsory for such treatises. In addition, he was a fencing master. For these reasons he was a very popular figure among the students, and his Green Glass was one of their favourite resorts. Röntgen was also a frequent visitor, and fell in love with the proprietor's daughter, Bertha. He married her shortly afterwards, having been appointed assistant by Kundt when the latter was offered the chair at Würzburg in 1870. In 1872 Kundt moved to Strassburg, taking Röntgen with him.

While at Würzburg, Röntgen published two scientific papers, of which P. P. Koch writes: "Röntgen's first papers, which appeared in the early seventies of last century and dealt with the determination of the relation of the specific heats of gases, already show that he possessed the essential qualities of the research scientist in the most marked degree — the fine flair in the realization of the problem, the experimental skill in carrying out the examination, the keen critical sense which took into account to the greatest possible extent the sources of error and their effect upon the result and thus obtained within the limits of possibility a sharply defined measure of certainty in the final answer". And indeed these papers attracted a great deal of notice among physicists on account of the acute intelligence displayed in the arrangement of the experiments and on account of the precision of the results, which for many years were not improved upon by other experimenters.

It goes without saying that Kundt esteemed more and more Röntgen's brilliant qualities as physicist, and he therefore took him with him when after two years at Würzburg he was offered the chair at Strassburg. This change of university was a fortunate thing for Röntgen. For at the Bavarian universities it was impossible to be received into the faculty without having passed the Maturität at a Gymnasium. But at Strassburg University, after the Franco-Prussian war of 1870/71, there was greater liberty. Kundt was thus able to procure for his brilliant assistant the right of admission into the faculty. Röntgen was admitted in 1874. But in the following year he received an appointment at the Agricultural High School at Hohenheim, where however his experimental work was severely handicapped by the lack of precision apparatus. He was therefore very glad when through Kundt's influence he was recalled to Strassburg a year later as Extraordinarius for physics, and had to give lectures in mathematical physics. At Strassburg Röntgen spent a very happy time with his young wife, who had sympathy and understanding for his work; in later years both of them liked to remember their stay there.

Soon however, in 1879, the university of Giessen honoured Röntgen with the offer of a professorship. Here he formed firm friendships with von Hippel, the ophthalmologist, and others, which were ended only by death.

In the last years of his time at Giessen I became acquainted with Röntgen. It happened quite by chance at the "Weisses Kreuz" at Pontresina, where Röntgen spent his summer holidays almost every year. Enderlin, the proprietor of the hotel, who was also a schoolmaster and a mountain-guide, offered to show my wife and



me grazing chamois, and said he would like to bring a German gentleman as well. The German gentleman was Röntgen. We got into conversation, and discovered that Röntgen's wife was from our native town of Zürich, which was at once a bond between us. From that meeting dated our friendship with Röntgen and his wife.

As married man, with an education similar to Röntgen's, I was studying at that time in Berlin. As however it was not possible for me to graduate as a doctor at Berlin University, I asked Röntgen six months after our meeting whether it would be possible for me to graduate under him. He answered in the affirmative, but added that if I imagined that in view of my age he would make the examination easier for me, I was making a mistake; on the contrary, he demanded a high level from his students. That did not terrify me. I submitted to him, among other things, a piece of experimental work which, following an idea of my own, I had carried out at Helmholtz's institute, and asked him whether he would consider accepting it as a dissertation, although it gave a value for an elasticity coefficient for which there was no immediate explanation. For on account of my age I was anxious to take my degree in the coming summer term. After looking at the work, Röntgen agreed, but stipulated that I should do experimental work under him during the coming term on the chance of being able to get a more satisfactory dissertation finished within the time. First of all he told me to take up one of my earlier pieces of work, which he had liked, and to try to get greater exactness. I worked at this for several weeks, but without success. He then proposed that he should give me an entirely new subject, as he thought I might be able to solve the problem within the remaining eight weeks of the summer term. If I did not succeed, he would accept my work on the elasticity coefficient as dissertation. Thereupon I set to work, and the venture turned out well: within the time I finished the dissertation "On the influence of pressure on the refraction-exponent of water". Röntgen later published papers on this subject in conjunction with me.

