Personal Reminiscences

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On 10 January 1925 I graduated from the University of Latvia (in Riga) with the degree of chemical engineer. The University was the successor to the Polytechnic Institute of Riga, and its chemistry department had a very good reputation in Europe. To mention some of its former members: Wilhelm Ostwald (a native of the country) started his splendid career (after studies and assistant years in Tartu, Estonia) at the department and went then to Leipzig, Germany; his successor was the organic chemist, C. A. Bischoff; for a number of years Professor P. Walden, a student of Ostwald, taught various courses in chemistry and became famous though his 'Walden inversion' and the Ostwald-Walden rule; in chemical technology C. Blacher and Glasenapp were well known names.

My major professor and advisor was Dr. M. Centnerszwer (of Poland), a student of Ostwald and Walden. He was well regarded because of his work on phosphorous, hydrocyanic acid, critical phenomena and kinetics of dissolution of metals in acids. I worked in Professor Centnerszwer's laboratory for one year on a metal dissolution problem, for which I received an award from the University and for half a year on my thesis on hydrogen overpotential on various metals. Out of this work three publications in the *Z. f. physik. Chemie* resulted. After graduation I decided to continue the same work and prepare a thesis for a Dr's degree in chemistry. In Latvia, as in Russia and some other European countries, it is not at all necessary to enroll as a student in order to work for a Dr's degree. Thus, I could continue my experimental studies at the chemistry department under Professor Centnerszwer, receive a small fellowship and work the whole day as no courses were required. The laboratory was well equipped (we even had a glass blower and a good mechanical shop at the physics department), and this made working a pleasure and I was happy. At that time I developed an approach to the study of metal corrosion

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problems by exploring the behavior of simple single cells, e.g. Zn-acid-Pt. This method which earned us the name of the Baltic School (Centnerszwer-Straumanis) in Germany and Russia, was further developed by G. W. Akimow in Moscow and is widely used even now for solving various corrosion problems. After the acceptance of the thesis, comprehensive examinations followed before the department, which consisted of 40 professors and teachers, and then I had to defend the thesis before the professors and the public.

During my study I came to the conclusion that the rates of corrosion and dissolution of metals in various corrosive agents depend largely upon the concentration of impurities and imperfections in the metal and its structure in the case of alloys. Therefore I wanted to acquire more laboratory experience regarding the structure of metals. The Rockefeller Foundation gave me this opportunity by granting a generous stipend for physical metallurgy studies in Göttingen (Germany) in the institute of the famous physical chemist and metallurgist, G. Tammann. Before going to Germany I married Eva Reinhards, who was and still is a great help to me and who firmly stood at my side through all the hard years that followed. We went to Göttingen together in the spring and into a fine country which had just recovered from the grave consequences of World War I.

The research work in the institute of Professor Tammann was mostly qualitative at that time (1927/28). However, this work was of great importance for the German metal industry, as it showed clearly how to solve industrial problems on the basis of scientific research. In this respect I learned much from professor Tammann. The other aspects were less satisfactory, since he liked old methods of research and there was no X-ray equipment in the institute, although professor Tammann in discussions liked to refer to results obtained by X-ray methods. There was a large group in the institute: about 30 from many countries, including U.S.A. and USSR, working for their Dr's degree. There I first met professor Curtis L. Wilson, the present dean of the School of Mines and Metallurgy in Rolla, Missouri. I made many friends there. Upon my return to Latvia, I continued my research, was elected assistant professor, did some lecturing (complex compounds) and teaching in the preparative laboratory. For my corrosion studies I needed single metal crystals and, therefore, started to grow them from the melt and also from their vapour. Several publications on metal crystal growth resulted, which were very favourably accepted. The meshlike structure of impure single zinc crystals fascinated me[1] and so I decided to go once more to Germany to study such structures by X-ray methods.

This time I went to the K.W.I. für Metallforschung, then (1931) located in Berlin, for 5 months with the support of the U. of Latvia and the Cultural Fund. There I encountered a fine scientific atmosphere: Professor G. Sachs had just left the Institute, but there were such famous scientists as E. Schmid (now Vienna), G. Wassermann (Clausthal), W. Boas (Melbourne), J. Weerts (died), and P. Beck (Urbana). Dr. M. Valouch (Praha) was also working there. I started to make my first precision lattice constant determinations (back-reflection method Sachs-Weerts), and realized the significance of such measurements and the disadvantages of the present methods.

