

## *Personal Reminiscences*

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When I arrived in Göttingen in 1913 as a student of Physics, Göttingen was still a small quiet University town, full of charm and without factories; life was centered in the University, and under the still surface lay hidden an intellectual life of unsurpassed intensity. The circle of mathematicians surrounding Felix Klein,—David Hilbert, Carathéodory, Landau, Runge, Toeplitz, Herglotz, Hecke, Weil, Courant and Noether—as well as the particularly richly staffed Physics with Riecke, Voigt, Wiechert, Debye, Born, von Traubenberg, Madelung, Simon, Prandtl, von Kármán, and Tammann, rendered the Faculty very attractive to mathematicians and physicists. This was the time when at last the young Quantum Theory of Light began to be taken seriously although it could by no effort be straightened out with wave theory. One tried hard to become convinced of the reality of Bohr's electron orbits in the atoms in spite of all the hesitations the physicist felt in accepting the hypothesis that the electron on its stationary orbit about the atomic nucleus does not radiate,—a flagrant contradiction to Maxwell's theory. The next job to be done was therefore to find a check on Bohr's hypothesis which worked so simply and directly in the case of spectral emission, by looking for a direct evidence of the reality of the electronic orbits. Debye had come to the conclusion that specific diffraction effects should be produced with X-rays by the regular spacing of electrons on circular orbits: Using monochromatic X-rays, one should find the curve of diffracted intensity vs. angle of diffraction to show broad maxima and minima, provided the electrons kept at fixed distances from each other in the course of their motion. Debye proposed to me that we try together such diffraction experiments. We used at first a gas-filled medical X-ray tube with platinum target which happened to be available in the collection of the institute. For power source we used an enormous induction coil with mercury interrupter and a gas-filled rectifying

valve. The whole set-up appears nowadays like a show piece taken from a museum. The first diffraction photographs, with paper and charcoal as the scattering substances, showed no diffraction effects. The reason for this may have been that the thick glass wall of the tube absorbed the Pt L-radiation and transmitted only the continuous background. The film was relatively insensitive for the K-radiation, which, besides, was not strongly excited, so that possible maxima were covered up by the continuous background.

This prompted me to construct a metal X-ray tube, water-cooled and with copper target. The tube remained connected to the rotating Gaede mercury pump. An aluminium window, 1/20 mm thick, permitted the rays to emerge. I also constructed a cylindrical diffraction camera, of 57 mm diameter, with a centering head for the sample, of the type which is being used still nowadays.

For the sample I used the finest grain powder of lithium fluoride; Debye and I were most surprised to find on the very first photographs the sharp lines of a powder diagram, and it took us not long to interpret them correctly as crystalline diffraction on the randomly oriented microcrystals of the powder. The diffraction lines were much too sharp than that they could have been due to the few scattering electrons in each single atom. That in lithium fluoride we picked a cubic crystal powder with exceptionally favourable scattering properties was a piece of good luck.

Modifying the application of Debye's original idea we also obtained diffraction photographs with fine jets of benzene and cyclohexane. The perfectly clear broad interference rings produced by these were interpreted as the diffraction diagrams of the carbon hexagons in these molecules. In later years, and with utmost success, Debye has refined and followed up this first X-ray study of the structure of liquids.

The possibility of determining structures without the need for macroscopic crystals was obviously of great value, since for many substances single crystals were not available, or one wanted to investigate agglomerations of crystals.

There was a forbidding multitude of substances that were of interest. While the determination of cubic structures was easy (MgO, Fe, etc.), I did not succeed to find, for instance, the lattice of boron. Professor Stock of Berlin had supplied me with pure boron powder, and I devoted much effort on this substance. Even the method of indexing which Professor Runge proposed proved unsuccessful in application to the boron diagrams although using sufficiently accurate data one should arrive by it nearly automatically at the correct lattice. On the

other hand it was not difficult to determine the structure of a complex-salt like  $K_2PtCl_6$ . The arrangement of the atoms followed exactly the scheme which Professor Werner in Zürich had surmised from chemical reasoning: the small Pt ion at the centre of a octahedron formed by the six Cl ions.

In conjunction with Debye the structure of graphite was investigated, since this substance was not available in large crystals. Only later did we learn that graphite had also been studied by the Braggs. We prepared the samples in a variety of ways and found in all cases the same lattice independently of the crystal habit. The famous six-sided graphite flakes of Groth's mineralogical collection in Munich were kindly put at our disposal and, to our immense amazement, they turned out to be molybdenum trioxide crystals!