After taking my doctor's degree, I went home for the holidays. Shortly afterwards Röntgen offered me the post of assistant, which had just become vacant and which I gladly accepted. He then initiated me into the work of his practical course, which I now had to conduct independently; in this way I came to know and appreciate his conscientious, thorough method of working and his carefulness in taking into account all sources of error. During this period he made his brilliant experiments on the magnetic effect of a rotating, electrically charged insulator in an electric field, which on account of their great importance for the general theory of electricity and especially for the theory of electrons were submitted by Helmholtz to the Berlin Academy and appeared in their publications. H. A. Lorentz gave the name of "Röntgen current" to this effect which Röntgen had discovered, and based his theoretical work upon it. Theoretical physicists like Lorentz, Sommerfeld and others, attach almost as much importance to this discovery of Röntgen's as to his discovery of the Röntgen rays. When Röntgen had set himself a problem, he always worked at it quietly and secretly, without allowing anybody a glimpse into his

method of working and thinking. Thus I knew nothing of the experiments mentioned, with which he was still occupied. But as this Röntgen effect was so weak that to be certain of the movement of the magnetometer required the greatest care, he fetched me one day and told me to look through the telescope. He then said to me that he would make an experiment of which I could see nothing; I was then to tell him whether I could notice anything at the cross threads of the telescope. I actually did see a very slight movement, to the left, let us say, of about two tenths of a division of the scale. When the experiment was repeated, I saw a movement of about the same extent to the right. I had to make several such readings without being allowed to know what Röntgen was doing, or whether the movement was to be expected towards the right or the left. Röntgen wanted to have his own readings controlled by an unprejudiced observer. It was only after its appearance in the publications of the Berlin Academy that I learned of this discovery of Röntgen's.

During my second term as assistant Röntgen asked me to compare a newly procured set of precision weights, ranging from 500 to 0.01 gr., as accurately as possible with a normal kilogram standardised at Berlin, and to standardise the separate weights. When I had almost finished this work, which took up a great deal of time, Röntgen was offered the chair at Würzburg in 1888, as successor to F. Kohlrausch. I followed him there willingly, although he told me at once that Heydweiller, who had been first assistant to Kohlrausch, wanted to retain his position under him, and that it would be impossible for him to make me his first assistant. He would however take steps to ensure that Heydweiller and I should be independent of each other, and I should have charge of the practical course in physics, which had first place in his affections. Thinking that the standardisation of the set of weights which I had just completed would not have any great value for his successor, he ordered a new and exactly similar set of weights for the Giessen Institute and took with him the set which I had standardised to the Würzburg Institute. For an exact set of weights, standardised with the utmost care, was essential for the work he had on hand, in which the weighing had to be as accurate as possible. At the beginning of the long vacation, therefore, Röntgen moved to Würzburg, and I also. Then I went to Switzerland for the holidays. Röntgen however had various things to see to in his new institute at Würzburg. In the institute he found a normal 100 gr. weight which had also been standardised at Berlin. He laid it on the scales with a 100 gr. weight of the set I had standardised and found a difference of about a hundred-thousandth part of these weights. That was too much for Röntgen, conscientious as he was in his weighing. With errors of such magnitude, he could not possibly attain the desired precision. He therefore sent a very stiff letter to my holiday address, and told me that he had found my measurements were not sufficiently reliable. I wrote back that I thought I could vouch for my measurements, and did not believe that I had made a mistake anywhere as the control measurements had always come out right. The discrepancy remained unexplained until we had both come back from the holidays. I then had to repeat some of my standardisations,



and obtained the same results. Röntgen went over some of them himself. He then sent the Würzburg normal weight to Berlin to be standardised again, and had the Giessen normal weight sent to him at Würzburg for a second comparison. But the inaccuracy was still there. The set of weights I had standardised, which weighed altogether 1 kg., differed from ten times the Würzburg normal 100 gr. weight by about 14 milligrammes. Then Röntgen noticed that the Würzburg normal 100 gr. weight, being made of a different composition of brass, had not exactly the same buoyancy in the air as the tenth part of the buoyancy of the Giessen normal kilogram. That covered about half the difference. About the other half Röntgen never told me anything, and I never asked because in my standardisations no error had been found outside the permissible limits. This little episode will serve to illustrate what importance Röntgen attached to the most extreme accuracy possible in measurement.