Upon my return to Latvia I immediately got busy establishing an X-ray laboratory at the department of chemistry. The necessary funds were obtained from the Rockefeller Foundation and the University and an X-ray machine and equipment was ordered from Dr. H. Seemann (Freiburg i. Br., Germany). In 1932 the new X-ray machine was at work. The next year I did some studies in Professor E. Zintl's and in Dr. Seemann's laboratories (Freiburg i. Br.) and there I also had occasion to meet Professor M. von Laue (Berlin). Back in Riga the question of lattice parameters was put on the research program. I started the work with Dr. O. Mellis (now Professor of Mineralogy at the Univ. of Stockholm), by measuring the lattice constants of silver, copper and of sodium chloride. To eliminate the line shift due to the absorption of X-rays by the samples, they were made as thin as possible by sticking the powder to a thin fiber of Lindemann glass, much in the same manner as we are doing now. I further proposed, for exclusion of the uncertainty in line measurement due to film shrinkage after development, to make two powder patterns of each sample: one regular Debye pattern and one back reflection according to van Arkel. From a combination of the films the effective circumference of the film cylinder could be calculated [2] and from it the number of degrees per mm. This method worked well but not as well as I expected: one reason for the fluctuations of the calculated constants was the inaccurate knowledge of the sample temperature during the exposure and the second was the unequal shrinkage of the separate films. Although I considered the possibility of replacing both films by one but in an intermediate position, I did not grasp the full advantage of this possibility at that time.

Meanwhile, on the death of professor W. Fischer (1934) I was nominated his successor in analytical chemistry, and director of the analytical and X-ray laboratories. In this position I had more time for research than ever before. Students came to work for their engineer's thesis and there were some candidates for Dr's degrees. Among the latter was my former classmate at the University, Alfreds Ieviņš, a very able man (later Professor of Inorganic Chemistry at the U. of Latvia and now at the reestablished Polytechnic Inst. of Riga). We decided to attack the precision determination of lattice parameters again, with the intention of using the results for Ieviņš doctoral thesis. As the work proceeded, the errors endangering precision determinations were gradually removed, new precise cameras were designed (mostly by myself), thermostats for the cameras were built and comparators for film measurement were constructed. Ieviņš proposed again to use one single film in an intermediate position instead of two and this time I realized at once all the advantages connected with this film placement. Since the patterns, powder as well as single crystal, taken on such films had an asymmetric appearance, the method was called 'asymmetric' in the book of Halla and Mark.[3] We decided to keep this name as an appropriate one for our method of precision determination, which, however, consisted not only in the use of films in the asymmetric position, but also in using very thin samples, nearly free of absorption in the high back reflection region, high precision cylindrical cameras, kept at constant temperatures before and during the exposure, and high precision comparators for film measurement. It turned out that large cameras have disadvantages concerning operation and no advantages concerning precision, as compared with smaller ones, 57.4 to 64 mm in diameter. The experience gained in precision determination of lattice parameters was later described in a booklet, published by Springer in Berlin in 1940. Now more than 25 years have elapsed since the first publications of the asymmetric method [4] and I must say that a much better agreement in lattice-constants determination between various laboratories would have resulted (see report of Dr. Parrish[5]) if my colleagues and the camera manufacturers had followed the directions outlined in this booklet.

However, the lattice parameters were not the goal, but only a step toward it. As previously mentioned I was, and still am interested in the basic reactions of attack of metals by corrosive agents. How is the rate of attack influenced by impurities and their distribution in metals, and what is thereby the role of imperfections and point defects in metallic crystals? (At that time very little was known about dislocations.) In this respect a simple equation attracted me very much:

$$M_x = v\, d\, N/n,$$

where v is the volume of the unit cell, d is the density of the material, n is the number of molecules per cell, N is Avogadro's number, and where M_x may be regarded as the X-ray molecular weight of a compound or element. If there is a difference between M_x and M_y, the molecular weight as determined from chemical analysis, it could be attributed to the imperfect structure of the respective crystalline material. However, in order to use the equation, a precise knowledge not only of v, which could be determined to a high degree of precision by the asymmetric method, but also of N and d was necessary (n is supposed to be an integer, the magnitude of which is easily found). So, I started to determine the most probable value of N and began to think about precision density determinations.[6] However, it took more than 10 years to advance to this point. The reason was that I had to leave Latvia. This was very hard for me, because my research laboratories were very well equipped, there was money for research and I had a staff of well trained and reliable chemists.

The Russian Communists occupied Latvia already in June 1940. All was changed in line with the Moscow pattern. Although the best conditions of life were those of the University staff, the changes were, nevertheless, very extensive: many professors were dismissed, the Divinity department was closed, the administration of the University completely altered. Personally, I could not complain. Due to my scientific activity I was chosen to be professor (head) of inorganic chemistry and I had a staff of 14 assistants who were to be trained as future chemists in this field. I was sent to visit Moscow by the rector of the University and for a second time (in January 1941) at the invitation of Professor A. Frumkin of the Academy of Sciences. I delivered two lectures (in Russian) on lattice constant determination and on corrosion of superpure aluminium, both with results new to Russian scientists. Nevertheless at home, because of all the numerous changes and the general intimidation of the population, the research activity was at low ebb.

