Personal Reminiscences

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I was born on 27 June 1888 and brought up in Florence, Italy, where I attended high school and also took one year of university courses.

From there I went to the University of Munich, and after the preliminary mathematics courses, entered Röntgen's laboratory. Röntgen belonged to the classical school of experimental physicists. It was the time of plausible theories or models and Röntgen was fond of such interpretations. He was an experimentalist, not in the sense of a technician or gadgeteer, but one who stressed accuracy and insisted that everything observed should be accounted for. It was not fortuitous that he became the discoverer of X-rays. Many others, in pursuit of their research, would have disregarded the patches which appeared on the photographic plates, or would have attributed them to some chemical or other indefinite action. For him, nothing could pass by without his establishing a definite reason, and an experiment was deviated from its course or halted until this was found. In his later years he was engaged in the study of physical phenomena of crystals, which he considered to be better defined objects than other materials. He was retiring, self-effacing, almost shy, creating the impression of being unapproachable. Actually he was kind, and keen on encouraging the work of his students. Röntgen declined to enrich himself from his discovery, and sought to stop others from doing so; in fact, no major patents were taken on the field of X-rays until the advent of the Coolidge tube. When in the early days of X-rays a tube maker submitted a new model to Röntgen's endorsement he would enquire on its price and say that it was too expensive, and that it should be possible to make it for much less. X-ray tubes cost then 7 to 8 Marks.

The trend of the laboratory contrasted with that of the great experimental schools of Rubens, Lenard and Rutherford, where the

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whole institute is set to work on one field and the beginner is immediately provided with all the accumulated knowledge and given a place on the common front, which puts him in the position of making his own contributions. In Röntgen's institute each worker pursued a distinct line, he had to study the literature from scratch, and was left to his own devices. To many this was discouraging. I rather enjoyed it to be left to find my own way. As a preliminary to a thesis I was to examine the residual charges of dielectrics after they had been exposed in succession to electric fields in opposite directions. Röntgen appeared at intervals of four to six weeks, when he would spend a whole afternoon looking for any possible sources of error. He was satisfied when I could show that I had foreseen them, and was pleased when I had definite results. Actually my results disproved a theory he was developing. Not only did he get them published, but he offered his help to improve the wording of my article. When I ended with the customary acknowledgement of thanks, which in this case was well justified, he insisted that it should be cut out. After this first step, I enjoyed full freedom of using the facilities of the laboratory without having to submit to detailed scrutiny.

The chair of theoretical physics was then held by Sommerfeld, who was a brilliant lecturer as well as an inspiring leader of research. He ran a colloquium in which theoretical and experimental developments were equally reported. He took great interest in the work of his students and proposed problems covering a wide range of topics. Among his students at the time were men like Epstein, Ewald, Landé.

When I was in the midst of my studies, a third physicist appeared, Max von Laue, who established himself as a Privatdozent. He came from Berlin as a former pupil of Planck. Laue settled in the same boarding house in which I was staying. With his fine features, the open expression of his blue eyes, he won the affection of those he met. He was unconcerned of external social conventions and was somewhat absent-minded in the face of gossipy conversation, but always ready to take up the discussion of any topic of physics. He took a strong stand for whatever he considered just, and held high the integrity of science and of the scientist. He was very fond of nature; mountains and lakes held a great attraction for him and so also works of art. He was particularly fond of classical music, loved especially Beethoven. Soon after coming to Munich he built a boathouse with a living-room standing on posts in the way of a prehistoric dwelling on Lake Starnberg, where he owned a sailboat. Sundays I was often invited there and so I first became his pupil not in physics, but in sailing. While in Munich he married, and his wife became a most gracious hostess to the friends he used to invite.

At one time, when a group of students, including myself, wanted to take his course in thermodynamics, we sat waiting in the lecture room at the appointed time, but Laue did not appear. This repeated itself three or four times, when we learned from the Dean that Laue had come to give his lecture, but at a different hour, and finding the room empty had gone home. After the Dean had synchronized the hour we were given a course, not brilliant in delivery, but most inspiring in its logical development, and in the presentation of new aspects like the entropy of coherent radiation. Such an error in timing could have happened to him at any time, but in this case there was a special reason; Laue was pursuing with Friedrich and Knipping the experiments on the diffraction of X-rays which have been fully described in Part II of this book.

When the first diffraction patterns were observed Laue sent out copies to his friends, asking: 'what is this?' Soon after, he presented a complete mathematical treatment of the phenomena.