A lasting impression was made on Röntgen by an incident during a visit to the Physikalisch-Technische Reichsanstalt, which had a battery of standard cells for the purpose of standardisation. Like other electric batteries, it could be connected in series or parallel by means of plugs. In the course of his explanations, the official who was showing Röntgen round brought some metal object too near the terminals of this battery, and touched them with it. The battery was discharged in a single spark. That went to Röntgen's heart, for the costly battery was no longer above doubt for purposes of precise measurement.

At that time F. Braun, later one of the pioneers of wireless telegraphy, had published his sensational announcement that electric currents could be produced directly through deformation — deformation currents: nickel wires, if drawn through a draw-plate and then wound into cylindrical spirals, gave electric currents when deformed, without there being any other demonstrable source of the electricity, the current being of opposite direction according to whether the spiral wires were pressed together or pulled apart; and the same thing in the case of iron wires. I had read the paper and found it not free from objection, when Röntgen asked me whether I would not test it. Röntgen then went away for the Whitsuntide holidays, while I set to work with the greatest zeal to examine Braun's deformation currents, in the hope of being able to explain their nature during Röntgen's absence. I succeeded; and when Röntgen came back, he saw that I was right and Braun was wrong. For I was able to show that when the draw-plate was employed in the usual way, the deformation currents noticed by Braun were produced; but when the draw-plate was reversed, deformation currents were produced opposite in direction to those of Braun. That clinched the matter. Röntgen now told me to write a reply immediately. But when I submitted my draft, I could not admire sufficiently his care and conscientiousness. For hours he turned the subject over, talked with me about my experiments and the explanation given. In the course of these conversations he said: "In your remarks about Braun's deformation currents there must not be a single word, not the hint of an idea, which he could seize on and use to prove that there was even a grain of truth on his side or even a suspicion of an

error on yours". The proofs were then ready, and my draft was completely altered. I was in a hurry to print my attack on Braun, for I felt absolutely certain about the results of my experiments. But Röntgen laid the paper aside. A paper must be gone over again and again, he explained. After some days he sent for me once more. Again he weighed each word as if it was gold, examined everything with the extremest care, and then — again he laid the paper aside. After some days he filed at it and examined it for a third time to see if there was any word for which a better one might be found. At last he was satisfied and let me hand it in to be printed. I freely admit that as far as I can remember there was little left of my original draft as regards style. But the ideas and the arrangement of the experiments as well as their execution were mine. Röntgen had sought the cause of the deformation currents elsewhere. He therefore claimed no share in this refutation of Braun's theories. I was eager to see whether Braun would answer. Actually he added a note to his third article on the deformation currents in which he tried to prove that my views were wrong and rejected my explanation of the cause of his deformation currents. It was then easy for me to prove conclusively that in his answer Braun had started from false mechanical conceptions, and the question of Braun's deformation currents was thus settled once and for all.

As I had not passed the Maturität examination at a Gymnasium, it was also impossible for me to be received into the faculty at Würzburg. I therefore went to Basle in 1890 to be received into the faculty there, and eighteen months later succeeded in being received into the faculty at Freiburg in Baden. Thus when Röntgen discovered his rays, I was not with him. Frau Röntgen however told my wife and me that her husband sent special copies of his work on the subject and some photographs, notably of his own hand with two rings taken with the rays, to some medical friends in Vienna, and having done so, said to her: "Now the fun can start." He was thus fully aware of the extraordinary importance of his discovery.

There is no need for me to describe the tremendous sensation which Röntgen's discovery aroused in the whole world, as that is general knowledge. The Emperor Wilhelm II also took a lively interest in it and kept himself informed through Slaby. Röntgen was presently asked in confidence wheter he would consider coming to Berlin to show the Kaiser some of his experiments. Röntgen — perhaps because of his relations with Switzerland — was inclined to republicanism and did not care a straw for the noble gentlemen who at that time set the tone in Würzburg. When he was made a Bavarian privy councillor by the Prince Regent, with the title of "Exzellenz" and the right to sign himself "von Röntgen", he scorned to write the formal letter which the acceptance of that right involved. He did not however reject the proposal from Berlin and was therefore summoned thither to give the lecture. He told me that he was not at all easy in his mind, as he had only one serviceable X-ray tube with which he had to make all his experiments, and was worried by the thought that it might get broken on the way to Berlin. But Röntgen was so careful and cautious in scientific matters that the tube did not suffer and



his experiments were completely successful. Röntgen was convinced beforehand that the Kaiser hardly understood anything of the sphere of physics with which his discovery was concerned. He told me however that he was astonished at the intelligent and pertinent questions which the Kaiser put. After the lecture the Kaiser conferred upon him one of the higher orders of the crown, though Röntgen had no interest in decorations. He was also sounded as to whether he would accept a call to Berlin, but he answered in the negative as the quietness of Würzburg suited him much better than the bustle of Berlin.

