Autobiographical Data and Personal Reminiscences

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I was born in Moscow in 1887 into the family of a bookkeeper and obtained my secondary education in the Commercial School, graduating in 1906. My favourite subjects in school were geometry, physics, and chemistry. My interest in crystallography was aroused early during my years at high school after I attended a popular course in crystallography at the Polytechnical Museum, given by G. V. Wulf for a large audience. After having passed an additional examination in Latin, I entered the Moscow University, Department of Natural Sciences of the Physico-Mathematical Faculty, determined to specialize in crystallography. This particular subject attracted me, and still does so because it combines my three favourite subjects in great harmony and because of its deep underlying philosophy. Wulf started his research work in 1909. He already knew me as one of his rather outspoken students. My career was started with these two phrases: 'Iurii Viktorovich, I should like to work for you...' '...very well, set up the thermostat...,' and I started to work. I still remember the great satisfaction with which I turned up my sleeves in order to carry out my first task. My first paper 'On the Symmetry of Potassium Bichromate Crystals' was published in 1911 in German. In the same year, Wulf and many other professors left the State University and transferred, as a whole group, to the Free Peoples' University organized by private means. In the fall of 1912 the news of Laue's discovery reached Moscow. At that time I was enlisted into military service in an infantry regiment stationed in Moscow. Every now and then I could visit Wulf's laboratory in the evenings or do book-work at home. At that time I was particularly interested in the works of Fedorov and Schoenflies concerning space groups; these works had already had a twenty years history. In those days not one of the crystallographers doubted the lattice structure of crystals, that is, the validity of the law of rationality of parameters which was verified experimentally. Even

Laue himself could not doubt this (at that time he worked in Munich together with the leading German crystallographer P. von Groth). Therefore, Wulf and his associates saw in Laue's discovery primarily the triumph of theoretical crystallography and further the possibility of actual determination of crystal structures. Soon afterwards Wulf acquainted his associates with the formula $\frac{\lambda}{2} = \frac{\Delta\varepsilon}{m}$ derived by him, which in essence was identical with Bragg's well known formula. The unfortunate circumstance that Wulf was formally denied the priority for this discovery left me with a very deep impression. The following year, 1913, Wulf spent organizing experimental work of X-ray diffraction of crystals in his poorly equipped laboratory and I spent it in military service. From my evening visits to the laboratory I clearly remember the figure of the equipment agent shuffling his feet, I still remember his morning coat, his turned up moustache and his sleek blonde head which seemed to have been forced through the opening of the tight, high collar. With this agent Wulf led endless discussions about equipping the laboratory with the most necessary X-ray diffraction instruments from Germany. K. V. Vasiliev helped Wulf to install these instruments. My military service was over by the end of 1913 and for half a year I assisted Wulf during his lectures. It was my duty to prepare all possible demonstrations and experiments which usually accompanied these lectures. Sometimes I took the liberty of interrupting Wulf's speech and personally explaining the phenomena I demonstrated. Wulf jokingly called this 'lectures in duet'. In these lectures we used experiments and models for explaining the phenomena of the diffraction of X-rays when they pass through a crystal. Wulf designed the following model for explaining the distribution law of the spots on the Laue pattern. Several glass slides were glued together along the edges in such a manner that a system of planes belonging to a zone was obtained. The model could be rotated around the axis of this zone. When a narrow light beam was directed on to this model at a certain angle to the axis, the light spots on the screen described an ellipse when the model was rotated.—In the summer of 1914 I was again called into military service and sent to the front near Warsaw in August. After having been seriously wounded I was assigned to noncombat duty at a war plant. There during my free time I was able to work a little on geometrical crystallography. One of my papers from this period, published in the *Izvestiia Akademii Nauk* in 1916, was brought to the attention of Fedorov. Unfortunately, my cor-