In 1914, when I obtained my Ph. D. degree, Laue made me his assistant. I was then to join him in Zurich, where he held an appointment for theoretical physics at the University. Before I arrived, however, Laue had accepted a chair of theoretical physics at the newly-founded University of Frankfurt and I became his assistant there. The 1914 World War had broken out and my initial task was to assemble equipment for X-ray diffraction. No instruments could be bought and I had to scan secondhand metal dealers; one shop in the house 'Zum goldenen Kopf', which had been the home of my ancestors and now had become a tool and metal store, played a great part in providing supplies. Other useful ties were established. Friedrich Dessauer, the director of Veiva, then one of the leading manufaturers of X-ray equipment, came from a family closely befriended with mine. For a mechanic I found an excellent craftsman beyond military age, who after his daytime work came for nightwork to the University. My hours at the University extended thus into the night, when one could hear from the street the marching songs of the young soldiers who were led for embarcation in troop trains for the front. After some time the mechanic collapsed because of overwork and had to stop. It was not the atmosphere for indulging in X-ray diffraction, particularly for an alien, so in March 1915 I left for Zurich to join my ageing father. The impact Switzerland had on me was bewildering; in Germany I thought I had kept aloof of war bias and had distinguished between true news and war propaganda. Suddenly confronted with news from all parts of the world, I became aware of how much I had been the victim of the psychological experience that repetition makes truth.

I was at once taken on by Auguste Piccard, then in charge of the physics department of the Polytechnical Institute, while Weiss, the head of the department was engaged in war-work in France. Piccard had then already conceived the idea of the high altitude balloon and of the bathysphere with the ingenious oil compensation to counteract the effect of pressure, and I had to promise as unwritten part of my contract that I would be willing to take part in an ascension of the first. This did not come off. Piccard had come to physics after having studied mechanical engineering, and this reflected on his choice of problems and on the methods of attack. He was responsible for a great part of the instrumentation in Weiss's laboratory and was very imaginative in devising original and unconventional methods. The Weiss laboratory was concerned with the measurement of magnetic susceptibilities and Piccard reduced the uncertainty in the measurement of magnetic field strengths with a paramagnetic liquid, by introducing small particles held in suspension and observed with a microscope as indicator for any small displacement. I adopted this method with the object of measuring the X-ray yield as a function of wave-length and tube potential by observing the increase of pressure when the X-rays are absorbed in a closed vessel. The apparatus had the required sensitivity. The work was interrupted because I was called upon to replace Professor Schweitzer, who held the second chair in physics and was struck by illness.

Another problem, somewhat related to the first, was the design of an X-ray monochromator giving the highest possible yield. It implied developing the geometry for the diffraction of a diverging beam of X-rays of given wave-length and of large aperture so that, emerging from a source point, it would be diffracted by a given spacing to another point. This means distinguishing between the geometrical locus of the surface from which such diffraction takes place, which is a toroid, and the orientation of the diffracting lattice planes, which in general are inclined to it like the tiles of a roof. Later we referred to this geometry as para-focusing. The general aspects were presented in *Arch. Sc. Phys. et Nat.* (4) *44*, 66 (1917); the application to a powder goniometer in *Arch. Sc. Phys. et Nat.* (5) *1*, 550 (1919).

Using gypsum we constructed high-intensity monochromators. When high resolving power rather than high intensity is required the

Cauchois ground and bent crystal is the elegant solution of the parafocusing condition for one plane.

Edgar Meyer held the chair of physics at the University in Zurich. He ran a colloquium which we used to attend. Einstein had come to enjoy the freedom of Switzerland for part of the war. He gave a fascinating course in special theory of relativity and attended the colloquium, where, to the general surprise, he took a lively interest in the discussion of experimental papers. I became particularly good friends with the mathematician George Polya. He would open any discussion with a sentence reminding of scholastic dialectics; 'Also die Frage ist die—'followed by the statement of the premises made. He is now in the U.S. actively engaged in raising the level of high school mathematics.

From Zurich, after the war, I visited von Laue who had not taken root in Frankfurt and had returned to Berlin. He opened the door in person, a revolver in one hand and a dagger in the other. It was the time when the new German Republic was weak and terror bands of opposing parties had a free hand. He was relieved to find that the ringer of the doorbell was a harmless individual.

With the death of Professor Schweitzer and the termination of my duties of replacing him I went to England in 1920, where young W. L. Bragg was establishing an X-ray centre at Manchester University. I became a member of the staff, and found myself suddenly transplanted in quite new surroundings. Bragg had come to Manchester shortly before, and the memories of Rutherford's activities were still very much in evidence. They were passed on to the newcomers by W. Kay, the steward of the department, who used to assist Rutherford during the summer months in exploratory experiments to set out the research topics for the work to be proposed to students in the coming term. Rutherford was more interested in qualitative results than in exact quantitative measurements. He had no time to wait for the building of elaborate instruments and so the department had no proper mechanics shop, and what tools there were had been misused for purposes other than those for which they were intended, but there was a tradition how to set up experiments quickly and effectively. Outside firms, a mechanics shop, a glassblower and a tinsmith were under contract to give precedence to university work when required and these were called in whenever the resources of the department were insufficient. Of these, the tinsmith was most in demand. In England the trend of a department is determined by its head, so the delicate optical instruments assembled by Sir Arthur

Schuster, Rutherford's predecessor, were short of reading telescopes and eye pieces, which Rutherford had used for scintillation counting. With Bragg, the aspect of the department changed again and a further change occurred when he was succeeded by P. M. S. Blackett, when the activities were concentrated in one single large room which happened to have a light roof so as not to interfere unduly with cosmic rays.