At the time of Röntgen's discovery I was professor at Freiburg in Baden. Being a former pupil and assistant of Röntgen's, I was asked by the president of the Naturforschende Gesellschaft of Freiburg to give a lecture before the society on Röntgen's discovery, and if possible to carry out some of his experiments. Thereupon I tried to improve the tubes which were then available for use, and constructed a Röntgen tube which could be regulated by means of wood charcoal. Röntgen asked me to send him one of these tubes, and then wrote to me that it had given him the sharpest pictures he had hitherto got. With the use of such a tube I was the first to succeed in taking a Röntgen photograph, in six sections, of the whole of the human body. On the occasion of the next annual meeting of the society of former pupils of the Polytechnic at Zürich (1896), of which Röntgen and I were members and had agreed to attend the meeting, I surprised Röntgen by showing these Röntgen photographs. They, as well as the regulatable Röntgen tube with which I took them, are now on permanent exhibition in the German Museum in Munich.

After that I did not occupy myself any more for the time being with Röntgen's discovery. I followed however the publications concerning it, and saw how a host of physicists threw themselves upon it in order to seize on some new phenomenon connected wih it and if possible to explain the nature of the rays. During more than ten years hundreds of publications appeared, but none of them brought to light anything essentially new. Röntgen had already examined everything himself and described it clearly in his three papers on the subject. But the authors' excitement and their haste lest they should be too late with their supposed discoveries, prevented a thorough and careful reading of Röntgen's communications. Otherwise they would have recognized that Röntgen really had discovered and described everything already. The communication of Dorn and Brandes on the direct effect of the Röntgen rays on the eye, in a sense the visibility of the Röntgen rays, was the only one that contained anything not already published by Röntgen. But Röntgen later told me that he had already perceived this effect himself by observing the point of issue of his rays through a narrow slit. Thereby the image of the slit had not been in straight lines but had appeared as a curved line, because a section of a plane through the retina does not appear to our consciousness as a straight line. For geometrically it is easy to understand that a straight line, after refraction through the lense of the eye, must be reproduced upon the almost spherically shaped retina in another form than that plane section through the retina which is produced through

the unrefracted Röntgen rays. But Röntgen, in his excessive caution, thought that this observation might be the result of a peculiarity of his eyes, because he was colour-blind — which personal defect he, especially as he was a physicist, did not want to publish to the whole world. He therefore did not announce this observation.

Many physicists tried to find more simple and convenient ways of producing Röntgen rays, but without success. I made attempts with the explosion of powder, but gave up the experiments with more powerful explosives as on one occasion in Berlin such an explosion had unfortunate consequences, including the temporary loss of my hearing. I believe however that violent explosions are the most likely new way of producing Röntgen rays.

One scientist however was fortunate and found a really new source of Röntgen rays. This was Becquerel in Paris. He discovered radio-activity, which at last placed the theory of the atom, so long a subject of dispute, on a sure basis and in its subsequent results threw extraordinary light upon the interior of the atom.

An incident during the holidays will serve to show how quickly the whole world became enthusiastically interested in Röntgen. He arranged with me that we and our wives should meet at Rigi-Scheidegg and afterwards at Pontresina for our summer holidays. When we arrived at Rigi-Scheidegg station, Röntgen sent my wife and me on ahead as he had to see about his heavy luggage. We made our way up to the hotel through a crowd of visitors who, many of them armed with cameras, lined the road to see the great Röntgen. As we went past, my wife heard murmurs of "But he doesn't look like that!" He certainly didn't. Röntgen's photograph had already been published in all the more important newspapers. Shortly afterwards came Röntgen and his wife — a fine, imposing couple. At Pontresina also the visitors were always trying to get snaps of Röntgen, which he found very annoying. During the years when the curious were most importunate, he used to choose the least known walks at Pontresina where there would be most chance of not meeting anybody. He could sometimes be quite rude to people who molested him, like the Swiss writer Gottfried Keller, whom he had met in Zürich.