The powder method was also invented independently about a year after the beginning of our work by A. W. Hull in Schenectady, and was used by him mainly for the structure determination of metals; during the war the communication with U.S.A. was interrupted.

In spite of the many crystallographic applications which the method suggested, it seemed more suitable to Debye and myself to take up problems of physical interest in preference to those which fell naturally to crystallographers.

It was the atomic scattering factor that interested us most. Evidently the diffraction diagram of a crystal could at the best yield the scattering amplitude of an atom only for the discrete angular values of a crystal diagram, and not the continuous scattering curve down to quite small angles. But even so, by extrapolating the experimental intensities to the scattering angle zero, it should be possible to find the actual number of the scattering electrons in the atom. The evaluation of the LiF diagrams demonstrated with gratifying certainty that both kinds of atoms were present in the crystal as ions: the extrapolation to  $\theta = 0$  gave scattering amplitudes which stood exactly in the ration 2 : 10. Lithium had only 2, fluorine however 10 electrons, corresponding to ionized atoms. At the time this was an interesting and by no means obvious result, and I remember well Professor Voigt's enthusiasm when he heard of it.

Another problem of great interest since the time when Planck first quantized the oscillator was that of the existence of the zero point energy  $\frac{1}{2}h\nu_0$  of an oscillator. The corresponding motion of the atoms in a crystal, which should persist even at the absolute zero of temperature, ought to show up on an X-ray diagram. Debye, and Waller, had calculated the effect of atomic motion on the diffracted intensity.

Diamond with its high Debye temperature seemed a particularly well suited crystal for demonstrating zero-point motion. Accurate intensity measurements were made and gave an unqualified confirmation of the existence of zero-point energy of the oscillating lattice atoms.

My thesis for becoming a lecturer (*Habilitationschrift*) dealt with the determination of the structure and size of colloidal particles by means of X-rays. Up to then colloids could be investigated only with the ultramicroscope, and of course no information could be gained on the structure of the particles. I started with colloidal gold and silver because of these substances samples with a definite particle size could easily be prepared by Zsigmondy's nucleation method. I derived a formula for the broadening of powder diffraction lines with decreasing particle size and succeeded not only in measuring the size of minute particles of only 20 Ångström linear dimension, but also proved that particles of such a small number of atoms already crystallize in the normal structure.—Organic colloids too gave interesting diagrams. Using the fibres of ramie I observed for the first time the interesting fact of fibrous structure, namely the regular arrangement of cellulose crystallites along the direction of the fibre axis. Unfortunately, this paper became very little known owing to its delayed publication in Zsigmondy's book on Colloids.

Later on, in Zürich, I continued working on X-ray diffraction for a while before getting involved in other lines of work, mainly on ferro-electrics and on nuclear physics. This work dealt in part with the determination of the atomic scattering factor. The interest in such measurements was revived once that wave mechanics and the Fermi statistical model of the atom led to more precise predictions regarding the scattering curve of the atom. The electron density distributions were thus determined for metallic lithium, copper and gold. Of even greater interest, however, were the scattering measurements on mono-atomic gases, for instance a mercury gas jet, by which the monotonic character of the scattering curve, i.e. the absence of maxima and minima, was confirmed in agreement with wave mechanics.

Other work, with Wollan and Kappeler, was aimed at establishing the details of the intensity distribution within the Compton line in the scattering by the inert gases. Because the electrons are not at rest within the atom, the Compton line is broadened by a kind of Doppler effect, and it becomes possible to correlate the momentum distribution of the electrons in the atom to the details of the shape of the Compton line. Professor Sommerfeld kindly had the theory for the discussion of our experiments worked out.

Together with Dr. Dubs I carried out comparative scattering experiments on water at rest, in laminar flow and in turbulent motion, the latter at high Reynolds numbers, looking for small differences in the scattering diagrams.

As a matter of curiosity I would like to mention that, while still in Göttingen, I gained the impression that perhaps electrons in passing through a crystal might be diffracted and show interference like waves. I borrowed from Professor Lise Meitner a strong beta ray source and used it for transmitting a beta ray through rocksalt—without, however, finding an unambiguous result. This experiment was prompted by one of those silly ideas which, according to Professor Runge, one should always follow up, without, however, feeling disappointed by lack of success.