When I came to Manchester, Bragg, James and Bosanquet, in consultation with Darwin, were engaged in the problem of primary extinction, a problem which had also been covered by Ewald's dynamic theory of diffraction. Soon, however, the interest shifted to the structure of inorganic materials, in particular metals and alloys. Bragg had an extraordinary gift of interpreting X-ray patterns in terms of structures by a direct visual approach and trained his pupils in these methods. The X-ray patterns were obtained with cameras using the rotating powder rod and Bragg did not waste time with elaborating this technique as long as it was yielding patterns good enough for interpretation.

Sir William Bragg occasionally came to Manchester to give a lecture, in which the most complex physical facts were presented in the simplest of terms. Like his father, W. L. Bragg possessed to an extraordinary degree this ability of finding forceful analogies. He was very much in demand for giving outside lectures.

While measurements from powders present several advantages, the method of the powder rod, exclusively used at the time, allowed to utilize intensity data for the interpretation of diffraction patterns to a limited extent only, since the relative intensities of lines at different angles depend on the shape, density distribution, and absorption coefficient of the individual powder rod, which factors are difficult to allow for. We became thus interested in the problem of developing a powder method for obtaining intensity data. We could establish intensity expressions for a powder layer conforming to the para-focusing condition, such that relative line intensities become independent of absorption in the powder. A goniometer was constructed for photographic recording which satisfies the requirements of these expressions; it makes use of a slit in a screen rotating in front of the film, which for counting becomes the counter slit. This instrument gave very sharp lines while using much wider beams than permitted with the powder rod technique. It could be adapted to the cases of having the X-ray entrance and counter slits equidistant or not from the powder.

For photographic recording—then generally used—there remained the handicap that photographic opacity and X-ray exposure are not proportional, the evaluation of each line requiring thus a conversion by an elaborate process of point to point scanning. We found that for small densities a linear relation exists between X-ray exposure and the number of silver grains produced. Instead of counting the grains we evaluated them by measuring the light scattered from a scanning beam. The goniometer and the method of scatter densitometry gave us the means for making quantitative intensity measurements from powders. Several investigations have been made of extinction, differential absorption and atomic scattering power using these methods. A. Baxter investigated the anomalous scattering near the L-edges, which for the K-edge had been investigated theoretically by Hönl.

My official duties in Manchester consisted in giving a course in thermodynamics and kinetic theory and running an advanced laboratory for honours students. Such a laboratory with set experiments becomes very dull to students and teacher alike, so it was gradually converted into short research, or rather pseudo-research, projects; more often the latter, because in most cases similar results had been obtained previously by other methods. The students presented their work in a colloquium which Professor Hartree, the mathematician, attended. He gave valuable comments on mathematical treatment and theory. This activity maintained contact with a broad front of physics.

The students impressed their interviewers for industrial jobs, but I wonder how much this was due to their better training and how much to their greater ability of expression.

When Bragg was called to succeed Rutherford in Cambridge, P. M. S. Blackett became head of the department. He had a winning personality and great enthusiasm for his work, which he transmitted to staff members and students. Apart from a passion for cosmic rays, he had widespread outside interests extending to art and politics.

In this period—spring 1938—also falls a visit to Vienna, where I was to establish contact with Mark and Schrödinger with a view of helping them to leave Nazi territory. Hitler had just invaded Austria and everybody was wearing distinctive buttons. Having none I found myself at a handicap, the more so since my hotel happened to be the headquarters of the German General Army Staff. Grubbing in my pockets I discovered an enamel button B.I.F.—British Industries Fair —which I had just visited on my way. This produced miracles: with the button pinned to my coat the soldiers presented arms, officers saluted and many heels clicked whenever I passed. I found that

Mark was taken care of; I was able to see relatives of Schrödinger and to make some arrangements.