Röntgen often fought energetically against ideas that seemed to him to be wrong. The exactness of his work gained him more than one secret enemy among his fellow-scientists. Many could not forgive him when he repeated their experiments, made and published more accurate measurements, and was sometimes thus actually lead to new discoveries.

Probably nobody has been so envied the result of his researches as Röntgen. Being in the habit of repeating interesting experiments by other scientists, or of getting his pupils to repeat them, in order thus to gain a clearer idea, he had repeated the beautiful experiments with cathode rays published by Lenard. Following Lenard's directions, he had encased the Lenard tube in black cardboard impervious to light, and then observed the lighting up of the fluorescent screen when the Lenard tube was set in activity. Knowing Röntgen's method of working, I imagine that he then took other vacuum tubes which he had in his institute, encased them in black



cardboard and then examined them with the fluorescent screen. One of the most effective of the cathode ray tubes was that constructed by Hittorf, the discoverer of the cathode rays, for the proof of the heat effects of the cathode rays: in this tube the cathode rays issuing from a hollow spherical cathode are concentrated in a focal point, and at this point Hittorf placed a platinum sheet in the path of the rays. When this "Hittorf tube", as Röntgen always called it, is used, the platinum sheet soon becomes red-hot at the focal point and with a powerful inductor may even be melted through, as unfortunately happened once to me when I was assistant for the first time at Würzburg. Röntgen told me himself that he discovered his X-rays with the Hittorf tube and not with the Lenard (L. Zehnder: Ueber Kathodenstrahlen und Röntgenstrahlen, Berichte der Naturfor. Ges. zu Freiburg i. B., Bd. 11 Heft 1, S. 24, 1898). This statement of his cannot be called in question, for Röntgen was truthful through and through. He hated nothing more than that anyone should wish to make even the very slightest "correction" of a scientific result for the sake of a previously formed opinion. Because Röntgen had been making experiments with a Lenard tube before his discovery, some scientists out of motives of envy said that Lenard had observed Röntgen rays before him. What is true in this is that Lenard in his experiments had had to do with Röntgen rays. But the point is that he did not realise they were present in addition to the cathode rays, because he concentrated all his attention on the latter. In Lenard's brilliant work on cathode rays there is not the slightest hint concerning any other rays which might be those of Röntgen. And Röntgen rays are not produced at the cathode, but at the anti-cathode, or if there is no anti-cathode, as in the Lenard tube, then on the sides of the tube. Therefore in the Lenard tube, which lets the cathode rays issue from the cathode only as a sheaf of parallel and partly divergent rays, the Röntgen rays are extremely weak. In conformity with the above described construction of the Hittorf tube, which has got an anti-cathode, Hittorf was already dealing with Röntgen rays in much greater intensity. Similarly, numbers of physicists — all who had ever made experiments with Hittorf's (heat) cathode-ray-tube, which at that time was very widely known — all had been dealing with Röntgen rays, including myself. But none of us saw the phenomena which they caused. Röntgen, one of the most outstanding physical experimenters of the world, he alone saw them, discovered them, and because he carried out all the arrangements of his experiments with the greatest care and was in addition extraordinarily well read, he knew at once at the first lighting up of the fluorescent screen that this could not be caused by the cathode rays themselves, and that he was therefore confronted with a perfectly new phenomenon. An annoying thing, almost a shameful thing, for all us physicists — and there were countless numbers of us — who before Röntgen's discovery had had Röntgen's rays beneath our hands, and had not seen them. Or was it that Röntgen, just because he was colour-blind, had an especially sensitive eye for differences in brightness? That he distinguished remarkably fine details in photographs — that I am sure of.

Other detractors started the report that it was really Röntgen's laboratory attendant Marstaller who had first observed the lighting up of the fluorescent screen and consequently discovered the X-rays. But Röntgen's method of scientific working was certainly not to let a laboratory attendant help him, or even an assistant. All the time that I was with him, he did everything himself in his experiments. Dr. Stern, who was Röntgen's assistant at the time, was as much surprised by the discovery as the rest of Röntgen's colleagues. Further, from my knowledge of Marstaller during my three years as assistant at Würzburg, he did not observe particularly closely nor work particularly well. Even more absurd is the statement that Röntgen's preparator at Munich, named Weber, was the discoverer of the Röntgen rays. How blind must envy be when it does not even consider that Röntgen was not at Munich at all when he made his discovery, but at Würzburg! People also told me more than once that they had heard that I was the real discoverer of the Röntgen rays. But I was not at Würzburg at all at the time, but at Freiburg in Baden. It was only from 1898 to 1900 that I was again Röntgen's first assistant at Würzburg.