respondence with Fedorov concerning this subject, together with extensive unpublished material, was lost. Part of this work was later published (in 1922) in the paper 'The Basic Law of Crystal Chemistry' which discussed the relationship which I discovered between the chemical formula of a substance and its crystal structure. In 1918, after having been demobilized, I returned to my work as Wulf's assistant at the Peoples' University in Moscow, but during those difficult days we could not begin any serious research work. In 1920 I went to Sverdlovsk to work in the Mining Institute which was being established there at that time. My assignment at that higher School was to lecture on crystallography. There, in Sverdlovsk, the center of the stone cutting industry of the Urals, I was personally in charge of the preparation of the thin plates of natural crystals for the courses in crystal optics. I developed an interest in the problems of cutting, grinding, and polishing of crystals and stone. This served as the starting point for many of my papers on quartz, later published in my book *Quartz and its Applications*. In order to teach crystallography in the spirit of my teacher Wulf, I had to create, with my own hands, new models and demonstration devices, repair polarization microscopes, prepare quartz wedges, and sometimes even simple glass lenses to replace those that were used as burning glasses (there was a shortage of matches in the smaller towns). A turning lathe was urgently needed for work of this kind. I was able to find a person who owned a small lathe and was willing to sell it for a million rubles. While I was attempting to obtain this money at the Mining Institute the price of the lathe was increased to two million rubles. I needed the lathe very badly in the laboratory and paid the second million out of my own pocket. My affection for the lathe was not looked upon with approval by my 'geological' superiors who considered crystallography as a science which could be taught with a piece of chalk and wooden models and nothing else. As I had to prepare the crystal optical specimens myself, I worked out the sorting of emery powders according to grain size and became interested in the moiré patterns which appeared when two sieves were superimposed on each other. Comparing the sieves with crystal lattices I determined some rules pertaining to symmetry and to the interference of waves. The outcome of this work were two papers concerning new phenomena of moiré optics. I remained in Sverdlovsk for five years. During this time I succeeded in introducing the teaching of crystallography and wrote a textbook of crystallography, published in a limited edition, for the students. Because it was impossible to order X-ray equipment from abroad, organized research of crystal

structures did not materialize in Sverdlovsk. At the invitation of Academician A. E. Fersman I went to Leningrad in May 1925 and accepted a position in the Mineralogical Museum of the Academy of Sciences (USSR). From that time on my work has been in the Academy of Sciences (USSR). At the Museum in Leningrad I had a room of which the floor was destroyed by the flood of 1924. After the floor was repaired and I started to organize crystallographic research, I again had to live through the trials and tribulations of acquiring a lathe, but in a slightly different version. The finances of our government had been put in order, millions of rubles had been converted into ordinary rubles, but to obtain them for the purchase of a lathe was impossible for the same old reason. The generally accepted opinion was that any developments in the realm of crystallography required only a brain but no hands or physical devices, and to use a lathe or milling machine was unheard of. Fedorov was cited as an example and argument; the majority of his crystallographic work was produced by the 'armchair method' alone and without any 'experimental stage settings'. Unexpectedly, the problem of the lathe found a very simple solution. Without much ado Fersman gave me some money from his personal funds and so helped me to purchase a second-hand lathe which must have dated back to the time of Frederick the Great at least. I still remember my great satisfaction and pleasure when, after having undertaken the repairs myself, the lathe was ready for work. The example set by Fersman found followers within the walls of the Mineralogical Museum. In addition to the immediately necessary items, many rather valuable objects were donated by various associates. Later, we gave the Administrative Department of the Academy of Sciences (USSR) many a headache when the objects acquired in this manner had to be listed in the inventory. I am frequently reminded of those wonderful bygone days by my own bookcase in my office at the Institute of Crystallography which still carries the inventory number affixed to it 35 years ago.

In December 1925 Wulf passed away. With his passing, the chair of crystallography at the Moscow State University became vacant; at the same time the chair became vacant also at the Leningrad University. I was elected to both chairs but accepted neither for various reasons, the main one being that I did not wish to relinquish or reduce my work at the Academy of Sciences (USSR). As a result, the chair of crystallography was discontinued in Moscow, and the one in Leningrad was headed by Prof. O. M. Ansheles.

In 1927 I was sent to Germany and Norway for the purpose of

becoming acquainted with the X-ray diffraction analysis work in these countries. This trip coincided with the 'Week of Russian Science' in Germany which made it possible for me to meet Einstein, Planck, Laue, and other leading figures of German Science. At that time I asked Laue to show me his laboratory where he investigated crystal structures; to my great amazement Laue told me that he did not have a laboratory at all. In Munich I had the opportunity to meet Groth (several months before his death), to work for about four weeks on X-ray diffraction of crystals with Rinne in Leipzig, and to contact Schiebold. In Norway I also had the opportunity to acquaint myself with Goldschmidt's X-ray diffraction apparatus and to meet the then little known Zachariasen. On my return trip from Oslo to Hamburg I spent a day in Copenhagen where I visited Bohr's laboratory where a certain amount of optical research went on in the basement.