In July 1941 the country was taken over by the German armies. Under the German regime the old order was restored only to a limited extent, much to the great disappointment of the population. However, the University could continue its teaching and research activity nearly in the old manner with only a few German supervisors. As time progressed, new difficulties arose (e.g. food shortage) and it became more and more clear that the Russians would return. So, one day in 1944 we had to decide whether or not to stay in Latvia. Why did we leave our native country? There were three main reasons:

1) The German administration ordered the professors and teachers of the University to leave for Danzig (Germany) and gather at the Institute of Technology.

2) The population of Latvia was so intimidated in only one year by the ruthlessness of the Communist regime that it started in masses to leave Latvia.

3) We had four daughters and one son.

Thus, we left all our possessions, including two houses, and began our troublesome exit to Danzig in the fall of 1944. I said good-bye to my laboratories in which in 10 years 100 publications were produced, not only on lattice constant determination but also on dissolution and corrosion of metals, on description of new complex compounds (with Dr. A. Cirulis), on structure of eutectics (with Dr. N. Brakšs) and on various questions of colloid chemistry (Dr. B. Jirgensons, now at the University of Texas, Houston), and analytical chemistry (Dr. E. Eegrive).

Professor W. Klemm of the Danzig Institute of Technology was kind enough to take care of us and in a short time, I received several offers for employment. I chose a position as far as possible to the West, the Institute for Metal Chemistry at the University of Marburg, directed by the 'Geheimrat' Professor R. Schenck. On his suggestion I started research on the sodium tungsten bronzes. The Institute was well equipped, also for X-ray work, but no high-precision measurements could be made there. For my family and myself I received a couple of small rooms in a village at a distance of one hour by train from Marburg. The train was nearly never on time and more and more difficulties piled up toward the close of the war; finally the Institute was damaged by bombs. After the German surrender the Institute was closed by the Allies, but after some months I received permission from the local American Government to continue my research. It was very difficult to work, although Dr. A. Dravnieks (now at Armour Research Foundation, Chicago) helped me. In addition, the political development became very unfavourable for refugees, as it turned out clearer each day that there was not the dimmest hope for the restitution of an independent Latvia. Thus, like hundreds of thousands of other refugees we could not return to our native country, to the great surprise of many American and English authorities. There was only one way out: emigration to the U.S.A. I began studying English, of which I had very little knowledge, and wrote letters to professors of American Universities. My wife was a great help to me in my English studies. Among the offers received, the best one came from Dean

Curtis L. Wilson of the Missouri School of Mines and Metallurgy at Rolla. He still remembered me from the studies with Professor Tammann in Göttingen and offered me a position as Research Professor of Metallurgy with very little class work.

In December 1947 we arrived in Rolla and, with the invaluable help of the Dean and other members of the Faculty, we soon settled in and began slowly to enjoy the advantages of life here: we acquired a house and a few years later a second hand Steinway grand piano, a very fine instrument. Although I have a bad musical memory, I, nevertheless, like to play the piano. The reason may be the deep content and the brilliance of many musical pieces, the beautiful tone of good pianos and a sense of satisfaction felt after a piece has been mastered. Also, while practicing, I think about my research and laboratory problems. My children had no difficulty in high school and they all graduated with good grades and attended college. Of course, there were some things which were less to my liking: many of the graduate students were not too well prepared for research, some articles for publication had to be tailored according to the wishes of the referees and I was astonished by the amount of teaching required from most professors. However, I am pleased to notice that this is now gradually improving.

Finally precision lattice parameter determinations could be made. I compared some of the values obtained with those determined previously in Riga (e.g. Al, Ag) and they agreed within the limits of error. Hence, I could go farther and begin with precision density determinations after an interruption of more than ten years. I also realized that it is more advantageous to determine n, the actual number of molecules per unit cell (see equation mentioned above), instead of d or M_x for the determination of the perfection of the lattice of a crystalline substance.[7] Good results were obtained with zone refined Al, with dislocation free Si (which showed a perfect lattice) and with oxide phases TiO, Ti_2O_3 and TiO_2 which showed the presence of constitutional vacancies and interstitials. In this work I enjoyed the effective support of Dr. W. J. James, Professor of Chemistry at the School. However, difficulties arose when, against all expectations, the lattice constant and density method indicated the presence of interstitial atoms for deformed Al (hard wire). These results have been reported by me in Berlin, Vienna, Bern, Neuhausen (Switzerland) and Rome, while a Fulbright Professor in Austria at the invitation of Professor H. Hohn. Aluminium is still under examination here, but I hope now that in other places, where dislocations are being investigated, more attention will be paid to the very important experimental precision density determinations. They are difficult and this is the main reason why only few precision determinations are to be found in the literature.

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