Soon the war was to engulf the whole of Europe. I had just been given a sabbatical leave and was on my way to the U.S., where I intended to make arrangements for the manuscripts of my father, the late philosopher Franz Brentano. With the outbreak of war I returned to Manchester to find actually that there was no special need for me. So after one year I went to the U.S. for the summer months, taking the manuscripts of my father with me. I was offered a position at Northwestern University, where a large expansion of the physics department was planned and an X-ray physicist needed. This opened up the tempting opportunity of planning my own laboratory. I accepted to stay and found myself once more in new surroundings. In England the students are polite on the borderline of being submissive, the sharpest criticism is expressed in the words: 'how interesting', and the superiority of the teacher is so much respected that a student standing next to me watched my sleeve catching fire in reaching over a burner without so much as making a sign. To my reproach he answered: 'I did not want to be rude, Sir.' In the U.S. the students have much less restraint, instead of saying: 'how interesting', they would say: 'I don't see that', and would back up their doubts by a lengthly exposition in my office, giving me the opportunity to clarify the point and to satisfy them. This made for refreshing relations. At the beginning of my stay it also happened that a student, to whom I had given a low grade, commented: 'I am never given a low grade, you must change it.' On asking a colleague about this strange behavior and giving the name of the student, he said: 'this is the name of a well-known Chicago gangster, you have to be very careful.' I did not change the grade and I am still here to report it. Probably the father gangster condoned the newcomer for his lack of experience. It did not happen again, so I had no opportunity of testing whether I had learned how to behave. The vitality and keenness of the students, and the availability of technical facilities, were encouraging. The situation would arise when I could say to the head of the department that I had all I wanted and did not require anything more. When did this ever happen in Europe! For the first time I did not have to make my own X-ray tubes and in this connection I was pleased to be able, on a very minor scale, to make a contribution in the sense of Röntgen: a manufacturer of diffraction tubes thought he had to avoid a more efficient design in order not to infringe on patents of another tube maker. I could point out that I had published the description of this design much earlier and that there was no valid patent on it. A students' laboratory similar to the one in Manchester was established, out of which some research items resulted. I may dwell on one because it is of some significance in X-ray technique. Many attempts had been made to introduce scintillation counting to X-rays, which has obvious advantages, particularly for short wave-lengths. They failed because the X-ray count was drowned in the general background of thermal and other random emission from the crystal and the photomultiplier commonly used. The difficulty was overcome by considering that each X-ray photon produces the emission of several photoelectrons (about 13 with sodium iodide crystals and Mo K radiation according to West, Meyerhof and Hofstadter), while thermal electrons are emitted individually at random. By chopping the electron emission into sufficiently short intervals it can be arranged, that for each such interval the probable number emitted by random processes is small compared with the number emitted by an X-ray photon. Using a discriminator it is easy to count only the pulses due to X-rays. This was done by Ladany and reported at the 1953 Rochester Meeting of the American Physical Society.

We also tried to improve photographic X-ray techniques. By projecting the line pattern on a film, which is being rocked through a small angular range (a fraction of the line width), narrow high peaks could be broadened out and the quantitative evaluations improved. On the other hand, when correct line contours in terms of intensities were required, a microdensitometric method was developed for automatically converting opacities to X-ray exposures. These methods were used in investigations on small-angle X-ray scattering and in particular, jointly with Spencer, in the study of any change of the AgBr lattice, when the latent image is formed. This required all the refinements of powder techniques; it led to distinguishing between two theories. The formation of the latent photographic image, of the electrical conductivity of pressed powders, and of electrical properties of highly disturbed lattices, were investigated. This is reported in Ph. D. theses by the students who undertook this work, but this leads away from the field of X-ray diffraction.

During my stay at Northwestern von Laue came for a visit to the U.S. (1948). It was wonderful to see him stepping out of the train, in Chicago, somewhat aged but with his radiant expression of idealism unbroken, in spite of all he had gone through. He had exposed himself by protecting many who had been persecuted under the Nazi régime, he had also opposed submitting to its external ritual: entering the university he used to carry one heavy book in either hand, so that he could not give the Nazi salute.

He came shortly after the war from the country where the war devastation of institutions and laboratories was greatest to that where they had suffered least. The seeming opulence of America hurt him. When we took him to a Woolworth shop he noticed toy balloons being blown up with helium gas, to be handed over to children. Laue said with a sigh: 'if we had only two of these helium cylinders in our laboratories, what could we not do!' But this was not his lasting impression. He was given full opportunity to state the plight of German physics, which he was so well qualified to present. It was given to him to see the resurgence of scientific activity in his country.

On reaching retiring age the university gave me the use of a laboratory. I then reverted to X-rays and the spinning powder layer which I had introduced a long time ago as a means of increasing the number of crystalline domains taking part in diffraction for reducing the background of short-wave radiation.

A teaching position tends in a way to develop a split personality, the extrovert teacher, versus the introvert researcher. To be relieved from this duality may seem a certain simplification, but a university position is an ivory tower and when this is abandoned soon other tasks appear; in my case this is the care of the manuscripts of my father, so that there are two sides to the change.

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