On my return I started an active campaign for X-ray structural analysis in my own laboratory. As a result I was again sent to Germany in 1929 for the specific purpose of ordering an X-ray diffraction apparatus for studying crystals. After about a year, the huge apparatus consisting of a 10 kilowatt transformer, six enormous condensers, instrument panel, large X-ray tubes, rectifiers, and so on, finally arrived. The apparatus was set up by my fellow laboratory workers under the supervision of a specialist sent from Germany. Soon a young physicist, a recent graduate of the Leningrad University, B. K. Brunovskii, was found who was willing to work with this device and independently specialize in the field of X-ray structural analysis. Thus, after a 15 year period caused by the war and the death of Wulf, research of crystal structures again came to life. But only to be cast aside again because of World War II ten years later. In 1934 the government decided to transfer the Academy of Sciences (USSR) from Leningrad to Moscow. This undoubtedly wise decision had, however, a rather undesirable effect on the progress of the research on crystal structures. The X-ray device had to be dismantled and reassembled in Moscow. Brunovskii's paper 'X-ray Analysis of Catapleiite', the first paper of its kind in the Soviet Union, was published in 1935. In this paper Brunovskii acknowledged the assistance of J. Bernal who, when visiting the Soviet Union, always made a point of visiting our laboratory which at that time, as a smaller research group, was a part of the Lomonosov Institute of Geochemistry, Mineralogy, and Crystallography founded by Fersman. Towards the end of 1937 the laboratory was separated from the Institute and became the independent 'Laboratory of Crystallography of the Academy of Sciences

USSR.' This created the most advantageous conditions for the future development of the research on crystal structures. The work started by Brunovskii was unfortunately terminated by his untimely death. I then asked N. V. Belov to take over and direct the X-ray structural analysis of crystals with the existing device. Belov was then already known in our country as a great expert of investigated structures and, in particular, as the translator into Russian of the book *Kristallchemie*, 1934, by O. Hassel; during its translation he added a great number of his own remarks and 27 figures. Belov did not consent right away, his refusal being motivated by the fact that he had never been concerned with or really interested in the experimental side of X-ray structural analysis. After a certain time our negotiations were resumed; the outcome was that Belov consented to start the work but on the condition that for two years I should not intervene while he was 'learning' X-ray structural analysis. Some time later Z. G. Pinsker joined the Laboratory of Crystallography. Pinsker, having been associated with another institution of the Academy, had already designed his electron diffraction device. Thus, our laboratory had two very promising divisions of experimental research on crystal structures. The war which started in 1941 did not enhance the continuation of this work. When Hitler's armies approached Moscow there were only 15 people in the laboratory. The majority had been evacutaed to the Urals where they were engaged in defense work. Although in charge of the latter, I still found some time to work on some abstract ideas (antisymmetry, piezoelectrical textures). Some of the X-ray structural analysis work, started under Belov's supervision, managed to keep alive. And at the height of the war L. M. Beliaev defended his candidate's dissertation on the structure of the mineral Ramsayite from Chybiny.* In 1943 the staff of our Crystallography Laboratory, inspired by the victories of our army, returned to Moscow confident that the war would end with the complete defeat of fascist Germany.

In 1944 the laboratory had become so well staffed that the question of reorganizing it into the 'Institute of Crystallography of the Academy of Sciences USSR' with all the rights and responsibilities resulting from such a move was contemplated. It was decided to organize this institute within the Department of Physico-Mathematical Sciences of the Academy of Sciences and not in the Department of Geological-Mineralogical Sciences under which the laboratory had functioned during the war. The Institute of Crystallography began its work in

* Chybiny—Mountainous region near Murmansk (Kola Peninsula, USSR) where Apatite and rare minerals are found (translator).

1945. After the end of the war, research on the structure of crystals started to develop rapidly in our and other Soviet Institutes and has continued to do so right up to the present time. In addition to the problems of theoretical and experimental crystallography, I was also interested in the technical application of single crystals: quartz, rubies, Rochelle salt, and others. The result of my interest in crystallization was the publication of my book *How Crystals Grow* in 1935. My books *Quartz and its Applications* and *Symmetry* were published in 1940, *Piezoelectrical Textures* in 1946. In the following year, 1947, the pamphlet *Crystal Growth* appeared. *Optical Crystallography* was published in 1950 and revised in 1958 under the title *Fundamentals of Optical Crystallography*. The book *Symmetry and Antisymmetry of Finite Figures* was published in 1951. *The Investigation of Piezoelectrical Textures* (written in collaboration with my associates) appeared in 1955. Altogether, including papers which appeared in journals, I have published over 250 items concerning different branches of crystallography. The Institute of Crystallography which I helped found now boasts of a large number of people working under its roof. A third large building for the Institute has recently been completed. The Institute has shops equipped with all kinds of lathes. The journal *Kristallografiia* has been published since 1956 under my editorship. This journal is translated into English and is also available in the United States.