As I was often asked in Freiburg to give public lectures on Röntgen's experiments. I wrote to him and asked him whether he would let me have some copies of his first photographs with Röntgen rays for use at these lectures. He willingly consented, and even personally wrote in pencil explanations of the experiments on the copies. As at that time there was a hunt for letters, postcards etc. in Röntgen's hand-writing (I have never let any of Röntgen's numerous letters to me out of my hand), I wrote on these copies which Röntgen had given me: "Property of Prof. Dr. L. Zehnder, Freiburg i. B.", in order that these valuable copies should not be stolen from me when I handed them round at my lectures. It is well known how shameless even educated people can be in this respect. After Röntgen's death a Röntgen room was organised at the Würzburg Institute under the direction of my younger friend E. Wagner, and I thought it was only right that I should present my copies to the Röntgen room. They are now preserved there. If there is still any physicist left in the world who is not perfectly convinced by my above remarks and by other facts that Röntgen really did discover the Röntgen rays, I should like to invite that physicist to visit the Röntgen room. Let him then look at the photograph in which Röntgen let his rays act on the photographic plate through the closed door, and partly through the door-post as well. Everybody can see at the first glance that the usual, familiar moulding of the door-post is reproduced. Quite natural! But Röntgen did not find it so natural. For he saw that the shading of the door-post in the picture did not exactly correspond to the absorption through the mass of wood in the post. I imagine that probably thousands of other physicists would have regarded this shading as quite natural and would not have given it another thought, and I do not venture to say that I should have been an exception. But Röntgen drew my special attention to this copy. "This shading attracted my notice", he said to me, "and made me realise that the decisive factor was not the absorption through the unequal thicknesses of the wood of the door-post, but absorption by the surface of the post. I inquired what the door had been painted with, and learned that it was white lead. As lead is very impervious to these rays, a layer of white lead running in the direction of the rays absorbs them considerably more than a layer at right angles to the rays." In the copy of his shot-gun, which he photographed with his hard rays in the first months after his discovery, he showed me, not the lead pellets in the gun which anybody could see at once, but layers of cardboard in the cartridge, a number struck in the barrel, and other hardly noticeable irregularities. And Röntgen, who, while fully aware of the enormous importance of his discovery, still observed with such keenness and care and considered every detail, Röntgen was not the discoverer of the rays named after him, but a not particularly bright laboratory attendant? No physicist can believe that.



In 1899 Röntgen was offered the chair in Munich. He wanted to refuse because the nice quiet laboratory at Würzburg suited him so well. I pointed out to him that, world famous as he now was, he should not think too lightly of the wider scope of activity that he would have in Munich. And as artistic town he valued Munich highly, and had always enjoyed being there. Did I do wrong? After having taken me with him for a close inspection of the Munich Institute, he decided to accept the offer if I would come with him again. He went to Munich in 1900, and I stayed there for four years as his assistant until I moved to Berlin.

Much of Röntgen's work both before and after his great discovery is concerned with properties of atoms and molecules and what takes place in them. The fact that water reaches its maximum density not at freezing point but $4^{0}$ above, led Röntgen to the hypothesis that in the neighbourhood of freezing point there are not only water molecules in the water, but ice molecules are already formed in which the two hydrogen atoms are differently arranged with relation to the oxygen atom than in the water molecules, on account of which the water becomes again less dense below $4^{0}$ until it changes completely into the lighter ice. Although this view, in the light of our present knowledge, is probably correct, Röntgen has more than once expressed his regret to me that he had published this hypothesis. For he was a great enemy of unproved hypotheses.

It was a great joy to Röntgen that it was in his immediate neighbourhood, in the laboratory of Sommerfeld, that the wonderful discovery of von Laue and his collaborators Friedrich and Knipping was made, whereby light was at last thrown upon the nature of the Röntgen rays. Shortly afterwards I happened to be on a visit to Röntgen from Berlin. He took me to the Institute and showed me a couple of the first of von Laue's photographs with crystals. I said to him immediately that they were reflections of the molecules of the crystals, which must act like grate-points. This seemed to me so natural and von Laue's photographs reminded me so strongly of the corresponding optical grate-spectra (vide Schwerd) that I did not think I had said anything new. But a couple of years ago I happened to meet my friend E. Wagner during the summer holidays at Wengen, and we went for walks together; he told me that Röntgen had said to him that I was the first who had interpreted von Laue's

photographs to him as reflection phenomena of the crystallically arranged molecules — a remark which Röntgen never made to me, but which I was very glad to hear later, though I do not wish to claim the smallest share in von Laue's discovery.

It was a great grief to Röntgen that his rays had an injurious effect and could even cause the death of doctors working to relieve suffering. When Röntgen diagnosis became extremely important during the war, I constructed in the Zürich Hospital (where I was temporarily in charge of the physical section of the Röntgen department) the first metal Röntgen tube, which allowed the rays to issue practically only through a small window, so that the doctors employing the rays could protect themselves more easily against their injurious effects. This metal Röntgen tube is also now preserved in the German Museum. At Christmas 1914 I telegraphed to Zehme, the editor of the Elektrotechnische Zeitschrift in Berlin, that I had discovered a metal tube (ETZ, S. 19, 1915). In the first February number of the ETZ appeared my more detailed account of this new tube, and I added that I handed it over for general use, without taking out a patent. For my only wish was to protect the doctors against injury. I also hoped to give pleasure to Röntgen through the construction of the tube. But when some weeks afterwards I was returning to Berlin by way of Munich and stopped for a few days with Röntgen, a representative of a Röntgen tube factory came to Röntgen's house and wanted to learn from me more details about this new tube, of which he had heard. He had been told at Zürich that I had just left to visit Röntgen. But Röntgen was annoyed about the incident and reproached me seriously for having made the publication, perhaps because I had written in it that with correspondingly enlarged metal Röntgen tubes it would be possible to get effects a thousand times stronger than could then be obtained. All the same, in the course of recent years the " Metallix " tube of the Dutch firm Philips has established itself, and I still believe that its effects could be made a thousand times stronger.

Röntgen's lectures were distinguished by extremely clear experiments, which never failed. He had a strict sense of justice, and therefore the more friendly his relations were to his pupils, the less eager he was to recommend them for a post. He also never made any material profit out of his discovery, though he was often given the opportunity. But he was the first physicist to be awarded the Nobel prize. He also received many other important scientific prizes and numerous other distinctions. In 1919 he retired from his professorship.

Röntgen's most understanding and truest friend was — his wife. She was in every respect an excellent wife, who took the liveliest interest in all his scientific endeavours — a woman of noble character. Like Röntgen, she loved nature and art. They used to spend their holidays together at Pontresina and in Italy, generally with friends like von Hippel, Krönlein, Boveri and others. She accompanied her husband when he went hunting, and waited patiently for hours while he endeavoured to do from the stand what he was a master at with a gun in his hand, but what in science he felt as a disgrace — to shoot bucks*)! Frau Röntgen was ill for many years and had

*) A translation of a German proverbial expression for making a great mistake.



to have morphium injections, which Röntgen himself gave her in the most conscientious manner. But he used to change the dose daily according to her condition, without telling her anything. In this way he tried again and again to break her of the habit. When her condition was at its best and she asked for morphium, she got a dose without any morphium. There is no doubt that in this way he added years to her life. After she had preceded him in death, for years he decorated her bed with flowers, as his faithful Käthchen told us, who was with the Röntgens for thirty years, was often taken with them on their journeys, and served them devotedly till her death.

Six months before Röntgen's death, I visited him once more for a day with my wife. He wanted to keep us longer, but the reduced circumstances in which he, who had once enjoyed the income of a very wealthy man, now found himself after the loss of his property, forbade me to stay longer in spite of his urging. I did not want to involve him in unnecessary expense, and also had no idea that he would soon be no more. I shall never forget the last and so strikingly powerful pressure of his hand.

Röntgen died on February 10, 1923 in Munich after a short illness, his hand in those of his faithful nurse, the servant Käthchen. In accordance with his wish, the urn containing his ashes was placed next that of his wife in the grave of his parents in the old cemetery of Giessen.

Taking him all in all, Röntgen was not only one of the greatest physicists in the world, one of the most important benefactors of humanity; he was also a fine man.

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*Translated by J. A. West.*

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